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MEDICAL SCIENCES.

EDITED BY

ISAAC HAYS, M.D.,

FELLOW OF THE PHILADELPHIA COLLEGE OF PHYSICIANS; MEMBER OF THE AMERICAN MEDICAL ASSOCIATION; OF THE AMERICAN PHILOSOPHICAL SOCIETY; OF THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA; ASSOCIATE FELLOW OF THE AMERICAN ACADEMY OF ARTS AND SCIENCES, &c. &c. &c.

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TO READERS AND CORRESPONDENTS.

The following works have been received:—

On Epilepsy and Epileptiform Seizures, their Causes, Pathology, and Treatment. By Edward Henry Sieveking, M. D., F. R. C. P., &c. &c. London. John Churchill, 1858. (From the Author.)

On Vesico-Vaginal Fistula, and its successful Treatment. Illustrated by eleven cases. Read before the British Medical Association, at Edinburgh, July 31, 1858. By I. Baker Brown, F. F. C. S., &c. &c. &c. London, 1858. (From the Author.)

The Microscope in its Application to Practical Medicine. By Lionel Beale, M. B., F. R. S., &c. Second edition, with 270 wood-cuts and a coloured plate. London, John Churchill, 1858. (From the Author.)

Syllabus of the Course of Lectures on Medical Logic, delivered in Mareschal College and University, Aberdeen. By Francis Ogston, M. D., Prof. Med. Logic and Med. Jurisp. Edinburgh, 1858. (From the Author.)

Etudes sur la Monorchidie et la cryptorchidie chez l'Homme. Par M. Ernest Godard, Interne des Hôpitaux de Paris, &c. Extrait des Mémoires de la Société de Biologie. Paris, 1857. (From the Author.)

On Amputation by a Long and a Short Rectangular Flap. By Thomas P. Teale, F. L. S., &c., Surgeon to the Leeds General Infirmary. Illustrated by engravings on wood, by Mr. Bagg. London, John Churchill, 1858. (From the Author.)

A Treatise on the Human Skeleton (including the joints). By George Murray Humphrey, Esq., M. B. Cantab. F. R. C. S., Surgeon to Addenbrooke's Hospital, Lecturer on Surgery and Anatomy in the Cambridge University Medical School. Cambridge, 1858. (From the Author.)

Selections from the Records of the Government of Bengal. Published by authority. No. xxviii. Reports on the Asylums for European and Native Insane Patients at Bhowanipore and Dallunda for 1856 and 1857. Calcutta, 1858.

The Pathology and Treatment of Stricture of the Urethra, and Urinary Fistulæ. By Henry Thompson, F. R. C. S., M. B. Lond., &c. &c. Second edition, much enlarged and revised. London, 1858. (From the Author.)

Sur une Fonction peu connue du Pancréas la Digestion des Aliments Azotés. Experiences paralelles sur la Digestion Gastrique et Intestinale; Inductions Cliniques. Par Lucien Corvisart. Paris, 1857–'58. (From the Author.)

Observations on Diphtheritis. By Willoughby F. Wade, B. A., M. B., &c. &c. London, 1858. (From the Author.)

A System of Human Anatomy, General and Special. By Erasmus Wilson, F. R. S., &c. A new and improved American from an enlarged London edition. Edited by Wm. H. Gobrecht, M. D., Prof. Anat. in Philada. Coll. Med. With 397 illustrations on wood. Philadelphia: Blanchard & Lea, 1858. (From the Publishers.)

A Practical Treatise on the Diseases of Children. By D. Francis Condie, M. D., F. C. P. P., &c. &c. Fifth edition revised and enlarged. Philadelphia: Blanchard & Lea, 1858. (From the Publishers.)

A Treatise on the Venereal Disease. By John Hunter, F. R. S. With copious additions, by Dr. Philip Ricord, Surgeon to the Hôpital du Midi, Paris, &c. Translated and edited, with notes, by Freeman J. Bumstead, M. D., Lecturer on Venereal at the Coll. Phys. and Surg. N. Y. Second edition, revised, containing a résumé of Ricord's recent Lectures on Chancre. Philadelphia: Blanchard & Lea, 1859. (From the Publishers.)

The Science and Art of Surgery. Being a Treatise on Surgical Injuries, Diseases, and Operations. By John Erichsen, Prof. Surg. and Clin. Surg. in University College, and Surgeon to University College Hospital. An improved American edition, from the second enlarged and carefully revised London edition. Illustrated by 417 engravings on wood. Philadelphia: Blanchard & Lea, 1859. (From the Publishers.)

Brief Expositions of Rational Medicine; to which is prefixed the Paradise of Doctors, a Fable. By Jacob Bigelow, M. D., late President of Massachusetts Med. Soc., &c. Boston: Phillips, Sampson, & Co., 1858. (From the Author.)

The Modern Practice of Midwifery. A Course of Lectures on Obstetrics, delivered at St. Mary's Hospital, London. By Wm. Tyler Smith, M. R. C. P., with an introductory Lecture on the History of the Art of Midwifery, and copious practical annotations, by Aug. K. Gardner, M. D., &c. &c. Illustrated by 212 engravings. New York: R. M. De Witt, 1858. (From the Editor.)

Selections from Favourite Prescriptions of Living American Practitioners. By Horace Green, M. D., &c. &c. &c. New York: Wiley & Halstead, 1858.

Illustrations of Typhus Fever in Great Britain, the result of personal observations made in the summer of 1853, with some remarks as to its origin, habits, symptoms, and pathology; to which is appended a brief account of the reappearance of Typhus in Boston in the winter of 1857–'58. By J. B. Upham, M. D., &c. Boston, 1858.

A Treatise on Fractures. By J. F. Malgaigne, Chirurgien de l'Hôpital St. Louis, &c. &c. With 106 illustrations. Translated from the French, with notes and additions, by John H. Packard, M. D. Philadelphia: J. B. Lippincott & Co., 1859. (From the Translator.)

A Treatise on Diseases of the Air Passages: comprising an inquiry into their history, causes, and treatment, of those affections of the throat called Bronchitis, Chronic Laryngitis, Clergyman's Sore Throat, &c. &c. By Horace Green, M. D., &c. &c. Fourth edition, revised and enlarged, with an appendix. New York: Wiley & Halsted, 1858.

An Essay on the Climate and Fevers of the Southwestern, Southern Atlantic, and Gulf States. By Jas. C. Harris, M. D., of Wetumpka, Ala. New Orleans, 1858. (From the Author.)

Diphtheritis: a concise historical and critical essay on the late epidemic Pseudo-membranous Sore throat of California (1856–'57), with a few remarks illustrating the diagnosis, pathology, and treatment of the disease. By V. J. Fourgeaud, M. D. Sacramenta, 1858.

An Inaugural Dissertation on Strychnia: presented to the Medical Faculty of McGill Medical College, May 1, 1858. By Alex. P. Reid. Montreal, 1858.

Induced Abortion, on account of extreme Narrowness of the Pelvis. Prize Essay of the Medical Faculty of Tubingen. Abridged and published by Ferd. Rallenmann, M. D., &c. Philadelphia, 1858.

Report on the Chemical Analysis of the White Sulphur Water of the Artesian Well of Lafayette, Ind. With remarks on the nature of Artesian Wells. By Chas. M. Wetherill, Ph. D., M. D. Lafayette, Ind., 1858.

Description of a New Midwifery Forceps, having a sliding pivot to prevent compression of the fœtal head (with cases). By Geo. T. Elliott, Jr., M. D. (From the Author.)

The Treatment of the Paralysis of Motion. By Charles F. Taylor, M. D., &c. (From the Author.)

An Inaugural Address, delivered before the New York Academy of Medicine, February 3, 1858. By J. P. Batchelder, M. D., President elect. New York, 1858. (From the Author.)

Communications to the Massachusetts Medical Society. By Ephriam Cutter, M. D. Boston, 1858. (From the Author.)

An Essay on Inflammation. By J. H. Watters, M. D. St. Louis, 1858. (From the Author.)

On Medical Colleges. An Introductory Lecture to the Course of 1858–'59 of the Medical Department of the University of Nashville. By J. Berrien Lindsley, Chancellor of the University. Nashville, 1858.

Quarterly Summary of the Transactions of the College of Physicians of Philadelphia. From March 3 to August 4, 1858, inclusive.

Transactions of the Third Session of the Medical Society of the State of California, convened at San Francisco, February, 1858. Sacramento, 1858.

Annual Report to the Legislature of South Carolina relating to the Registration of Births, Marriages, and Deaths, for the year ending December 31, 1857. Columbia, 1858. (From R. W. Gibbes, Jr., M. D., Registrar.)

Fifth Report to the General Assembly of Rhode Island, relating to the Registry and Returns of Births, Marriages, and Deaths, in the State, for the year ending December 31, 1857. Prepared under the direction of John R. Bartlett, Sec. of State. Providence, 1858.

Constitution, By-Laws, and Code of Ethics of the Iowa State Medical Society, together with the Transactions of the Eighth and Ninth Annual Meetings, held in Iowa City and Mount Pleasant, 1857–1858. (Davenport, 1858.)

Twenty-Second Annual Report of the Officers of the Vermont Asylum for the Insane, Aug., 1858. (Brattleboro', 1858.)

The following Journals have been received in exchange:—

Annales Médico-Psychologiques. Par MM. Baillarger, Cerise, et Moreau. July, October, 1858.

Le Moniteur des Hôpitaux. Redacteur en chef: M. H. De Castelnau. September, October, November, 1858.

Journal de la Physiologie de l'Homme et des Animaux. Publié sous la direction du Docteur E. Brown-Séquard. July, 1858.

Journal de Médecine de Bordeaux. Redacteur en chef: M. Costes. August, September, October, 1858.

Ophthalmic Hospital Reports, and Journal of the Royal London Ophthalmic Hospital. Edited by J. F. Streatfeild. July, 1858.

Edinburgh Medical Journal. September, October, November, 1858.

British Medical Journal. Edited by Andrew Winter, M. D. September, October, November, 1858.

Dublin Hospital Gazette. September, October, November, 1858.

Medical Times and Gazette. September, October, November, 1858.

The British and Foreign Medico-Chirurgical Review. October, 1858.

Guy's Hospital Reports. Edited by Samuel Wilks, M. D., and Alfred Poland. October, 1858.

The Sanatory Review and Journal of Public Health. Edited by B. W. Richardson, M. D. October, 1858.

The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M. D. October, 1858.

Edinburgh Veterinary Review. October, 1858.

The Glasgow Medical Journal. July, October, 1858.

The Indian Annals of Medical Science. July, 1858.

The Dublin Quarterly Journal of Medical Science. November, 1858.

The Medical Chronicle. Edited by Mr. Wright, M. D., D. C. MacCallum, M. D. Oct., Nov., Dec., 1858.

The Boston Medical and Surgical Journal. Edited by W. W. Morland, M. D., and Francis Minot, M. D. October, Nov., Dec., 1858.

The Chicago Medical Journal. Edited by N. S. Davis, M. D., and W. H. Byford, M. D. Aug., Sept., Oct., Nov., 1858.

Buffalo Medical Journal. Edited by Austin Flint, Jr., M. D. October, November, December, 1858.

The Cincinnati Lancet and Observer. Edited by George Mendenhall, M. D., J. A. Murphey, M. D., and E. B. Stevens, M. D. October, November, December, 1858.

The Medical and Surgical Reporter. Edited by S. W. Butler, M. D., and W. B. Atkinson, M. D. October, Nov., Dec., 1858.

American Druggists' Circular and Chemical Gazette. October, Nov., Dec., 1858.

American Medical Gazette. Edited by D. M. Reese, M. D. October, Nov., Dec., 1858.

The Maine Medical and Surgical Reporter. Conducted by Dr. W. R. Richardson and R. W. Cummins. October, Dec., 1858.

The Peninsular and Independent Medical Journal. Edited by A. B. Palmer, M. D., Moses Gunn, M. D., and F. Stearns. October, November, Dec., 1858.

Atlanta Medical and Surgical Journal. Edited by J. P. Logan, M. D., and W. F. Westmorland, M. D. October, November, Dec., 1858.

Nashville Journal of Medicine and Surgery. Edited by W. K. Bowling, M. D.,

R. C. Foster, M. D., and George S. Blackie, M. D. October, November, Dec., 1858.

Oglethorpe Medical and Surgical Journal. Edited by H. L. Byrd, M. D. and Holmes Steele, M. D. June, August, October, 1858.

The Virginia Medical Journal. Edited by James B. McCaw, M. D. and J. Otis, M. D. October, November, December, 1858.

Southern Medical and Surgical Journal. Edited by H. F. Campbell, M. D. and Robert Campbell, M. D. October, November, Dec., 1858.

The American Journal of Insanity. Edited by the Medical Officers of the New York State Lunatic Asylum. October, 1858.

The Medical Journal of North Carolina. Edited by Edward Warren, M. D. Aug., October, 1858.

The New Orleans Medical and Surgical Journal. Edited by Bennett Dowler, M. D. November, 1858.

The American Journal of Science and Arts. Conducted by Professors B. Silliman, B. Silliman, Jr., A. Gray, L. Agassiz, and Dr. W. Gibbs. November, 1858.

New York Journal of Medicine. Edited by Stephen Smith, M. D. November, 1858.

Charleston Medical Journal. Edited by J. Dickson Bruns, M. D. November, 1858.

Proceedings of the Academy of Natural Sciences of Philadelphia. October, November, 1858.

The Nashville Monthly Record of Medical and Physical Science. Edited by D. F. Wright, M. D., and R. O. Curry, M. D. October, November, December, 1858.

New Orleans Medical News and Hospital Gazette. Edited by D. W. Brickell, M. D., and E. D. Fenner, M. D. October, November, December, 1858.

The New Hampshire Journal of Medicine. Edited by G. H. Hubbard, M. D. October, November, 1858.

The Pacific Medical and Surgical Journal. Edited by John B. Trask, M. D., and David Worster, M. D. August, September, October, November, 1858.

The American Journal of Dental Science. Edited by Chapin A. Harris, D. D. S., and A. S. Piggot, M. D. October, 1858.

The North American Medico-Chirurgical Review. Edited by S. D. Gross, M. D., and T. G. Richardson, M. D. November, 1858.

St. Louis Medical and Surgical Journal. Edited by M. L. Linton, M. D., and W. M. McPheeters, M. D. November, 1858.

American Journal of Pharmacy. Published by Authority of the Philadelphia College of Pharmacy. Edited by William Procter, Jr. November, 1858.

The Ohio Medical and Surgical Journal. Edited by John Dawson, M. D., and J. W. Hamilton, M. D. November, 1858.

The Savannah Journal of Medicine. Edited by J. S. Sullivan, M. D., Juriah Harriss, and R. D. Arnold, M. D. November, 1858.

Communications intended for publication, and Books for Review, should be sent, *free of expense*, directed to Isaac Hays, M. D., Editor of the American Journal of the Medical Sciences, care of Messrs. Blanchard & Lea, Philadelphia. Parcels directed as above, and (carriage paid) under cover, to John Miller, Henrietta Street, Covent Garden, *London;* or M. Hector Bossange, Lib. quai Voltaire, No. 11, *Paris*, will reach us safely and without delay. We particularly request the attention of our foreign correspondents to the above, as we are often subjected to unnecessary expense for postage and carriage.

Private communications to the Editor, may be addressed to his residence, 1525 Locust Street.

All remittances of money, and letters on the *business* of the Journal, should be addressed *exclusively* to the publishers, Messrs. Blanchard & Lea.

☞ The advertisement-sheet belongs to the business department of the Journal, and all communications for it should be made to the publishers.

To secure insertion, all advertisements should be received by the 20th of the previous month.

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OF THE MEDICAL SCIENCES

FOR JANUARY 1859.

Art. I.—*On the Minute Structure of the Hepatic Lobules, particularly with reference to the Relationship between the Capillary Bloodvessels, the Hepatic Cells, and the Canals which carry off the Secretion of the latter.* By H. D. Schmidt, M. D., of Philadelphia. (Illustrated with 33 Figures.)

Several months since, in examining the microscopical structure of the liver, I obtained very unsatisfactory results from a mere superficial examination. The great discrepancy of opinion existing among the best histologists of the present day in regard to the minute anatomy of this organ attracted my attention, and, wishing to satisfy myself on this point, I determined to make it a special subject of investigation.

The modes I adopted for this have perhaps enabled me to work with greater advantage than others.

When we consider the complex structure of this organ, we need not wonder that the best microscopists have been baffled in the attempt to unravel its minute anatomy; though it is in some instances astonishing that their opinions should be so widely different.

The following pages are devoted only to the consideration of the minute structure of *the hepatic lobule, or the relationship existing between the capillary bloodvessels, the hepatic cells, and those passages or canals destined to carry off the secretion of the latter.*

Although I have examined the liver of several animals, yet I preferred for special investigation those of the sheep and hog. The liver of the latter has generally been recommended as being most suitable for investigation; but for injecting (although the difference is slight) I prefer that of the sheep. The portal vein and hepatic duct in the latter (like in that of man)

branch out at acute angles, and consequently offer less resistance to the passage of the injection, than in the hog in which the branches of the interlobular vessels mostly come off at right angles with the periphery of the lobule, so that a greater pressure is required to send the injection into them.

Since commencing these investigations I have endeavoured to obtain healthy human livers, but have as yet succeeded in only one instance. In this, unfortunately, the vena cava had been cut off so close to the organ, that it prevented me from throwing an injection into the hepatic veins; nevertheless, I injected the other vessels, and the examination proved satisfactory, though I did not consider the injection sufficiently perfect to serve for thorough investigation. I shall therefore postpone to a future period the investigation of the microscopical anatomy of the human liver.

Before proceeding further, it may perhaps be useful to cite the views of different observers on the subject.

E. H. WEBER[1] has been led to the conclusion that the bile-ducts form a network, the meshes of which fit exactly in that of the capillary bloodvessels of the liver. Both these networks are interwoven in such a manner that the one fills up the interspaces left by the other. The bile-ducts nowhere anastomose with the blood-carrying capillary system, but both classes of canals touch each other only on the sides of their walls. He further remarks that the rows of cells in the liver are true canals (this he proved by injections), and these form a network, the tubes of which are of the same diameter as the finest bile-ducts injected by him.

KRUCKENBERG,[2] who also injected a network of bile-ducts interwoven with the capillary blood system of the liver, is more cautious in his explanation than Weber. He believes that the hepatic cells lie in reticularly arranged tubes (finest bile-ducts), the walls of which, being very thin, were invisible. The latter, from being torn so easily, and on account of their reticular arrangement and intimate interweaving with the capillaries, could not be demonstrated. At the same time he refers to the uriniferous tubules, which, formed by the union of peculiar cells by means of a structureless membrane, have not always a visible tube.

THEILE[3] also believes in the existence of a membrana propria, in which the liver-cells lie. But this is only hypothesis, as he has not seen it. According to him, the latter fill up the membrana propria, for which reason the injecting matter penetrated to the periphery of the lobule, but not into the tubes themselves.

BACKER[4] even pretends to have seen this membrana propria, which by Kruckenberg and Theile is only hypothetically thought to exist. He describes it as a structureless membrane, covered by longitudinal fibres, which

[1] Gerlach's Gewebelehre, II. Auflage, p. 329. [2] Ibid., p. 330.
[3] Ibid., p. 330. [4] Ibid., p. 330.

only becomes invisible by drying, or when the cells (by imbibition of liquid) swell up, and thus lie close to the membrana propria.

REZIUS and WEJA[1] make statements to the same effect.

KRAUSE[2] declares that the bile-ducts take their starting-points from vesicles or true acini. These acini ought not to be confounded with the lobules of the liver; but they are, according to him, small round or slightly oval bodies, which by reflected light have a yellowish-gray appearance. They inclose six or eight hepatic cells, and form the greater part of the mass of the lobule. He also thinks that these small bodies, as they were not observed by others, had been looked upon as very large liver-cells.

LEREBOULLET,[3] in regard to the arrangement of the biliary cells, says:—

"The biliary cells are joined by their ends so as to form longitudinal series which converge towards the centre of the lobule. These longitudinal series are united by shorter transverse ones, so as to represent a network with meshes, polygonal or rounded, at the periphery of the lobule, and elongated towards its central part.

"Each thread of this network is double—that is, formed by two ranges of cells, which touch at their sides, and leave only a linear interval between them. But these two ranges of cells are only in juxtaposition, separating easily by the slightest traction.

"The cells which constitute the series are, on the contrary, adherent to each other. Hence we frequently see simple series of cells yet adherent after tearing a minute piece of the substance of the liver.

"These series or chains of cells do not form tubes, as was supposed by E. H. Weber. The cells which compose them do not open into each other, but are, on the contrary, perfectly distinct and independent.

"The network formed by the double ranges of cells pervades the whole thickness of the lobule from the perilobular vessels to the central one. Hence it is inaccurate to speak of the secretion taking place exclusively at the periphery of the lobule. The meshes of the network of cells are filled by the bloodvessels of the lobule.

"The double threads of the biliary network are probably surrounded by a proper membrane, which would constitute the basement membrane of the secretory tubes; but this is so adherent to the walls of the bloodvessels, as to render it impossible to prepare and demonstrate it in such a manner as to show that the included biliary cells are only epithelial. Therefore, in the natural state, these secretory tubes within the lobule would be full—that is, entirely occupied by the secretory cells; and hence their cavity is simply linear.

"When we succeed in throwing an injection into these biliary passages, the injected matter distends the linear intervals just described, compresses the cells, and gives the appearance of a rete of capillary ducts, which takes the place of the network formed by the double ranges of cells.

"The capillary biliary ducts of authors are, then, produced mechanically by the injection. These canaliculi have, indeed, no proper walls, the injected matter being in immediate contact with the secretory cells.

"The rest of the lobule is occupied by a vascular rete, formed by the por-

[1] Gerlach's Gewebelehre, II. Auflage, p. 330. [2] Ibid., p. 331.

[3] Medical Examiner, vol. x. (1854) N. S., p. 206 *et seq.*

tal vein and the radicals of the hepatic vein. The meshes of this network adapt themselves exactly to the threads of the biliary rete, and *vice versa*, so that the two are closely interlaced. The mean diameter of the threads forming the meshes, and of the meshes themselves, in either network, is 0.15 of a millimetre."

Further on, he states that "the biliary canals which have lobules, are always multiple. They arise from all points of the surface of the lobule, and after having frequently united with each other like the roots of a tree, leave the lobule and form one or more ducts, which, with the corresponding trunks of the portal vein and hepatic artery, are surrounded by a fibrous sheath, the capsule of Glisson."

LEIDY,[1] in his researches into the comparative structure of the liver, in speaking of the biliary tubes, says:—

"The lobules are composed of an intertexture of biliary tubes (*pori biliari*), and in the areolæ or interspaces of the network the bloodvessels ramify and form among themselves an intricate anastomosis, the whole being intimately connected together by a combination of white fibrous and yellow elastic tissue."

"In structure, the biliary tubes correspond with those of the interverte-brata, consisting of cylinders of basement-membrane containing numerous secreting cells, and the only difference exists in the arrangement; the free tubes of the lower animals in the vertebrata becoming anastomosed, or forming an intertexture. The tubuli vary, in size in an unimportant degree, in different animals, and also in the same animal, being generally from two to two and a half times the diameter of the secreting cells. The tubes of one lobule are distinct from those of the neighbouring lobuli, or only communicate indirectly by means of the trunks or hepatic ducts originating from the tubes and lying in the interspaces of the lobuli. The secreting cells are irregularly angular, or polygonal in form, from mutual pressure, and line the interior surface of the tubes. They vary in size in a moderate degree in different animals and also in the same animal, appearing to depend upon certain conditions of the animal and liver. The colour is light-yellowish, or brownish when in mass; the other and darker colours of the liver appearing to depend upon the blood in the organ. They contain a finely granular matter, oil-globules, a granular nucleus, and a transparent nucleolus."

BEALE,[2] one of the latest observers on the minute structure of the liver, and to whom I shall have occasion to refer again, maintains: That the smallest biliary ducts are directly continuous with the tubular network of basement-membrane in which the liver cells lie; for, in favourable specimens, injection, forced in from the duct, will pass into every part of the tubular network, even quite to the centre of the lobule. It is possible to inject the capillary network in the same preparation as that in which the ducts and cell containing network are injected.

KÖLLIKER[3] remarks, that in all his continued search, he has never dis-

[1] American Journal of the Med. Sciences. New Series. Vol. xv. p. 18.

[2] Beale, On the Anatomy of the Liver, 1856, p. 54.

[3] Kölliker's Handbuch der Gewebelehre des Menschen, 1852, p. 421.

tinctly observed a direct connection of the finest ducts with the network of hepatic cells, which, he continues, is not surprising in considering the softness of the parts in question; yet, leaves an opening in the minute anatomy of the liver which can scarcely be closed by hypotheses. As such, he offers the supposition that the finest ducts come in direct opposition with the columns of the network of hepatic cells, and thus have their orifices closed. He believes that such connections exist in no very great numbers at the periphery of the hepatic islets, which might be inferred from the scanty number of the finest branches of the hepatic duct.

The bile, he says, must be transmitted outwards from cell to cell, for the possibility of which process he refers to the physiology of vegetables.

Views, similar to those of Kölliker are entertained by C. HANDFIELD JONES.[1]

This observer, in regard to the excretory ducts, states: That the liver in all vertebrate animals may be regarded as consisting of a secreting parenchyma and excretory ducts. The size of the excretory apparatus bears only a small proportion to that of the secretory.

These two portions of the liver are not continuous with one another, but disposed simply in relation of juxtaposition.

The action of the liver seems to consist in the transmission of the bile as it is formed from cell to cell, till it arrives in the neighbourhood of the excretory ducts by which it is absorbed. This action is probably slow, and very liable to be interfered with, contrasting remarkably with that of the kidney, where a particular apparatus is added to insure completeness and rapidity of action.

In a second paper, written some years afterwards,[2] he says that farther observations confirm him still in the opinion he formerly expressed.

GERLACH,[3] from observations made on injected livers, comes to the conclusion that the intralobular ducts, after having arrived at the lobule, send off small branches, 0.002 to 0.004 of a millimetre in diameter, which, after having formed a free anastomosis at the periphery of the lobule, terminate in intercellular passages or free spaces, left between the cells. He remarks,[4] that the sudden transition of true tubes into intercellular passages, where the structureless membrane of the tubes ceases abruptly, is certainly a very uncommon phenomenon and altogether wanting of analogy. But the results of his numerous injections point it out so distinctly, and exclude every other explanation so perfectly, that he does not think proper to change his views. As a farther confirmation, he observed the termination of a small duct, belonging to the peripheral anastomosis, in a specimen of injected human liver. Here, he saw, with the greatest distinctness, that it terminated by an open mouth, in opposition to the views of H. Jones.

[1] Philosophical Transactions of the Royal Society of London, 1849, p. 132.
[2] Ibid., 1853, p. 2. [3] Gerlach's Gewebelehre, II. Auflage, p. 333.
[4] Gerlach's Gewebelehre, II. Auflage, p. 336.

Since I commenced this article I have met with the valuable paper of M. NATALIS GUILLOT, "On the Structure of the Liver of Vertebrate Animals."[1] The results of his extensive researches, which were made ten years ago on injected specimens, are almost the same as those obtained by myself. The only difference is, that by means of superior accessory instruments my conclusions were drawn not only from observations on well injected specimens, but also from the fragments of fresh livers from divers animals.

Guillot, in speaking of the termination of the hepatic duct, says:—

"After having followed the ramifications of the portal vein and the hepatic artery, and having surrounded them with numerous loops, and after having connected themselves a thousand times by the finest anastomoses, the biliary vessels (ducts) are replaced, or, in other words, continued by an *order of canals*, the traces of which may be recognized in the middle of the mass of hepatic cells.

"It is, to the middle of the islets,[2] surrounded by the anastomoses of the capillaries of the blood that this collection (ensemble) of biliary canals may be seen; they can be traced to this place, not only in fishes, reptiles, and birds, but also in mammiferous animals, and in the liver of man.

"They cannot be distinguished without first being made visible by means of injection. Without this indispensable precaution, the finest slices of the liver of fish, bird, or mammiferous animal, will give no evidence of the existence of any sort of regular canals."[3]

[1] Annales des Sciences Naturelles. Troisième série. Zoologie. Tome ix. p. 163.

[2] Groups of six or eight cells contained within the meshes of the capillary blood-vessels, but connected with each other.

[3] As I consider the observations of Guillot important to the confirmation of my own, I will quote his own words: *Annales des Sciences Naturelles*, p. 132.

"Ayant cherché à savoir si cette manière de voir était exacte, elle m'a, au contraire, semblé fort douteuse. Quelsqu' aient été les animaux soumis à mes observations, nulle part l'agrégation de ces particules (cells) ne m'a paru être régulière.

Lorsque les particules du foie des animaux vertébrés sont réunies les unes auprès des autres, l'irrégularité des points de contact, par lesquels elles se touchent, apparaît avec une grande évidence. On est ensuite frappé du caractère singulier des fragments observés, lorsqu'on a fait aucun effort capable des dissocier les éléments qui les composent, et de les éloigner les uns des autres. Ils forment alors une agrégation tellemens serrées, qu'il paraît impossible à la bile ou au sang de trouver un passage entre ces particules.

Cette apparance est commune à tous les animaux.

Ceux d'entre eux qui ont péris par suite d'une hémorrhagie offrent au plus haut degrée cette contiguïté des particules du foie; chez ceux, au contraire, dont la mort a été lente, il reste encore une assez grande quantité de sang dans l'organe pour donner à la matière des caractères entièrement opposées. Certain details apparaissent alors, obscure encore, il est vrai, mais trop intéressants pour ne pas meriter une serieuse attention.

Dans ces organes pénétrés d'une certain quantité de sang, ce liquide est encore contenu dans les canaux, au travers desquels il circulait pendant la vie, et c'est

It will be seen from the above, that the views of the majority of observers have been based mainly on hypotheses or analogy. I shall, therefore, be very guarded in expressing any opinion not founded on observation, and shall leave the rest to the judgment of men more experienced on the subject than myself.

While, with Guillot, Gerlach, Beale, and others, I consider a good injection absolutely necessary for a thorough investigation of the structure of the liver, yet I do not think that we should confine ourselves altogether to this mode of investigation. We ought also assiduously to examine the tissue of the organ in its fresh state; but if the choice were left to me between the two modes of investigation, I would unhesitatingly give the preference to the former. We are surely better enabled to distinguish the relationship of the vessels, canals, &c., in a thin, transparent slice of the organ, when they are well filled with colouring matter, than on a fragment of soft tissue which mostly shows only a confused mass of ducts, capillaries, cells, fibrous tissue, &c. Although after long practice and study we succeed in distinguishing readily a capillary, duct, &c., yet it is extremely difficult, after these delicate

precisement dans les endroits où les globules sanguins séjournent que les particules du foie cessent de se toucher.

Par un examen attentif des parties où l'on observe les globules sanguins, on peut déjà être conduit à distinguer, certains canaux régulièrement disposés dans l'épaisseur de la matière.

Les traces régulières de ces canaux n'indiquent-elles pas déjà que les particules du foie, appliquées les unes contre les autres dans quelques circonstances sont forcées, dans d'autres cas, de s'éloigner de celles qui les toucheraient si l'organe était privé de sang?

Négligeant maintenant d'autres considerations, je ne m'attacherai qu'à faire remarquer l'evidence avec laquelle ces canaux apparaissent, lorsqu'on examine, même sans de très forts grossisements, des parcelles de foie injectées avec l'eau colorée, l'essence de térébenthine ou même le mercure. Ils effacent, et disparaissent dès ques ces liquides se sont écoulées, et les particules redeviennent alors comme auparavant exactement appliquées les unes sur les autres. C'est principalement à ces études que sont utiles les injections de matières diffluentes.

P. 163. Après avoir suivi les ramifications de la veine porte et de l'artère, les avoir entourées d'anses numbreuses, après être unies mille fois par des anastomoses de plus en plus fines, les vaisseaux biliares sont remplacées ou mieux continuées par un ordre de canaux dont on reconnaît les traces au milieu de la masse des particules du foie.

C'est jusqu'au milieu des îlots entourées par les anastomoses des canaux sanguins que l'on decouvre cet ensemble de canaux biliaires ; on peut les suivre jusqu' à cet endroit non seulement dans les Poissons, les Reptiles et les Oiseaux, mais encore dans les animaux mammifères et sur le foie de l'Homme.

On ne peut les distinguer sans les avoir mis en evidence à l'aide d'une injection préalable. Sans cette précaution indispensable, les tranches les plus minces du foie d'un Poisson, d'un Oiseau ou d'un animal mammifère, ne laisseraient supçouner l'existence d'aucune espèce de canal régulier. Les particules du foie serraient alors immédiatement appliquées les unes sur les autres."

parts have been roughly torn and displaced by means of needles, to detect the relative position they held before their separation.

As a good, minute injection of the organ is so important for its investigation, it may be proper here to make a few remarks on this subject.

If I may judge from the expressions of different authors, it seems that minute injections have often been undervalued as a means of microscopic investigation. The reason for it is very obvious. To acquire facility in making injections requires much practice, and the expenditure of more time than most persons can devote to it. Besides this, it is a tedious and vexatious process. Frequently a small vessel will rupture, and the colouring material be thrown over the person of the operator. It is also expensive, as a great deal of material is wasted before experience enough is acquired to have the process perfectly under control. Many disappointments are met with; and if the injector be not possessed of a good share of perseverance, he will certainly become discouraged and give up the matter. Further, to be a skilful injector requires not only some mechanical skill and judgment, but also manual dexterity; which, unfortunately, all men of science do not possess. I have frequently seen profound students using their fingers as awkwardly as a child. Experience, and manual dexterity, are therefore required to make good injections; and I have no doubt that those anatomists renowned for their beautiful injections, as Bèrres, Hyrtl, and others, possessed both.

To make a good injection, the pressure should be applied very gradually. In injecting a liver, I am in the habit of first injecting the duct, then the artery, and lastly the veins. With one exception, I have always injected the entire liver, although small portions may be used. In order to inject the organ perfectly, it should be healthy and uninjured.

The material to be injected is a most important consideration. I believe that gelatine has been a favourite vehicle of many anatomists for the conveyance of the colouring matter. In former experiments I have frequently used it; but there are inconveniences attending it, and now I never employ it, except for special objects. For instance, the organ to be injected must be kept at the same degree of temperature required for retaining the injecting matter in a fluid state; besides this, the colours cannot be readily mixed with it, as few of them are soluble in water; it is also very inconvenient to strain the material.

Ether I have found to be the most easily managed liquid. It is one of the most penetrating of fluids, but by itself is of too low a specific gravity to carry a heavy substance like vermilion; therefore it is necessary to give it a body. For this purpose the resins, wax, fats, &c., may be used, but the best material is *Canada balsam;* this is an excellent vehicle for carrying the colouring matter into the minute biliary vessels. I am unable to state the precise density which answers best. My mode of determining this is by letting some drops of it fall on a piece of glass; it ought to evaporate in about

half a minute, and leave a body which may be tested by scratching it with a needle. After the solution is brought to its proper density, it must be filtered through good filtering paper, for any liquid thin enough to pass the paper will also penetrate into the capillaries. Besides the Canada balsam, I use wax. The density of this solution is regulated by the filtering paper; if it is too dense, the superfluous wax remains behind. This solution always looks clear when well filtered. If we use the solution of Canada balsam alone for the injections of tissues to be dried and then cut in thin, transparent slices, they become too hard; to avoid this, I usually mix the solution of balsam and that of wax, in equal proportions, as the wax gives softness and pliability to the preparation. But for the injection of the biliary ducts I use the solution of Canada balsam alone, as the wax is granular.

Another important point in regard to the injecting matter is the consideration of the colour to be mixed with it. The finest colours are those ground up with linseed oil, and used by artists. By the process of trituration which they undergo they are thoroughly mixed with the oil, which is very soluble in ether.

My only method of testing the density of the coloured liquid is by slightly shaking the bottle in which it is contained, and then observing whether the colour is dense enough to remain upon the glass for a few seconds, before falling to the bottom.

For the injection of the biliary ducts I use only half the amount of colouring matter for filling the small ramifications, and afterwards inject a denser liquid, which, by pressing upon the former, forces it into the smaller passages. The colour I prefer for the ducts is chrome-yellow. After a little practice the operator becomes familiar with these particulars, and distinguishes them without losing much time in weighing and measuring.

After a liver has been well injected, it should be dried in the air for three or four fine days, so that the ether may evaporate, then be cut into slices 1 or $1\frac{1}{2}$ inch thick. Without this precaution the peritoneal covering prevents the evaporation of the watery parts, and thus a longer time is required for drying.

In regard to the mode of making the best sections, I refer to the description of the *apparatus for making microscopic sections.* The sections, if well made, must be transparent. They should be examined in some liquid, as water, glycerine, &c.

The tissue of the liver is no more altered by the action of ether than by that of alcohol. The former coagulates the albumen, giving a more granular appearance to the cells. In fine sections the nucleus is difficult to recognize, but I have sometimes seen it. The watery parts of the tissue lost in the process of drying, are regained when the tissue is immersed in liquid for a short time, so that it is as good for examination afterwards as before. Some may object to the examination of specimens which have

been dried; but the relationship of the structure is not altered. If the cell loses its watery parts, the capillary or the duct does so likewise; both will imbibe in like proportion, the amount of colouring matter remaining the same.

Having now given the necessary directions for the injection of the organ, I shall proceed to the consideration of the anatomy of the hepatic lobule.

The views I had been taught, and which I still entertained when commencing these investigations, were, that the cells lay within a network of tubes of basement membrane, continuous with the branches of the duct; and, reasoning by analogy, these views seemed to me probable, for I could not believe that the bile was transmitted from cell to cell until it reached the open or closed mouth of the branches of the hepatic duct.

When I examined specimens of the first liver injected by me, which was that of a cat, I noticed a reticular arrangement of the colour (chrome-yellow) which I had thrown into the hepatic duct. This examination was merely a superficial one, and made on opaque pieces by reflected light. Although my object had been to inject the network of tubes of basement membrane, yet I was much surprised at my early success, and the idea suggested itself that a duct had been ruptured and the colour had entered into the bloodvessels. After some reflection, I saw the improbability of the liquid having ruptured a duct from within, and then perforating the wall of a bloodvessel from without. In several other injections I obtained the same results; but all these examinations were made with reflected light, for I had not yet made a thorough examination of transparent sections with transmitted light. I also examined fragments of fresh livers; and yet, with all my constant efforts, I could never discover the slightest evidence of the existence of the tube of basement membrane. The rows of cells I always met with seemed to be held in close apposition by some invisible agent. I saw capillaries with their nuclei, ducts lined by an epithelium, fibrous tissue, &c., but no membrana propria.

I met with a similar disappointment in examining fine sections. The rete injected had not the appearance of one formed by tubes large enough to contain hepatic cells. I noticed, too, that this injected rete corresponded mostly with the course of the capillaries, only crossing the latter here and there; which observation led me to think that this rete was an independent one.

Further observations on injected and fresh specimens of liver have confirmed opinions which I will state in substance before proceeding to details, viz: *Two capillary networks, each independent of the other, exist in the lobule of the liver; the one, commencing at the periphery of the lobule, from the smallest branches of the portal vein and hepatic artery, and ending in the centre in those of the hepatic vein, is destined for the circulation of the blood brought there by the portal vein and hepatic artery; the other, commencing independently in the centre of the lobule, near the*

intralobular vein (branch of the hepatic vein), and ending in the smallest branches of the hepatic duct, is most probably destined to carry off the secretion of the cells. The cells lie within the meshes of these two networks, but seem to be especially held in their position by their adhesion to the network destined for the secretion.

These fine biliary vessels are in reality *biliary capillaries;* but, for the sake of contradistinction from the capillaries that carry blood, I shall call them *biliary tubules,* until my observations have been confirmed by others, and a better name proposed.

The observations made on injected specimens shall be first considered.

In sheep, as in man, the lobules of the liver have no definite borders, hence it is difficult to say where one ceases and the other commences; we can only judge by the relation and proximity of the branches of the different vessels. The intralobular branch of the hepatic vein runs at a right angle with the portal vein, which is accompanied very closely by the hepatic duct. The portal vein sends off branches which ultimately ramify into the capillary system; similar branches of the hepatic duct pass into a system of their own, the boundaries of which extend to the intralobular vein. The capillaries of the duct (biliary tubules) are mostly seen lying alongside of those of the portal vein, except when they cross each other to form an interlacement; the cells lie in the interspaces.

Pl. I. Fig. 1, which represents a thin transparent section of a part of the lobule of the sheep (viewed by transmitted light), conveys a good idea of the relationship existing between the branches and capillaries of the portal vein and those of the duct. Here, we see at (*a*) the transverse section of a branch of the former, and at (*b*) one of the latter; both send off smaller branches, and after having become capillaries, interlace themselves; between and around the two vessels the fibrous tissue can be seen, belonging to the capsule of Glisson.

In the liver of the hog (Pl. I. Fig. 2) each lobule is enveloped by a capsule of fibrous tissue; which by some anatomists is supposed to be a continuation of the capsule of Glisson. The branches of the *portal vein,* after entering between the lobule, divide into numerous other smaller ones, which surround the capsule; these again give off shorter ones, which penetrate the capsule to form the interior capillary network. The branches of the *duct* lie close to those of the portal vein, and are given off exactly in the same manner. The *hepatic artery* also closely follows the vein and surrounds it with a network of its branches; its finest ramifications, after having penetrated the capsule, are blended with the blood carrying capillaries of the lobule, and thus the blood of both vessels is mixed within the latter. I have often seen the capillary network, of entire lobules, filled with the colour *injected through the artery.*

In the liver of the sheep I have noticed very extensive anastomoses of the small branches of the duct around the branches of the portal vein.

It is believed by some, that the meshes of the capillaries are more oblong near the intralobular vein than the portal branches, but I find little difference between them; and in some instances, in the liver of the sheep, I have observed directly the reverse. The capillary network seems to be formed (at least in the hog) by more or less strait vessels, radiating from the intralobular vein as a common centre towards the periphery of the lobule; these vessels are connected with each other by shorter transverse branches, and thus the rete is formed. In fresh specimens I have often observed capillary vessels as long as six or eight liver cells with the remnants of the broken transverse branches adhering to them. The cells I believe to be arranged in the same manner, that is, generally in single rows, radiating from the centre and connected by shorter ones. Of course, these radiating vessels and rows of cells, arising from the periphery, cannot all run to the centre, but the greater number of them are lost between the others.

In fine transparent sections I have observed a tendency to split in the lobules of the liver of the hog; usually this commences at the centre, extending towards the periphery, though it sometimes occurs from one periphery to the other, through a small portion of the lobule. Searching for the cause of this in entire lobules, I found it to be fine transparent lines, bounded by fine double contours, and running mostly from the centre of the lobule towards a branch of the portal vein; other finer lines, with the same contours, are seen extending into them; the course of both are usually serpentine. The capillaries can be seen running across these lines. This has been observed in hundreds of lobules, for my apparatus enables me to make sections, containing about eighty lobules, with great rapidity. In tracing one of these lines, I observed in one instance that it passed from one lobule to another through the fibrous tissue of the capsule; in some cases more than one is seen in the same lobule. (In examining sections of the liver of sheep, treated with a weak solution of potassa, which renders them transparent, I observed empty vessels running towards the vein. At first I supposed them to be lymphatics, until I noticed others as large as the finer branches of the portal vein, which made me suspect them to be uninjected vessels.)

That the above-mentioned fine transparent lines in the hepatic lobule of the hog are not nerve fibres, seems evident to me, since the course of a nerve is more wavy than these lines. Are they then the ultimate branches of the *lymphatics* which have not yet been observed? I merely mention this for the purpose of directing the attention of other observers, not having examined it sufficiently to form an opinion myself.

The *biliary tubules* can also be recognized in specimens in which only the capillaries have been injected. If such a section is treated with a weak solution of potassa, which makes it expand, fragments of fine vessels can be seen at the sides of the capillaries, often traversing the latter. Such a view is represented in Pl. I. Fig. 4. If these double contours extended entirely

along the sides of the capillaries, they might be taken for their walls; as it is, I suppose them to be nothing but the fragments of uninjected biliary tubules, especially as we find them here and there crossing the capillaries. The dark places in the vessels are caused by the presence of colouring matter.

I have already advocated the mode of investigation by injection. But as some may consider the injected biliary tubules as nothing more than extravasations between the cells, I will adduce further considerations in support of my views.

An extravasation is very readily distinguished from a perfect injection by one accustomed to the examination of minute injections. It is an infiltration into the tissue, caused by the rupture of one or more of the capillaries, and looks very irregular. The interspaces between the capillaries are filled up by the colouring matter, if the extravasation has taken place to any extent; the material sometimes forms small curves, but these can never be mistaken for a regular arrangement of vessels; Pl. II. Fig. 9 represents the aspect of a duct whose branches are ruptured and the colour extravasated. Every one can recognize the very great contrast between it and Fig. 3, Pl. I., which represents a part of a thin section of the liver of the hog, in which only the duct has been injected. Here the regular arrangement of the injected *biliary tubules* is too evident and striking to be mistaken for an extravasation. If the extravasation is slight, it has an aspect somewhat like Fig. 8, Pl. II. This has probably been the case with *Gerlach's* injections of the ducts, which led him to think that these canals had no walls of their own, but were intercellular spaces. If the drawing, accompanying the extract from *Lereboullet's* researches on the intimate structure of the liver, in the *Medical Examiner*, be a true copy, I can only consider it the representation of a complete extravasation into the interspaces of the capillary network of the lobule. The injected specimens examined by C. H. Jones seem also to have been imperfect. In the injections of the livers of the pig, of which he speaks,[1] the fault, most probably, was in the material, which consisted either of a bad vehicle contained with too much colouring matter, preventing it from penetrating into the biliary tubules. The trials which he afterwards made[2] on only two livers with the acetate of lead, were not sufficient for drawing any conclusion. The acetate of lead is not a proper material for injections: it formed, as he says, a precipitate with the albuminous plasma between the cells, but only after having ruptured the biliary tubules. The injecting material should be as neutral as possible.

Very slight pressure is required for the injection of the biliary tubules; less than that for injection of the bloodvessels.

In regard to the theory of the cell containing network of tubes of base-

[1] Phil. Trans., 1849, p. 125. [2] Ib., 1853, p. 2.

ment membrane, I would observe that if such an arrangement existed in the hepatic lobule, and the tubes of this were injected, a section of it would give the appearance represented in diagram Pl. II. Fig. 7. The injected rete, instead of being alongside the capillaries, would occupy the middle of the meshes and send little branches for a short distance between the cells. Besides this, there is another very important fact, which seems to have been entirely overlooked; this is in regard to measurement. The diameter of a cell is twice or three times that of a capillary. Now, the interspaces of the capillaries in a thin section of injected liver are not large enough to admit more than one cell; again, the cells in fresh specimens are mostly met with, arranged in single rows; we seldom see them double. It might be said that the capillaries are distended by the colouring matter; but when this is the case, the interspaces will be seen almost obliterated by the distension of the vessels. If, on the other hand, the capillaries are but moderately filled, it might be said that the cells collapse and shrink away. This view, however, cannot be sustained, because if the cells shrink away, the ethereal solution in the capillaries will evaporate and leave nothing behind but the colouring matter and other solid elements of the solution. This can be well seen in Pl. I. Fig. 4, where both capillaries and cells have expanded again by the action of the potassa; the colouring matter not being sufficient to fill up the vessels. Pl. I. Fig. 6, which represents the outlines of the capillaries of a thin section, treated by a weak solution of potassa, shows this also. Pl. I. Fig. 5 represents two cells from the same piece; they overlap each other, and are too large to be admitted through some of the meshes.

Additional confirmation of the views above expressed, have been obtained by observations on fragments of the fresh liver, in which I demonstrated the existence of the *biliary tubules* very satisfactorily to myself in different modes, as follows:—

When we take a small fragment of liver, tear and separate its constituents on a glass slide, and then place it under the microscope for examination, it is only by accident we meet with a favourable exposition of capillaries, ducts, &c.; and, even then, it is no easy matter to observe in this confused mass, the true relationship of the elements of which it is composed. But if we can separate these parts by means of fine needles, and in the mean time observe all the details of the process, points are brought into view, which before were hidden to our closest observations.

The Microscopic Dissector, I invented for this purpose, has enabled me to make such observations. (See description of this instrument.)

For this purpose the liver of the hog is especially suitable. In cutting through one of its lobules, and taking a small fragment from its contents, we are certain not to have fibrous tissue mixed with the cells and capillaries, for the capsule of Glisson does not extend into the interior of the lobule. To corroborate this, I will cite the opinion of Beale, who is one

of the latest writers on this subject. In speaking of the capsule of Glisson, he says:[1]—

> "*Most anatomists have failed to demonstrate a trace of areolar tissue within the lobules of the liver. Occasionally a few fibres of a structure like fibrous tissue, undoubtedly, is observed in uninjected specimens; but such an appearance is produced by physical alterations of the structures in the lobule, in the preparation of the specimen, or it is the result of disease. In the lobules of the livers of all animals which have fallen under my notice, it was impossible to demonstrate any fibrous structure whatever.*
>
> "*Even in the interlobular fissures of the human liver, and of others allied to it in structure, I have been unable to detect any fibrous structure.* BOWMAN, HENLE, *and* VOGEL *have altogether failed to detect any areolar tissue in this situation in the human liver.*"

In examining such fragments, taken from the lobule of the hog, we often meet with rows of cells, either floating free or still adhering at one extremity to the fragment. In taking hold of the extremities by means of the needles of the dissector, and then separating them in a very slight degree, the row of cells will first become more straightened, and afterwards one or two of them usually become elongated; if the needles are still more separated (but in the slightest possible degree), the separation of those two cells may be observed, and one or two *tubular elements* will appear between them. In Pl. II. Figs. 10 and 11, this appearance is represented. These tubular elements I believe to be the *biliary tubules;* their diameter is $\frac{1}{16500}$ of an inch. Little dilations are seen here and there in their course, which appear very distinct after the treatment with alcohol and other similar reagents, which often gives them the appearance of a row of beads. The remnants of their branches are almost always still adhering; very often we meet with the entire branches, representing meshes from which the cells have escaped. Pl. II. Fig. 12 is an exact representation of a good specimen. They may be stretched to a great extent without breaking, and are very distensible, which is proved by injection. Unlike the fibrils of connective tissue, these elements do not swell up when treated with acetic acid; furthermore, their contours are not sharp and distinct like those of connective tissue, but are softer and often irregular.

It has already been mentioned that the existence of connective tissue in the lobule of the liver, has been denied by most anatomists. If then these opinions be correct, the question, what are *these elements?* remains open for discussion. The only answer I can give is, that they are the *biliary tubules,* which I injected not merely in a few but in numerous livers.

In the liver of the ox I have, in some instances, by means of the dissector, isolated one of the smaller branches of the duct, $\frac{1}{3300}$ of an inch in diameter, with the remnants of biliary tubules still adhering.

Pl. II. Fig. 13 represents a dissection of a fragment of the liver of the hog, made with the Microscopic Dissector. The outlines of this drawing are accurately copied from the dissection, but for the sake of distinctness

[1] Beale on the Anatomy of the Liver, p. 19.

I have altered the shading. As the constituents of the tissue are transparent to some extent, some of the capillaries lying either above or below, some cells are only distinctly demonstrated by observing closely the changes of their position, produced by the movements of the needles; the better to distinguish them they are left light. This manœuvre is one of the greatest advantages derived from the *Microscopic Dissector.*

But it is not only by injection or by means of dissection that the *biliary tubules* can be demonstrated. They can also be seen without any previous preparation with a high power. If a fragment from the interior of the lobules of the liver of the hog be taken, and after having been treated with ether (to get rid of the oil), it is examined with compression, a network of light streaks is seen, which does *not* correspond with the outlines of the cells, as might be supposed; this is the network of the *biliary tubules;* the capillaries with their nuclei can also be recognized in such a preparation. Even in fragments, consisting only of 4–8 cells, the tubules can be seen.

In taking a fragment (which has been treated with ether and then compressed) and tearing it slightly without separating it, we often observe in the fissures, produced by the separation, the tubules running from one margin to the other; after they have once been demonstrated satisfactorily, they can be recognized under almost any circumstances.

EXPLANATION OF THE PLATES.—PLATE I. *Fig.* 1. A thin section of a portion of a hepatic lobule of the sheep. *a.* Transverse section of a branch of the portal vein; two smaller branches are given off, which terminate in the capillary system. *b.* Transverse section of a branch of the hepatic duct; its finest branches are seen terminating in the network of biliary tubules. The fibrils of the capsule of Glisson are seen between the vessels; the interspaces of the capillaries are filled up by the cells. Magnified 172 diameters.

Fig. 2. A portion of a hepatic lobule of the hog. *a, a.* Transverse section of branches of the portal vein, sending off their branches to ramify around the lobule; they are enveloped by the capsule of Glisson. Shorter and smaller branches are seen to penetrate the capsule, terminating in the capillary network. *b, b.* Transverse section of branches of the duct, which are distributed in the same manner as those of the portal vein, terminating in a capillary system of their own. *c.* Transverse section of an intralobular vein (branch of the hepatic vein). *d.* Hepatic artery. *e.* Fibrils of the tissue of the capsule. The cells are seen in the interspaces of the capillaries. Magnified 172 diameters.

Fig. 3. A thin section of the hepatic lobule of the hog, in which the duct alone is injected. *a.* Branch of the hepatic duct, terminating in the network of biliary tubules. *b.* Fibrous tissue of the capsule. The cells are seen in the interspaces. Magnified 92 diameters.

Fig. 4. A thin section of the hepatic lobule of the hog, treated with a weak solution of potassa. *a, a, a.* Capillaries. *b, b.* Remnants of the uninjected biliary tubules. The dark places are caused by the colouring matter in the vessels. Magnified 400 diameters.

Fig. 1.

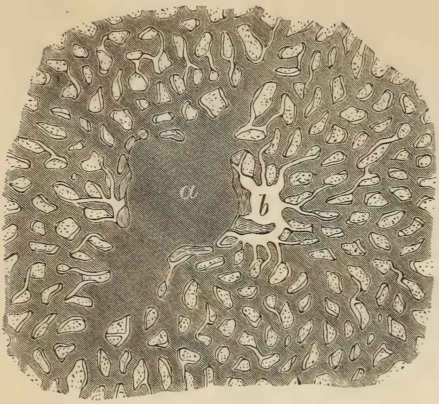


Fig. 2.

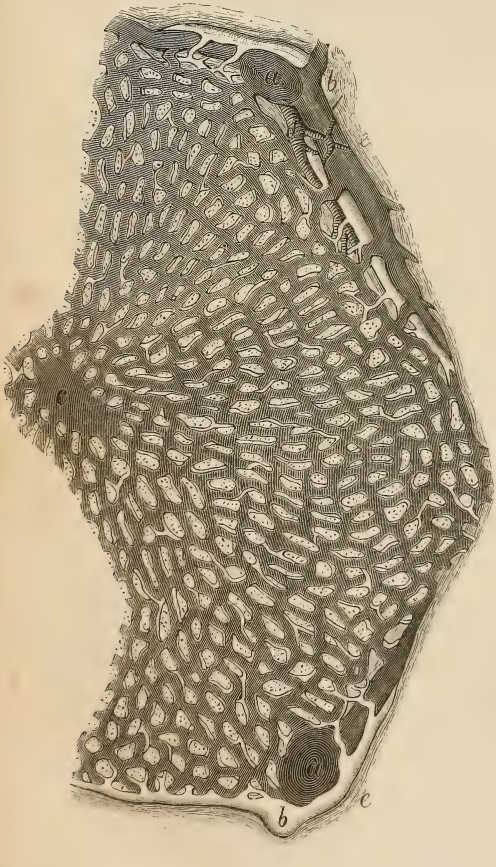


Fig. 3.

Fig. 4.

Fig. 6.

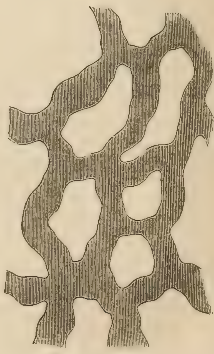


Fig. 5.

Fig. 7.

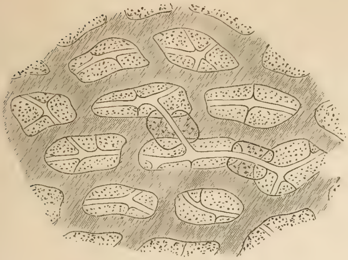


Fig. 8.

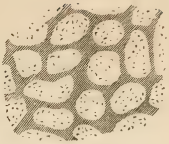


Fig. 9.

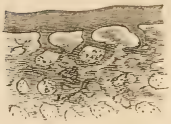


Fig. 10.

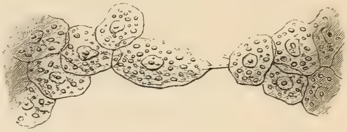


Fig. 11.

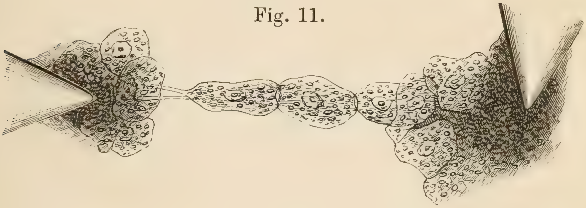


Fig. 12.

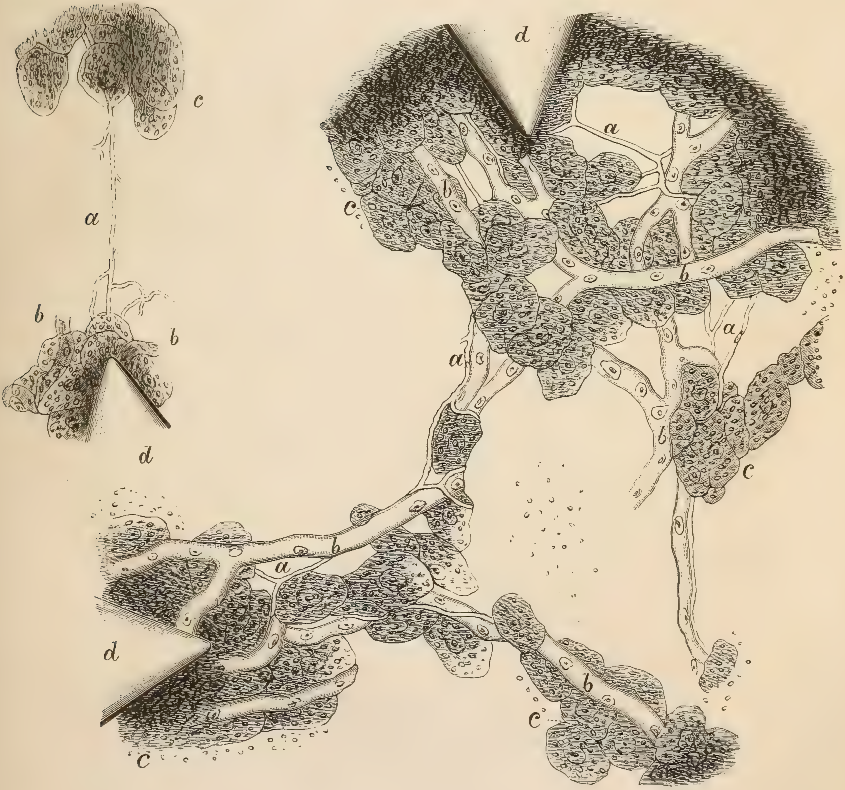


Fig. 13.

Fig. 5. Two cells from the section of

Fig. 6, which represents the outlines of the capillaries in a section treated with potassa. Magnified 266 diameters.

PLATE II. *Fig.* 7. Diagram of an imaginary section of injected liver, as it would look in case of the cells laying in tubes of basement membrane.

Fig. 8. Diagram of a moderate extravasation.

Fig. 9. Diagram of a complete extravasation.

Figs. 10 *and* 11. Liver-cells at the moment of separation, showing the *biliary tubules* between them. 10. Magnified 266 diameters. 11. Magnified 400 diameters.

Fig. 12. *a.* Biliary tubule, with branches still adhering. *b.* Capillaries. *c.* Cells. *d.* Point of a needle of the Microscopic Dissector. Magnified 400 diameters.

Fig. 13. Dissection made with the Microscopic Dissector. *a.* Biliary tubules. *b.* Capillaries. *c.* Cells. *d.* Points of the needles. For the sake of distinction the capillaries are left light. Magnified 400 diameters.

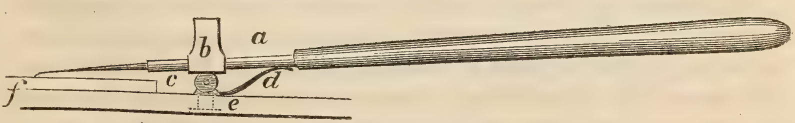
The Microscopic Needle Holder.—About eighteen months ago, when engaged in microscopical researches on the construction of the primitive nerve-fibre, I contrived an instrument which, by serving me as a support for my dissecting needles, enabled me to separate and stretch tissues under the microscope, while, at the same time, my observations with a comparatively high power would be continued. This instrument answered an excellent purpose, by enabling me to put single nerve-fibres on the stretch, but was insufficient for the slow and accurate movement required in some histological investigations. To accomplish this latter I was led to contrive a more complicated instrument, of which I shall speak hereafter. As the construction of the former is very simple, and is thus brought within the reach of every one, engaged in histological studies, I consider it worthy of description.

Figs. 14 and 15 are representations of it, with a slight modification.

It consists of three parts, viz., a needle (*a*) with a handle made of light material; a lever (*b*, *c*, *d*), movable in different directions, to hold the needle, and a brass plate (*e*) in which the lever turns like a pivot; the latter also supports the glass slide (*f*) upon which the preparation is to be placed.

A portion of the needle (Fig. 14, *a*) is cylindrical in order to move very

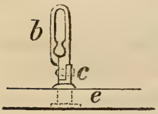
Fig. 14.



smoothly and evenly in the lever (*b*); the forward, backward, and rotary movements being thus effected. The needle part should of course be very fine at its points, and slightly bent, to facilitate dissection. The lever

consists also of two parts. The upper (*b*) acts like a spring, to hold the needle moderately tight in its place, though loose enough to allow the rotary and sliding motions of the cylinder to be effected with ease. In the lower half of this part a round orifice (Fig. 15, *b*), for the reception of the cylinder of the needle, is seen; this orifice is open above and below; the upper part being wider than below, so as to preserve the curved point of the needle from injury in passing through. The lower part of the lever serves as a pivot in performing the rotary motion in the brass plate (*e*). Both parts connected form a hinge-joint (*c*), by means of which the elevation and depression of the point of the needle is effected. Connected with one side of the lower part of the lever is a little feeble spring (Fig. 14, *d*), which, by pressing the handle of the needle upward, keeps the point constantly upon the glass to maintain its hold on the tissue to be dissected, thus allowing the operator to remove his hands from the instrument without disturbing the preparation. This spring should not be strong enough to injure the delicate point of the needle. The preparation may be held down to the glass by a lever with spring like that of the Microscopic Dissector. (Figs. 16 and 19, *b*.)

Fig. 15.



It appears from the works on the microscope and its accessory instruments, among which we may mention those of Quekett and Carpenter, that microscopic dissections had been previously carried on by means of fine needles attached to a handle, and managed only by the hand. The dissections made in this manner could only be performed under a very low power, and would necessarily be very coarse; for the slightest movement of the hand, scarcely observable by the unaided eye, would (under a high microscopic power) make the needle sweep almost over the whole field.

In former investigations it had been customary to separate the tissues finely before examination. But this is only blind dissection, since we are unable by this process to observe the changes going on *during* separation and destruction, and only observe their appearance after the minute structures have been partially destroyed.

To obviate these difficulties, and to carry on my microscopic observations with more accuracy, I contrived an instrument, which I have now in use, and shall now describe.

The Microscopic Dissector.—In the construction of this instrument, the principal object I had in view was, to be enabled to make the slightest motion of microscopic needles, knives, or scissors, in different directions. This can only be accomplished by the screw movement, which also keeps the instruments stationary, and thus gives freedom to the hands for changing object glasses or eye pieces, or applying reagents, while the preparation is undisturbed.

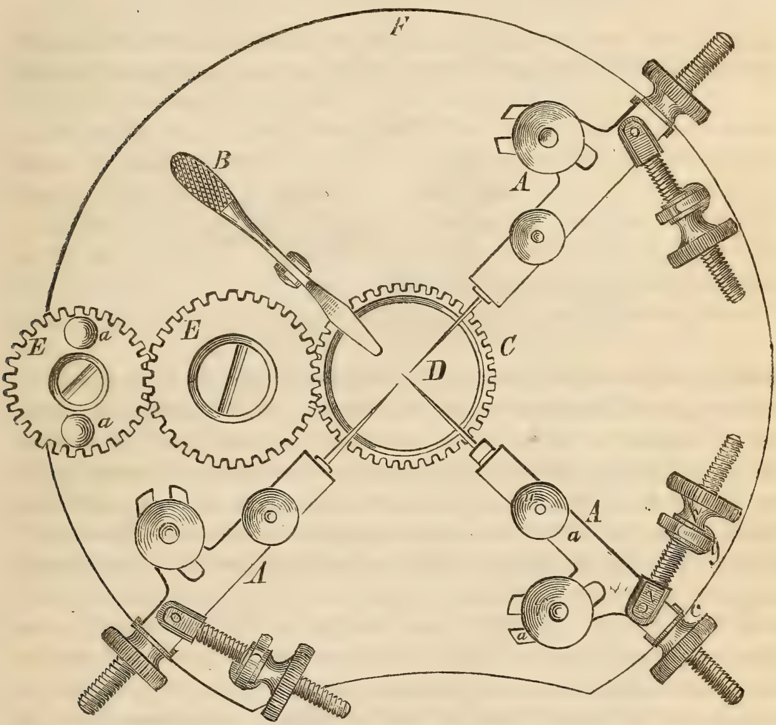
The instrument consists of three levers (*A*, *A*, *A*) to hold the needles,

one lever (B) with spring to fix the preparation to the glass, a brass ring (C) with a shoulder for the glass (D) to rest upon, and two cog-wheels to rotate it. These several pieces are connected with a brass plate (F), upon which they move, and the form and size of which will vary, according to the stage of any particular microscope.

In order to simplify the description of this complicated instrument, I shall speak of these different pieces separately.

The *lever* (A, Figs. 16 and 17) is destined to hold the instruments, and,

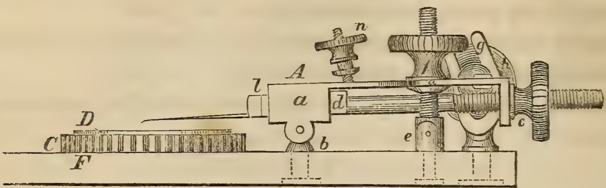
Fig. 16.



therefore, must have motion in all directions. Like the Microscopic Needle Holder, it consists of two principal parts (Fig. 17, a, b), which together form a joint by which the upward and downward motion is effected; the lower one (b) rotates in the brass plate (F) by which the horizontal movement is accomplished. The longer extremity of the upper part (a) is bent in a right angle, and has a round hole in which a nut (Figs. 16, 17, and 18, c) moves. To keep this nut in its place, it has a small notch, which by means of a pin secures the former and prevents it working out. The shorter extremity, or body part (Fig. 17, a), has a square hole, in which the piece (Fig.

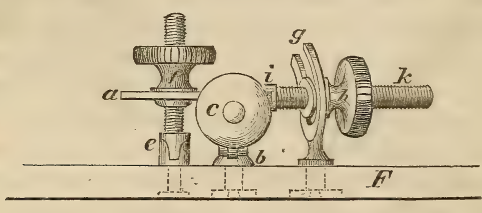
17, *d*), to which the different instruments are screwed, moves forward and backward. One extremity of this piece (*d*) is square, to prevent it from turn-

Fig. 17.



ing, and must work very nicely in the square hole of the lever, for the accurate movements of the point of the needle depend to a great extent upon the

Fig. 18.

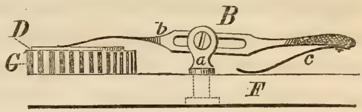


precision of this sliding motion. The other extremity has a fine thread cut on and passes through the nut (*c*), and thus the screw-movement for effecting the advance or retreat of the instrument is established. To produce the up and down movements of the lever, we have another piece (Figs. 17 and 18, *e*) added, which has a joint at its lower part; the upper part has a thread cut on, which forms a screw-movement with the nut (Figs. 16, 17, and 18, *f*). This nut has a notch on one end, in which a lateral prolongation of the lever (*a*) slides (Figs. 16 and 17, *a*, *f*, and Fig. 18, *e*, *a*, *f*, show this plainly). It will be seen that the nut (*f*) being held to the lateral prolongation of the lever (*a*) by means of the notch, will produce the upward and downward movements of the instrument, when turned on the screw of the piece (*e*), as the latter is attached to the brass plate. A third piece is added to effect a sideward movement; this is best seen in Fig. 18, where it is marked by the letter *g*. The lower part of this piece rotates in the brass plate (*F*); the upper one is split like a fork, in which the nut (*h*) turns and slides freely up and down, according to the upward and downward motion of the lever with which it is connected by the piece (*i*). The one extremity of the latter (*i*) is connected by a pin with the longer extremity of the lever (*a*), and thus forms a hinge-joint, while the rest (*k*) has a thread cut on, with which the nut (*h*) forms a screw-movement. If we now look at the lever (*A*) in Fig. 16, we can readily perceive how, by turning the nut (*h*) in one or the other direction,

the distance between the longer extremity (a) of the lever is either increased or diminished, and consequently the point of the needle moved toward one or the other side.

The *lever* (B) for holding the preparation is represented in Figs. 16 and 19. It consists of two parts; the lower one (a) rotates in the brass plate (F); the upper one forms the true lever; it has an oblong opening in the middle, which slides along a screw connected to the former, serving as a fulcrum. One extremity is thin like a spring, and is intended to hold the preparation to the glass, the other has a spring attached to it. By this arrangement the point of the lever can reach the preparation at any place of the glass.

Fig. 19.



The *brass ring* (C) has a shoulder for the glass to rest on; its circumference has teeth to form a cog-wheel, and to be thus revolved by the two other cog-wheels (E, E).

The *glass* (D) is a simple round glass plate, which rests on the shoulder of the brass ring (C). The glass plate fits also to a similar ring of my compressor; so that a preparation previously dissected may be compressed without disturbance, by simply transferring the glass slide on which it rests.

The *cog-wheels* (E, E) are simply held down to the brass plate (F) by screws with a large head, the one has two buttons (Fig. 16, a, a) to turn it.

The *brass plate* (F) has been mentioned already; it may be attached to the stage by means of small clamps.

Besides the dissecting needles, microscopic scissors, forceps, and knives (Figs. 20, 21, and 23), may also be worked by the instrument. The needle

Fig. 20.

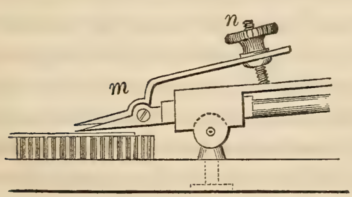


Fig. 21.

Fig. 22.

Fig. 23.

(Fig. 17, l) can be unscrewed, and other instruments substituted, as is seen in Fig. 20. The scissors (Fig. 20, m) are opened and closed by turning the nut (n), the notch of which is embraced and slides on the fork-like handle of the former (Figs. 20 and 22). The forceps are managed in the

same manner. Fig. 23 is a small knife for scraping off epithelium from small ducts, &c., after they have been cut open with the scissors.

The accompanying figures give so accurate a representation of this instrument, as to supply any deficiencies in my description. I have, however, to say that the threads of the screws are not represented as sufficiently close in Fig. 16. To Mr. Gemrig, No. 109 South 8th Street, I am indebted for the accurate construction of the instrument from my drawings, and I can cordially recommend him as one capable of executing any work of the kind. I am also under obligation to his workman, Mr. Wolf, the maker of the instrument, whom I consider the most accurate workman in his line I have yet met with in this country.

I cannot speak too highly of the instrument, for without its assistance, it would have been impossible for me to examine the microscopic structure of the liver so successfully. Yet it is, of course, susceptible of improvement, and by none more likely than my old friend Mr. Lüer, of Paris, whose well established reputation has no need of my indorsement.

The management of the instrument requires a little practice, as the object is reversed in most microscopes. In turning the nut by which the point of the needle is brought toward the glass, much care should be taken to preserve the point from injury, by pressure against the glass. No part of the instrument should be unsteady, while at the same time the motions should be easily effected, that the hand may detect the moment the needle-point touches the glass. The whole success of the manipulation depends on the accuracy of these movements. The point of the needle must be *very* fine; this is best accomplished on an Arkansas hone, and by the aid of a magnifying-glass of a low power. Sometimes, even with the greatest care and precaution, the point will break. To be obliged to apply to an instrument-maker to adjust it each time, would occasion much inconvenience. The investigator himself may obviate this difficulty, by annealing the needle first in the flame of a spirit-lamp, and afterwards bending the point, and then hardening and tempering it again. There is an inconvenience connected with the instrument, which, though slight, compared with its advantages, is scarcely noticeable, viz., when the point of the needle enters the liquid in which the tissues are dissected, the motion produced disturbs the rays of light, and confuses the appearance of the object; but this lasts only until the point of the needle has fairly entered the liquid and touched the tissue, when all will be as clear as before. The best liquid, therefore, to keep the tissues moist, will be one of a low specific gravity, which will allow the point of the needle to enter, without itself being too much disturbed. Water answers this purpose better than alcohol or turpentine, which evaporate too quickly. Glycerine, which is an excellent medium for the examination of tissues in other cases, is of too high a specific gravity for this purpose. The tissue should not have more liquid above it than is necessary for moisture, to prevent disturbance of the rays of light.

In using the No. 3 object-glass of my microscope (which is one of Nachet's), after the tissue is dissected, and properly adjusted for a favourable observation, it has been my custom to fill up the interspace between the tissue and the lens with water, or diluted alcohol, thus preventing any disturbance of the rays of light in passing through it, and also affording ample time before the evaporation of the liquid, for making any drawing.

The advantages and disadvantages incident to this, might be enlarged upon, but the operator's judgment and mechanical skill will readily suggest remedies for its defects; to its advantages, I can give my testimony, having used it without any difficulty. To manage the instrument successfully, delicacy of touch and a great deal of patience are required; but it is only by the latter, combined with perseverance, energy, and close observations, that scientific facts have, or ever will be, established.

Apparatus for making Microscopic Sections of Tissue.—This apparatus, by means of which I have made hundreds of the finest microscopic sections of various tissues and organs, is of even greater utility than the "Dissector." I designed it with the object of obtaining fine sections of the spinal marrow, or brain-matter, to aid in my researches on the nerve-structures. It seems worthy of remark, that at that time, in my references to the writings of several eminent investigators, I found the razor (which they considered the sharpest instrument), the only one regarded as most suitable for this purpose. This erroneous idea is readily accounted for; the razor having a very thick back, is more readily brought to a fine edge than any other knife. The reason is obvious; in honing a knife, the object is to remove the rough wire edge produced by the grindstone and polishing-wheel, and give to the blade another, smoother, and infinitely finer. Now, to do this, the nearer level we can render the two sides of the knife, the finer the edge. Or, in other words, the blade of the knife must form the same angle with the surface of the hone at each stroke, or the edge becomes round, and consequently dull. In honing the razor, less difficulty is experienced. The thick back will form an angle of the proper degree, when it rests on the surface of the hone, while, with another knife, much practice is necessary to preserve the same angle at each stroke. But the advantage of a keen edge which we obtain by using the razor, is counterbalanced by the clumsiness and thickness of the instrument in penetrating between the very fine slice of tissue, and the piece from which it is cut. Now, to avoid all these disadvantages, I have my knife, which is thin, arranged in such a manner as to enable every one to hone it with great facility; but I shall speak of this hereafter. Valentine's knife seemed to me useless for delicate, soft structures; it may do well enough for cartilaginous tissue; but even then, if the edge of one blade is not as keen as that of the other, it can never make a good section. As I have never used the knife, however, I can express no positive opinion about it.

In fact, it is almost impossible to make fine and uniform microscopic sections of the spinal marrow, of any considerable size, unless the knife be guided, by sliding over some smooth surface.

To accomplish this object, I at first cut a hole the exact shape of a transverse section of spinal marrow, in a small piece of thin board. Putting a piece of the former (previously hardened), through this hole, so as to project a little above the surface, I could slide a sharp scalpel over the surface, and cut fine sections comparatively easily. But there was still some trouble by the adhesion of the slice to the knife, which was remedied by cutting under water.

The *principal portion* of this apparatus (Fig. 24) consists of one plate upon which the pieces, destined to guide the knife blade move; a second

Fig. 24.

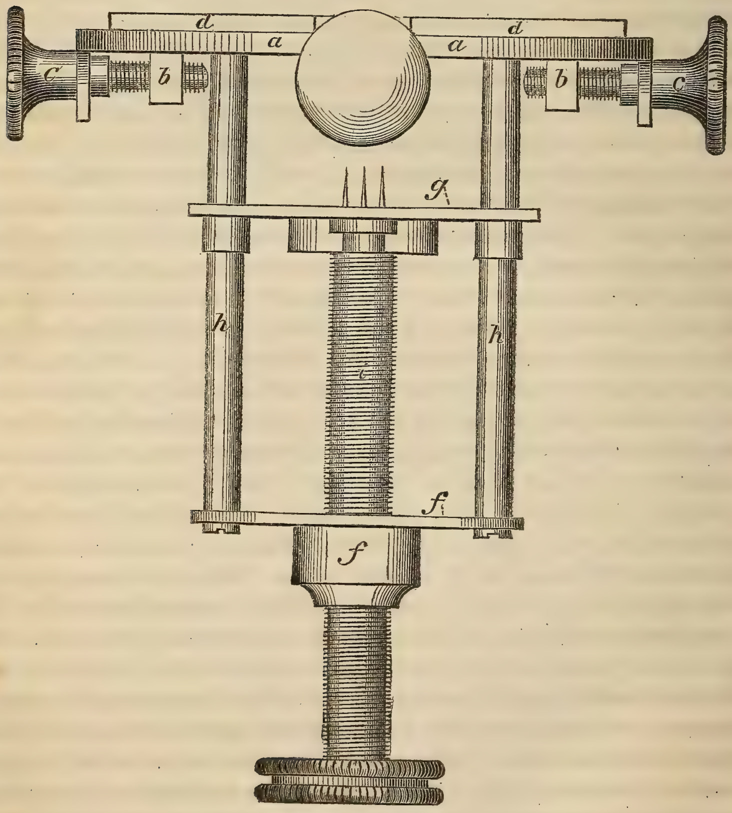
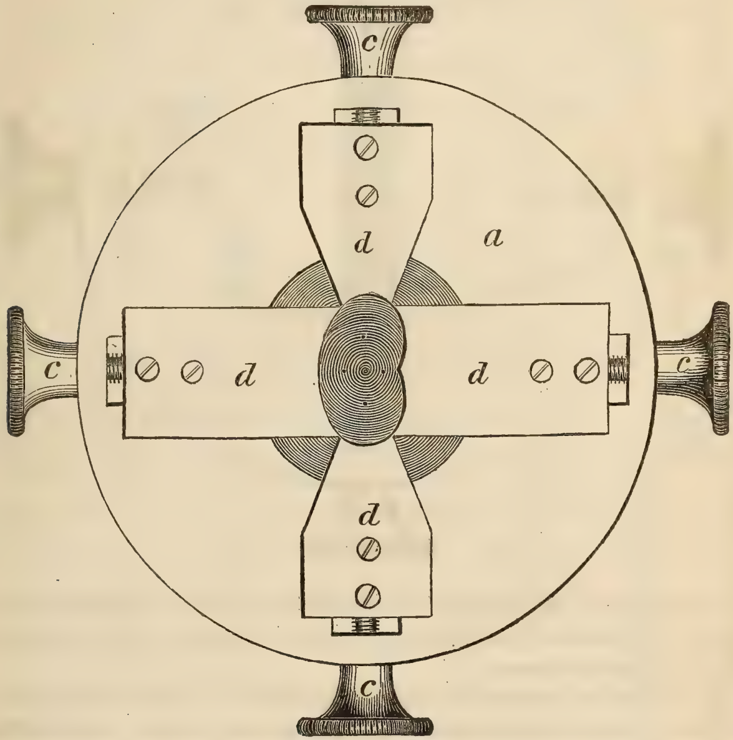


plate destined to hold the tissue to be cut; and a third one, in which the screw that regulates the thickness of the slices moves.

The plate (Figs. 24, 25, and 26, *a*) or stage of the instrument is round,

has a round orifice in the centre, and four others between the centre and the periphery. In each of these peripheral orifices a piece (Fig. 26, *b*) is made to slide forward or backward, by means of a screw (Figs. 24, 25, and 26, *c*) which turns in the plate (*a*). To the piece (*b*) can be attached smaller plates (*d* and *e*), which are intended to press against the sides of the spinal marrow, or other tissues, to hold them when cut; the surface of these plates (*d* and *e*) serves to guide the knife blade; by referring to the drawing, it will be noticed how they are attached to the pieces (*b*) by screws; it will also be observed how these plates can either be made to

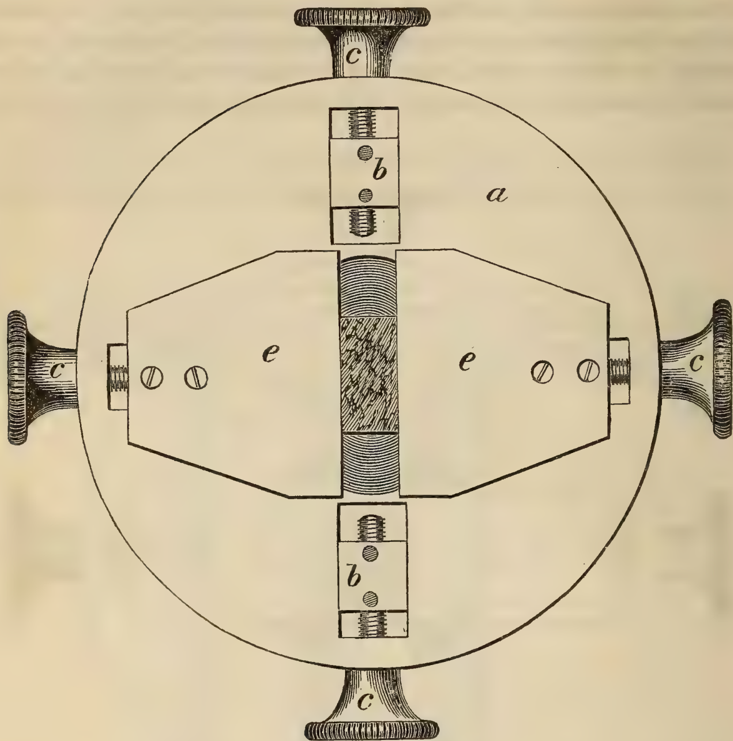
Fig. 25.



advance or recede by turning the screws (*c*). By this movement the preparation is held or loosened. The plates (*d*) in Fig. 25, as can be seen by their shape, are suited to hold a piece of spinal marrow; those in Fig. 26 (*e*) have their inner margins parallel to hold other tissues, as liver, kidney, &c. The plate (*f*), by means of four cylinders, is attached to the plate (*a*); these cylinders (*h*) assist in the accurate sliding movement of another plate (*g*) upon which the preparation rests; three sharp points can be seen upon this plate to prevent the preparation from moving sideways. A

screw (i), intended to move the plate (g), works in the plate (f), but by a notch is attached in such a manner to the plate (g), that it may turn freely in the latter. Now, when the screw (i) is turned, it will move the plate

Fig. 26.



(g), upon which the preparation rests, either upward or downward, and thus the thickness of the sections to be cut is regulated. The instrument should be made of brass.

As the cutting under water is one of the principal points in making good sections, I have a box of sheet tin, about one foot square, which is filled with water, and in which the instrument rests horizontally upon brass supports soldered to the bottom of the box.

As the spinal marrow varies in shape and thickness at different places in its course, there should be different sets of plates adapted to each portion. For the spinal marrow of small animals, two plates like Fig. 27 will be sufficient.

This instrument has been made to my satisfaction by Mr. C. Mannel, No. 704 Arch Street.

To make fine sections of nerves, an arrangement like Fig. 28 will an-

swer, which is a flat piece of wood, $\frac{3}{16}$ of an inch thick, with a little spring on each side to hold the extremities of the nerve. In making fine

Fig. 27.

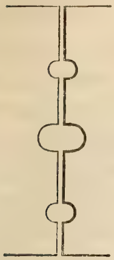


Fig. 28.

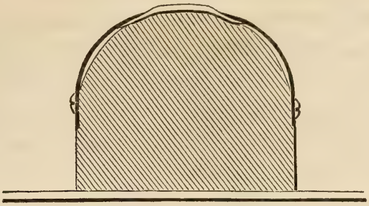


Fig. 31.

Fig. 29.

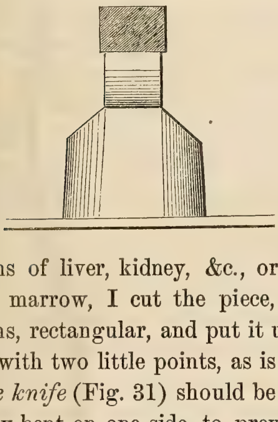
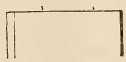


Fig. 30.



sections of liver, kidney, &c., or longitudinal sections of spinal marrow, I cut the piece, which shall furnish the sections, rectangular, and put it upon a piece of cork, provided with two little points, as is seen in Figs. 29 and 30.

The knife (Fig. 31) should be thin and very sharp, and slightly bent on one side, to prevent interference with the screw buttons; its back (a) is arranged to be slipped on, for the process of honing, and off when used for cutting. The cutting should be done from below upward, and by a scarcely perceptible sawing motion; if the knife is drawn only in one direction, the section will tear. If the section is well made, it ought to be thin enough to read fine print through it. In cutting spinal marrow, where four plates (d) to guide the knife and hold the preparation are required, care must be taken not to touch the angles of the plates with the edge of the knife in passing; for this purpose, those angles should be rounded off a little. After the section is cut and floating on the water, it is then caught fairly on a spatula, while yet under water. It is scarcely necessary to give more particulars in regard to the use of the instruments, the operator will soon discover them.

The Mounting Forceps (Figs. 32 and 33) was invented by me for holding the covering glass to the glass slide, when wiping and cementing its edges. It consists of a small forceps, to the jaws of which four little

Fig. 32.

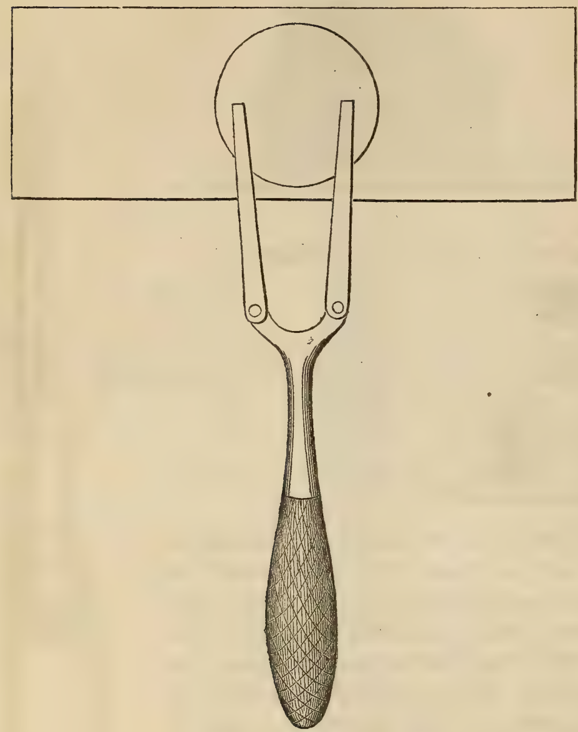
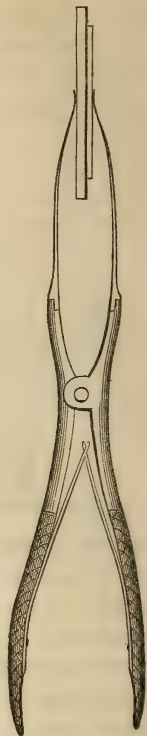


Fig. 33.



springs are attached, forming a movable joint; thus the distance between the extremities of the springs can be regulated, according to the size of the covering glass; the forceps is kept shut by a spring between its handles. This instrument will be found very useful for mounting microscopic preparations.

ART. II.—*Remarks on Sunstroke.* By JAMES J. LEVICK, M. D., of Philadelphia.

UNTIL a comparatively recent period there had been but little written on the subject of sunstroke, the name popularly given to those sudden attacks of loss of consciousness, with laboured respiration and prostration, which occur in such numbers during the summer season.

This circumstance may be attributed to two distinct causes: first, that, though of frequent occurrence with us, sunstroke is rare in England. It was from the mother country that, until comparatively lately, we derived most of our medical literature, and, as intimated, in England the affection is almost unknown. Dr. WATSON, in a brief allusion to the subject, states that he has never seen a case of the kind; and many English writers seem to ignore the existence of such an affection, by omitting any allusion to it in their treatises on the practice of medicine. What is known of it by our brethren in Great Britain appears to have been learned from the experience of the surgeons in India, where its effects are so fearfully felt. A second cause may be found in a mistaken classification, many cases of what would seem to have been genuine cases of sunstroke having been erroneously regarded as apoplexy, acute congestion, and inflammation of the brain.

And yet sunstroke has had its victims from the earliest times. In the sacred volume we have two instances recorded. The first, that of the son of the Shunammite, who, being "in the field with the reapers, said unto his father, 'My head, my head!' And when he had taken him to his mother, he sat on her knees till noon, and then died." (2 Kings iv. 19.) And again in the text given by Dr. Watson: "And Manasses was her husband, of her tribe and kindred, who died in the barley harvest; for as he stood in the field, overseeing them, and bound sheaves, the sun came upon his head, and he died in his bed in the city of Bethulia."

In looking over the various numbers of the *American Journal of the Medical Sciences*, from its commencement to the present time, I find twenty-four volumes to have been issued without a single paper on this subject, or even an incidental reference to the disease, as such. The first allusion to it is a short paragraph above the initials of "T. R. B.," which appeared in vol. i., N. S., p. 536, published in 1841, and which is as follows: "At this season, in 1821, the British frigate Liverpool was proceeding from Muscat to Bushire; the weather gradually increased in warmth; double awnings were spread, the decks kept constantly wetted, and every precaution used to prevent exposure of her men; yet in one day, from a species of *coup de soleil*, she lost three lieutenants and thirty men. If, for however brief a period, they exposed themselves to the sun, they were struck down senseless; vertigo followed, accompanied by foaming at the mouth." It is added: "The frigate's deck at one time resembled a slaughterhouse, so numerous were the bleeding patients." (*Wellsted's Travels to the City of the Caliphs.*) It will be remembered that the Persian Gulf intervenes between Bushire and Muscat, the latitude being about 25°.

In the same journal—vol. xviii., N. S., p. 66, 1849—Dr. H. Hartshorne reports four cases as treated at the Pennsylvania Hospital, two of which died.

In vol. xxi. p. 183, quoted from the *Transactions of the College of Physicians*, is a highly interesting paper by Dr. PEPPER on the subject, in

which it is stated that "during the seven years preceding, twenty patients had been treated in the Pennsylvania Hospital for sunstroke; of these, *ten* died, *seven* recovered, and *three* became affected with a chronic affection of the brain, eventuating in insanity." Added to the paper of Dr. Pepper are some valuable remarks by Dr. CONDIE on the same subject. In the same volume, page 536, is an able article by Dr. LENTE, then Resident Surgeon at the New York Hospital. In this it is stated that in the year 1847 there were *thirty-seven* cases of sunstroke occurring in the city of New York *in four days*, most of whom died so promptly that it was impossible to convey them to the hospital. Of *forty-two* admitted into the hospital during five years, *twenty-four* died and *eighteen* survived. In the "Transactions of the Boston Society for Medical Improvement"—reported in vol. xxvii., 1854—several cases are recorded; and in the last published number of the *Journal* some brief remarks are offered on the same subject by the writer of this paper.

In the *Philadelphia Medical Examiner*, vol. i., N. S., p. 526, Dr. DOWLER, of New Orleans, in the course of some experimental observations on animal temperature, incidentally alludes to the subject of insolation. After expressing his surprise that so little distinction is made between *solar asphyxia*, as he calls it, and *solar syncope*, he says "solar asphyxia is always fatal;" but adds that this might be curable if the patient were bled *instantaneously*. He states that on the 23d of July, 1845—the thermometer being 105° to 150° in the sun—fifteen victims perished in one day in the city of New Orleans from sunstroke.

Essays on this subject have at various times appeared in the New York journals. One of the most comprehensive of these is that of Dr. Dowler, published in the *New York Medical Gazette*, July, 1851, and republished in the *New Orleans Medical and Surgical Journal*, January, 1856, page 470. Dr. ISRAEL MOSES records some interesting cases in the *New York Medical and Surgical Reporter* for the year 1846.

One of the most instructive and complete essays which have come under my notice in our recent journals is that of Dr. H. H. SWIFT, then Resident Physician of the New York Hospital, and published in the *New York Journal of Medicine*, vol. xiii., N. S., p. 45, 1854. I shall have occasion frequently to allude to this paper, and cannot avoid here stating that it enters so fully into the subject, that the results of the observations made by the author, and the inferences thus drawn and expressed, correspond so closely with my own, as to leave but little unsaid that I could wish to say. After describing the three different stages of the affection, as he considers them, Dr. S. states that of *one hundred and fifty* cases admitted to the New York Hospital, *seventy-eight* died.

In the *Buffalo Medical Journal* for 1856 is a paper by Dr. S. B. HUNT on sunstroke, having especial reference to its exciting causes. Allusions to the subject may be found in the writings of Drs. WOOD, GERHARD, DICK-

SON, CARTWRIGHT, and others of our own countrymen. As I have already intimated, but little has been written on this subject by the English. MOSELY, in a treatise on tropical diseases, published in London A. D. 1803, page 57, says: "In England, in 1707, on the 8th of July, many people at work, and many horses and oxen, were killed by the sun's rays; and in Pekin, in the year 1743, between the 14th and 25th of July, *eleven thousand* people perished in the streets from the same cause!" In the *Medical Gazette*, vol. xviii. p. 71, is a paper by Mr. RUSSEL, of the 73d regiment, then stationed at Madras, which, so far as I am aware, is the first which gives the true pathological condition found after death from sunstroke. The *Medical Times and Gazette*, 1857, page 416, contains in a paper on the diseases of the brain and nervous system, as they affect soldiers, &c., in India, by CHARLES ALEXANDER GORDON, surgeon to the 10th regiment, in which is given a brief notice of sunstroke as it occurs in India. Out of twenty-eight cases coming under this writer's notice, *but one* recovered. The latest—and, so far as I know, the best trans-Atlantic publication appeared a few months since in vol. xv., N. S., p. 624, of the same journal, the author of which, Dr. BEATSON, a surgeon in the army of India, employs Dr. Gordon's paper just referred to as a text for his subject, rejecting altogether the pathological and therapeutical views of Dr. Gordon.

As respects the frequency and fearful character of this affection, the preceding notices are of themselves sufficient evidence. Thirty-seven cases of sunstroke in four days, almost immediately fatal, as reported by Dr. LENTE, fifteen deaths from this cause in one day, as recorded by Dr. DOWLER, certainly present the matter in a sufficiently serious light, without further facts on my part; and yet the scenes I have myself witnessed, though never so fearfully numerous as those above recorded, have impressed me even more forcibly than this frightful catalogue alone could have done. In most other diseases the victim has some forewarning of his doom. When pestilence stalks the streets, men are on the fearful watch for its approach, and are prepared, at least for a time, to combat the destroyer; if they fall, it is with a broken sword, and with their armor pierced. But sunstroke gives no such warning. It strikes down its victim with his full armor on. Youth, health, and strength oppose no obstacle to its power; nay, it would seem, in some instances, to seek out such as these, as if boldly to flaunt its power, and in the very glare of day to deal its fatal blow. Those who have witnessed the scenes of domestic distress which every summer affords us, as husbands and fathers, who, but a few hours before, had left their families in full health, are brought back to them struck down by death, will not think me disposed unnecessarily to deepen the picture. As I have elsewhere said, I know no place other than the field of battle where such sudden destruction of human life occurs.

The *symptoms* of sunstroke are now too well known with us, at least, to render a detailed description of them necessary. Dr. Beatson gives

three symptoms as the prominent ones in his cases, viz.: *unconsciousness, laboured respiration*, and *extreme prostration;* these are unquestionably the conditions we meet with in sunstroke; but similar phenomena present themselves in other affections. When, however, an individual, and still more so when a number of persons in ordinary health exposed to a temperature approaching 90° F. in the shade, are suddenly seized with these symptoms, for a moment perhaps preceded by thirst, vertigo, and a confused perception of colours, there can be little doubt that the attack is that of sunstroke.

The appearances described by different writers are very dissimilar. By some the pupil is found contracted, as in one case described by Russel (*loc. cit.*); so, too, by Hartshorne; while, on the other hand, Drs. Pepper, Lente, and Swift (*loc. cit.*), speak of dilatation of the pupil as of most frequent occurrence. Dr. Swift, however, adds that in what he calls the third stage, the pupils are often firmly contracted. "Of 33 fatal cases, the pupils were *contracted* in 20, moderately dilated in 7, and markedly so in 6; in the successful ones the pupils were *dilated* in 19, nearly natural in 15." (Swift, *loc. cit.*) This corresponds with what I have myself noticed. The probability is that the condition of the iris varies with different degrees of the affection, and is by no means a pathognomonic feature of the case.

The pulse, too, is differently described; by some as full and slow, by others as frequent, feeble, and thready; by Dr. Strong, of Boston, as in some cases "very strong;" by Dr. Coale, of the same society, as "very small." Dr. Dowler says: "The pulse is hurried, hobbling, and unequal, very often gaseous or air-like, but never slow, hard, and large, as in apoplexy." I have always found the pulse a feeble one; it is true, in some instances, a full one, but easily compressed, and frequent.

Coma and stertor are recorded by some as of almost constant occurrence, by others they are not mentioned. I have seen in some instances violent convulsions, while in the greater number of cases they have been absent. The truth is, as I have already intimated, that the symptoms vary with the difference in the intensity of the affection, and though it has not occurred to me to notice in so decided a manner the three stages described by Dr. Swift, yet I am quite prepared to adopt his classification. Dr. Dowler (*loc. cit.*) makes "a particular arrangement or classification of solar diseases, which is highly important in a practical point of view." He "submits the following: 1st. Solar Exhaustion or Syncope. 2d. Solar or Sun-pain. 3d. Solar Excitement or Inflammation. 4th. Solar Asphyxia."

"Solar exhaustion," he says, "differs from solar asphyxia both in symptoms and treatment. In solar asphyxia, the skin is extremely hot and generally dry; there is a choking sensation, and a total loss of sense. In solar exhaustion, the skin is moist, pale, and cool; the breathing is easy, though hurried; the pulse is small and soft; the vital forces fall into a

temporary collapse, the senses remaining entire. Horizontal position, free air in the shade, external stimulants and frictions, are usually sufficient to restore the patient." The extensive experience of Dr. Dowler and his well known habits of accurate observation, give great weight to whatever suggestion he may make on this or any other subject; but, though certainly existing to some extent, I must acknowledge that the distinction between the different forms of solar disease has not been so well defined in the cases coming under my notice as in those seen by him.

The *cause* of this affection is suggested by the name popularly adopted, and though by no means of scientific origin, the term sunstroke or heat-stroke, so long as we remain uncertain as to the exact pathological condition, may as well be retained.

While exposure to the direct heat of the sun is by far the most frequent cause of the affection, it is by no means the only one. Dr. Swift mentions eleven cases as occurring in a laundry, and several others in a sugar refinery; and the first case seen by me during the past summer was that of a man who had been at work for ten hours in a sugar refinery.

What is the kind of heat, and under what circumstances is it that these disastrous effects are produced? Almost all writers agree in classing among the causes which predispose to this result, fatigue, intemperance, improper clothing, being clad, as Dr. Dowler expresses it, "in garments which afford but a feeble resistance to the conduction of heat into the body." Dr. Beatson (*loc. cit.*) states that "it seldom occurs except after fatigue and exhaustion, under a tropical sun, aggravated, no doubt, by whatever tends to obstruct the freedom of the venous circulation of head and neck, and to produce such obstruction, I know of nothing—short of hanging—more effectual than the old leather stock and tight coatee collar." He might have added, anything which compresses the chest or waist, and thus interferes with the free expansion of the lungs. A case of this kind occurred in front of my office, on the occasion of the military parade in celebration of the laying of the Atlantic Telegraph Cable. A robust German soldier fell over in a state of unconsciousness, with laboured respiration, followed by a slight convulsion. I found him with a cravat fitting almost to strangulation, and with a belt so tightly encompassing the waist as to completely prevent the free and healthful expansion of the thorax and its contents; nor was it until all these ligatures had been removed that the favourable symptoms which terminated in recovery presented themselves.

While accepting the above as frequent auxiliaries in the development of sunstroke, my own observations by no means agree with those who believe it occurs chiefly to such as are addicted to the intemperate use of intoxicating drinks. Such was not the case with the patients treated in our hospital last summer, nor with those reported by Dr. Swift. In India it attacks most frequently soldiers whose military duties prevent such indulgence. So too with the sailors before reported. It is true, that scarcely

a summer passes that cases of unconsciousness from intoxication are not brought to the hospital as cases of sunstroke, which, at the first glance, might be so considered. Sunstroke is by no means confined to the intemperate; indeed, I am much inclined to believe that a moderate amount of stimulant might act as a prophylactic. *Unacclimation* I believe to be a strong predisposing cause. With one exception, all the men treated in the Pennsylvania Hospital during the past summer were foreigners. *Position* acts as a favouring cause; as in stooping or bending forward. This was illustrated in the cases occurring among labourers employed in working with pickaxes in making the passenger railways in our city during the late summer; in two cases related by Dr. Coale, of Boston, of sailors attacked while bringing a ship to anchor; as well as in the many instances of day labourers, most of whom are in this position while at work.

But none of these circumstances is necessary for the production of the disease, and I have seen it attack with equal fatality the strong and vigorous as the enfeebled and poorly nourished. I have never as yet met with sunstroke affecting a negro, though told that such cases are not uncommon in the South; and the natives of India are, it is well known, not exempt from it.

What is the peculiar character of the heat which produces these effects?

Had this question been asked me before this paper was begun, I should have replied, from a vague impression made on my mind, that sunstroke was most frequent during an intensely dry heat. Such, however, does not accord with the observations of others, and such, I am inclined to think, from a more careful investigation, is not the case.

Dr. SANDFORD B. HUNT, in a paper on Sunstroke published in the *Buffalo Medical Journal,* advances the opinion, that *a high humidity* of the atmosphere is the condition most favorable for sunstroke. He quotes, among other cases to sustain this view, the eleven instances related by Dr. Swift as occurring in a laundry, the atmosphere of which, he asserts, was doubtless surcharged with aqueous vapour from the dampened linen. A somewhat similar condition of the atmosphere may be supposed to exist in a sugar refinery. In all cases coming under his notice, he found a high dew-point contemporaneous, and he ascribes the fatal results to the absence of the refrigerating effects of free evaporation from the surface. During three days of last summer, in which six cases of sunstroke were brought to the hospital, and in which many other cases occurred, the maximum temperature in the shade was 94° F, the barometer about 30, and *the dew-point* at 9 A. M. of each day was 74, 72, 72—a high range even when we regard the temperature, of which the record in the shade can give but an imperfect conception. In the very first case quoted by me, that of Wellsted in the Persian Gulf, it is said that the *decks were kept constantly wetted,* which, if the above view be correct, must have increased the number of fatal cases. It is true, that RUSSELL (*cit. supra*) reports his cases as

occurring "at a season when the hot land winds had just set in, rendering the atmosphere dry and suffocating;" but, on the other hand, Dr. GORDON, writing from India, says: "India officers say the heat is not so much to be feared during the intensity of the dry heat, as in those calm sultry days when the sun is obscured by a film of clouds, or by impalpable dust, at seasons diffused through the atmosphere, these being the frequent precursors of an earthquake." Dr. BEATSON reports as the condition of the company to which he was attached, that they remained for three days and two nights located on a dry open rice flat without tents, exposed to and utterly unprotected from the blazing sun by day *and the heavy dews by night.* From a remark made by Dr. DOWLER (*loc. cit.*), I infer that his views correspond with those of Dr. HUNT. The latter correspond, too, as I shall hereafter show, with the occurrence of what are described by the older writers as predisposing causes of apoplexy. Though contrary to my own preconceived notions on the subject, I think it but fair to repeat that these views appear to be held by the majority of careful observers, and are well sustained by them.

Before calling attention to the pathological appearances and nature of the disease called Sunstroke, I wish for a few moments to recur to a statement made by me in the early part of this paper, that the paucity of medical literature on this subject, among the earlier writers, is to be accounted for on the supposition that many cases described as other diseases were really and would now be recognized as cases of sunstroke. Not to mention the cases of "death from cold water," which, I believe, are now generally admitted to have been instances of this kind, I am quite convinced that many cases of insolation have been regarded and treated as cerebral apoplexy.

Andral, in his *Clinique Médicale*, when describing various forms and causes of cerebral congestion, has the following:[1]—

"Three labourers occupied in three different places in getting in the harvest during days when Reaumur's thermometer marked 40° (122 F.) in the sun, died suddenly. The circumstances accompanying these three deaths could only be ascertained in two of them, for the third was found dead. According to eyewitnesses, the two former could not have left their work more than five minutes before their death. They turned round, putting their hands forward, as if they had been deprived of sight and must have expired at the moment when they wished merely to sit down. The individual who died first, that is, on the 6th of July, was a man of mature age, but putrefaction made such rapid progress that it was impossible to keep his body till the proper time for examining it. The second died on the following day. It was a woman twenty-one years of age. Her body was examined—muscles well-developed; all the articulations were completely rigid; on the back and face were livid spots, and already the odour of putrefaction began to manifest itself; abdomen tympanitic, smooth, and free from spots. * * * No effusion was found between the dura mater and bones of the cranium, nor was any found over or under the pia mater, but the veins and arteries of these two membranes were gorged with blood. The brain presented

[1] *Vide* Medical Clinic, or Reports of Cases by G. Andral, Professor, &c., translated by D. Spillan, M. D., &c. Bell's Medical Library, vol. viii. p. 199 *et seq.*

no irregularity, only its substance was very soft; the ventricles contained a little more serum than natural; * * * no signs of inflammation of the lungs or pleura, nor any effusion; * * * the pulmonary vessels were gorged with blood, and the bronchi filled with frothy mucus; * * * size of the heart natural; right ventricle a little distended and filled with liquid black blood; the left ventricle contracted and empty. On opening the abdomen, an infectious odour was diffused; * * * the uterus and ovaries contained a little liquid blood, and in the cavity of the pelvis were found an effusion of two ounces of blood. The third person died suddenly on the 8th July. This was a stranger, a woman between forty-eight and fifty years old. We proceeded on the following day in the morning to examine her body. * * * All the articulations were rigid; the back was traversed by blue spots; almost the entire face was covered with them; the face from the chin to the nose was absolutely livid; several were also to be seen on the chest, the size of which varied from that of a lentil to that of a twenty sous piece. These spots had precisely the appearance of petechiæ, and yielded when cut into some liquid blood. The body was still warm, but exhaled a fetid odour; the abdomen was tympanitic; the integuments and bones of the cranium presented nothing extraordinary; their vessels, as well as those of the brain, contained some liquid blood; the cerebral substance was extraordinarily softened; the lateral ventricles contained a bloody serum. The pericardium presented a slightly inflammatory tint on its inner surface; the right ventricle of the heart contained a little black liquid blood; the blood of the left ventricle was red and frothy, &c. &c."

A little further on, in the same treatise, the author has the following paragraph:—

"Baglivi, in 1694, and Lancisi, in 1705, saw apoplexy suddenly become so common in certain parts of Italy that they have described it as having been in those years really epidemic."

I have looked into these old authors, and find in the former the following quaint and interesting statement:—

"For these last two years (I mean 1694 and 1695), the Apoplexy has swept off great numbers of People both in this city and all over Italy, and the fatal effects of this Disease were so frequent that they alarmed even the healthful with apprehensions of sudden Death. The Cause of this Epidemick Apoplexy is justly imputed to the unusual Constitution of the Weather in those years. The Summer of 1693 was so extream hot and scorching, which was followed by nipping cold Weather, in the beginning of 1694, and, contrary to the Custom of Italy, with a rigid Frost, Snow, and Ice. * * * The Summer after that was much hotter than the former, insomuch that for five Months together there was no Rain. About the beginning of *October* it began to be wet weather, and continued such, with a Southerly Wind to April, 1695, and that to such a degree that so much Rain was not seen to fall at one time within the memory of Man. After fifteen days of continued Rain, perhaps we had two days clear Weather, which were followed by heavier Rains than went before. After this uncustomary Weather, the abovementioned Apoplexies display'd their Rage; and perhaps some part of the Epidemick illness was owing to the Universal Grief and Domestick Care, occasioned by such calamitous times; all *Europe* being at the same time engaged in a sharp War, the like of which has scarce been heard of since the Foundations of the Universe were laid. So many Cities were raz'd and burnt; so many thousands of Men slain; all Commerce disturbed, and the Avenues of Peace block'd up, that the strongest heart could not bear the Thoughts of it."[1]

[1] "The Practice of Physick reduced to the Ancient way of Observations. Containing a Just Parallel between the wisdom and experience of the Ancients and the Hypotheses of Modern Physicians, &c. &c. Written in Latine by Geo. Baglivi, M. D., Professor of Physick and Anatomy at Rome. Printed for Andr. Bell (*et al.*). London, 1704."

Further on he mentions that numerous earthquakes prevailed at this time, that many people were pinched with famine, and that the plague invaded one of the smaller towns shortly before.

While among the ancients, I may be permitted to quote from old FORESTUS,[1] who, in a series of observations on apoplexy (which for careful investigation and accuracy of description, may well put to the blush more modern writers), has this sentence: "And we have ourself seen, in the year 1562, when the condition of the atmosphere (*constitutio æris*) was humid and rainy, *and the south wind had existed for a long time*, very many persons to have perished by apoplexy." He then describes a number of cases under his care, mentions certain streets and villages as especially affected, and designates many of the attacks as *mild apoplexy* (*levis apoplexia*), from which recovery speedily took place. He recommends, among other remedies, stimulating frictions, and speaks of bleeding as unnecessary in many of these cases. Hillary,[2] in a work on tropical diseases, states that apoplexies are of frequent occurrence in Barbadoes in the month of July.

Now, I do not for one moment suppose, that all the cases thus recorded as those of apoplexy, not even all those designated as "*levis apoplexia*," were cases of sunstroke, the pathological researches of Forestus show the contrary; but I cannot forbear calling attention to the fact, that the circumstances attending their development were closely analogous to those which are mentioned by modern writers as attending and as producing sunstroke—"the south wind long existing;" the humid atmosphere as given by Forestus; the long hot and dry followed by the moist atmosphere; the conditions which precede (as before quoted from Gordon), and which resulted in earthquakes, as related by Baglivi; the mental distress prevalent in the community at the time, all these find their appropriate place, and accurately fit in the picture already given as descriptive of what we now call by the varied names of sunstroke, insolation; exhaustion from heat. Might we not without violence thus explain what otherwise it is difficult to explain, an *epidemic* of apoplexy? Descriptions similar to those already given, I find in an old copy of Lancisi,[3] who recommends the preparations of iron in the debility following such cases.

To return to the nature and *post-mortem* appearances of sunstroke, the first fact which meets us, and which very much interferes with a satisfactory investigation, is the rapidity with which putrefaction occurs. In two fatal cases which came under my notice during last summer, decomposition had occurred ten hours after death to such an extent as to render a *post-mortem* examination impossible. The exterior of the bodies was of a deep livid

[1] DOMINI PETRI FORESTI, M. D., Observationum et Curationum Medicinalium ac Chirurgicarum opera omnia. Francofurti, 1634.

[2] By Wm. Hillary, with notes by Benjamin Rush, M. D. Philad., 1811.

[3] Jo. M. Lancisii, M. D., Opera omnia in unum Congesta. Venetiis, 1739.

colour, horribly offensive discharges had taken place from the mouth and nostrils, and the surface imparted to the hand the soft boggy sensation so characteristic of decomposition.

Until the appearance of Mr. RUSSELL'S paper (*vide supra*) under the impression that the disease was allied to, if not identical with apoplexy, the attention of necroscopists appears to have been directed exclusively to the brain. Andral, in describing his cases of cerebral congestion, before quoted, gives a faithful description of the appearances now recognized as existing after sunstroke. Though meant to illustrate cerebral disease as produced by heat, with his usual accuracy, he states that with the exception of engorgement of the vessels of the meninges, there was no diseased condition of the brain other than softening, while the pulmonary vessels were equally as much engorged as those of the dura and pia mater. Russell (*loc cit.*), much to his surprise, found no lesion of the brain, though he had been taught to expect it. He states, "the brain was in all healthy; no congestion or accumulation of blood was observable, a very small quantity of serum under the base of one, but in all three *the lungs were congested even to blackness through their entire extent*, and so densely loaded were they that complete obstruction must have taken place. There was also an accumulation of blood in the right side of the heart, and the great vessels approaching it."

Dr. GERHARD had before this time noticed a similar condition of things. In his clinical lectures (Graves & Gerhard) p. 677, he says: "During the intensely hot weather of the summer of 1830, I witnessed the opening of thirty persons who died from the effects of heat; we found no organic disease of the brain, but merely a slight congestion, such as is observed in other acute diseases, which it would be idle to set down as a cause of death." Dr. PEPPER in his examinations (many of which I witnessed), found that "the brain exhibited no indications of congestion, nothing, in fact, of an unusual appearance. But in all of the four subjects the heart was pallid, flaccid, and softened, while the other muscles of the body were florid and firm. The lining membrane of the heart and of the large bloodvessels was of very dark, almost purple colour. The cavities of the heart contained but little blood and no coagulum." Similar appearances are reported by Drs. Lente and Swift. Dr. Dowler found "intense congestion of the lungs, and believes the cause of death, secondarily, perhaps, to be here." All these observers agree in representing the blood as fluid, and in a letter from Dr. B. DARRACH, now Resident Physician New York Hospital, describing the inspection of a fatal case occurring in the hospital last summer, he has written "the most interesting and remarkable appearance was the frothy fluid condition of the blood, and the distension of the heart with gas." (See following article.)

Now, no intelligent medical observer can witness, or read of these phenomena, without being struck with their resemblance, I had almost said

their identity with those presented in deaths from typhus fever. Take any of our standard authorities, Wood or Gerhard, *e. g.*, and compare their descriptions of the pathological appearances of typhus with those above given.

Post-mortem Appearances in Typhus Fever.	*Post-mortem Appearances in Sunstroke.*
A strong tendency to speedy putrefaction.—WOOD.	Speedy decomposition after death.—DOWLER.
Petechial eruption a characteristic lesion.—WOOD.	Face and neck covered with livid spots.—DOWLER. Petechiæ.—ANDRAL.
Blood found in the veins liquid; coagula rare. Within the cranium there is often venous congestion, with some serous effusion in the ventricles or beneath the arachnoid; and the substance of the brain is occasionally darker than in health, though in other cases unaltered or even whiter. No clearly ascertained connection exists betweeen the stupour and the anatomical appearance in the brain.—WOOD.	Slight congestion such as occurs in other acute disease; no organic disease of the brain.—GERHARD. Brain healthy; a very small quantity of serous effusion in some cases; venous congestion; brain healthy, substance soft.—ANDRAL. In a case of stupour and delirium no lesion of the brain found.—SWIFT.
The posterior and lower part of the lungs is often solidified; of a dark red colour, fragile, and impervious to air, though without the granular appearance of hepatization.—WOOD.	Lungs dark and injected with blood; the central and posterior parts presenting the appearance of an enormous clot of blood, somewhat solid, and united by a texture bearing no resemblance to the elastic, contractile texture of healthy lung.—DOWLER.
The heart sometimes natural, sometimes softened, sometimes containing liquid blood.—WOOD.	Heart soft.—PEPPER. " natural.—LENTE. Heart filled with liquid blood.—ANDRAL, et al.

And so too with the incidental lesions of other viscera.

If we carry our investigations further back, we shall find an astonishing identity in the symptoms presented during life. Each has a frequent and feeble pulse, each may or may not have a hot, pungent skin, each has its stupor deepening or not into coma, each its suffusion of face and eyes, its lividity of countenance, and we need but use the words given as descriptive of typhus, if we would describe some of the phenomena of sunstroke. "Now and then great restlessness and jactitation occur, with twitching in various parts of the body;" while, as if to complete the picture, I find a case of sunstroke reported in which epistaxis, a frequent attendant of typhus fever, existed.

These facts would seem to indicate that there is in sunstroke as there is in typhus fever a poisoned condition of the blood, and that it is to this that we are to refer the various morbid phenomena of the disease, an explanation which has also been given in cases of death from lightning. I cannot, however, divest my mind of the belief that in sunstroke this unnatural condition of the blood is a secondary affection, not the primary one; that there is in the first place an exhaustion or depression of the nerve-forces which regulate nutrition, respiration, circulation, and the other acts of organic life. A

condition to some extent analogous to this we have in congestive or pernicious fever, and perhaps also in cholera, though in neither of these diseases have we proof that the blood is poisoned. What are the various circumstances required to develop these results, and what the exact order in which they are produced we cannot positively say. We know so little of the nature of the vital force in its healthy condition that we can expect to know but little of the cause of deviations from the healthy standard. We can speculate, and we may draw our inferences as to the treatment, and watch for its results. If we adopt the theory recently proposed with much plausibility by Dr. DAVEY, that the so-called ganglionic system affects directly or indirectly all varieties of abnormal action, we must of course look to it for the first link in the chain of these phenomena. The vital force, place it where we will, is so affected by the modification of temperature, of meteoric or other condition of the atmosphere, that the conservative or regulating influence and supply of proper nerve power is lost in part or in whole. Hence result feeble action of the heart, imperfect respiration, stases of blood in the lungs, in the brain, loss of vitality of the blood, and the tendency evinced even during life, and still more in death, to rapid putrefaction. Just as we see, suggests Davey, under local depressing influences, slowly produced, fatty, amyloid, and other degenerations. Similar phenomena occur at either extremity of the thermometrical scale. Baron LARREY reports, as the result of the retreat from Moscow when the depressing influences of defeat were added to those of fatigue and of cold, that many perished from dizziness, vertigo, somnolence, ending in profound coma, and death. (Andral, *loc. cit.*)

If we would remedy these things we must direct our attention to the condition of system producing them, not to the mere phenomena themselves. We must not rely on local measures to relieve the congestion of the lungs or of the brain, but just as we would do in typhus and pernicious fevers, we must remedy the evil producing these congestions. That there may be congestions we do not doubt, but in most instances they are *passive* congestions—a stasis of blood from want of the usual motive power. *Post-mortem* inspections in all these diseases show us this, and death is often produced by a treatment based upon different views as to their nature. And yet bleeding has been the treatment practised by many even to the present time. Not to mention those of earlier date, Dr. GORDON, as late as last year, reporting 28 cases, only one of which recovered, seems to have bled every case. The solitary survivor is said to have undergone the following heroic treatment: "*Slight cautery to the nape of the neck, opening of the right temporal artery*, and, while the patient lay unconscious, a powerful jet of water was directed on the head and epigastrium, croton oil being exhibited internally!" Without quoting separately each author, let me here state that nearly all our American writers agree that bleeding, so far from being of advantage, generally hastens a fatal termination. Dr. Beat-

son (*loc. cit.*), after giving a fearful history of the results of bleeding in such cases, says of three cases treated by him all recovered. His plan of treatment was "to unfasten as quickly as possible the man's dress and accoutrements, to expose the neck and chest, get him under the shade of a bush, raise his head a little, and commence the affusion of cold water from a sheepskin bag, continuing the affusion, at intervals, over head, chest, and epigastrium, until consciousness and the power of swallowing returned. When this takes place the affusion may be stopped, and a stimulant mixture given occasionally in small doses."

I believe this to be, with some modifications, the treatment called for. In typhus and in pernicious fevers, we find cold affusions to have been long used, and with advantage; but, as in them so in sunstroke, I can readily conceive there may be instances in which the powers of life may be so spent, as to fail to respond to the shock of the cold, and the existing depression be thus increased; nor in any case should they be persevered in after the skin begins to assume a moist and healthy condition. So, too, in regard to the use of ice to the head, which is not to be resorted to indiscriminately. Where the individual can readily swallow, recourse may at once be had to stimulants, of which ammonia, in some form, is the one most frequently used. The carbonate of ammonia, in gr. v—gr. x doses, and the aromatic spirit, have been used by me with advantage. In the *New York Journal of Medicine* for 1854, p. 58, Dr. J. R. Leaming urges the use of muriate of ammonia in eight-grain doses every half hour. The cases are not very fully reported, and the pulse is described as *slow* and weak—a condition I have never seen in sunstroke. Dr. Dowler quotes from a German medical journal what purports to be "a most efficacious remedy in *coup de soleil*: acetic ether dissolved in wine vinegar, to be applied as a lotion on the forehead, temples, cheeks, hands, and chest, and the same to be given every half hour with coffee."[1]

Unless swallowing can be readily accomplished, I think it best to avoid the exhibition of remedies by the mouth, as, passing into the trachea, they often occasion unpleasant coughing and distress. In the cases mentioned

[1] Since the above was written, I have seen an interesting paper in the *London Lancet*, by J. R. Taylor, Esq., Deputy Inspector of Hospitals, on "Erethismus Tropicus," the name given by the writer "to the state of system which exhibits the acute effects of continued high tropical temperature on the European." Some of the cases therein recorded presented the ordinary phenomena of sunstroke, while in others the symptoms seem to have been modified by an epidemic of cholera which had preceded, and, as I believe, left its impress on the subsequent epidemic. The reports of these cases coincide in many respects with those I have given. The subjects of the attack were "men of remarkably stout build," but *unacclimated*. The pathological appearances differ but little from those given above, and the author of the paper recommends as the most effective treatment, cold affusions and the avoidance of venesection as a general rule. (See *London Lancet* for August 21—28, 1858.)

by me at the College of Physicians, I ventured to suggest that there might be a special value in the oil of turpentine, whether given by the mouth or, as in the cases there reported, by the rectum. I would again mention this remedy, and strongly urge its trial. As a stimulating enema, it has long been used, but it may be more than this; and the results in the cases alluded to were, to say the least, gratifying. It was prescribed in doses of f℥j of oil of turpentine, in a half pint of liquid, every half hour, until re-action began. Of the value of oil of turpentine in typhus and other malignant fevers, in scurvy and other altered conditions of the blood, there is strong evidence. In typhoid fever, we all know it to be useful, and the results of the cases treated recently in the hospital confirm me in the belief, that in sunstroke, which, as we have seen, resembles typhus fever in so many of its features, it is also of especial value. Nor is it a new remedy, even in cases regarded as apoplexy. Forestus, before quoted, in his cases of mild apoplexy which recovered, used, as one of his most efficient agents, the oil of turpentine. I may here quote from the *Transactions* of the College the following passages:—

"If one principle were more insisted on than another in the treatment of these cases, it was the importance of avoiding any expenditure of the little remaining strength of the patient. Though recommended in such cases, the full bath is not without danger. The chief indication for its use is found in the stimulus afforded by the *hot* bath—for which, in practice, the *warm* bath is often substituted—the depressing influence of which may fatally exhaust the forces which barely maintain the balance between life and death. * * During this time, the patient should have his feet placed in a strong mustard foot bath, but without any exertion on his part. When able to swallow, brandy, wine whey, or, as before mentioned, ammonia may be given him."

In a recent visit to the New York Hospital, I was told by Dr. B. Darrach, one of the resident physicians, that, to relieve the burning heat of the skin—which is justly viewed as a most unfavourable prognostic symptom—it had occurred to him that frictions of the body *with ice* might be useful, and that he had carried out the suggestion with very happy results.

After the immediate symptoms of an attack have passed, as after cholera, typhus fever, &c., so with sunstroke, various internal inflammations may be developed—as of the brain, the lungs, &c.—which require a treatment entirely unlike that mentioned above. In this way, we have cases reported as meningitis resulting from exposure to heat; but, according to my observation, these differ from sunstroke in their gradual mode of attack, although just as apoplexy may occur in warm weather, so may meningitis; but in by far the greater number of cases of sudden attacks of illness from exposure to heat, inflammation of the brain is not the pathological condition. Nor must we be misled by the presence of delirium, or of a hot skin, in such cases. The latter we have present even in malignant typhus, and no fact

is better known to the pathologist than this: that, in typhus and typhoid fever, phenomena almost identical in character with those of inflammation of the brain may be present, and yet, after death, no lesion of this organ be found. In the words of Dr. Bartlett, when writing of typhus fever, "Delirium and somnolence have occurred as frequently, and as strongly marked, in patients whose brains presented no changes, as in those of an opposite character."

OCTOBER 14, 1858.

ART. III.—*Cases of Exhaustion from Heat treated in the New York Hospital by Stimulants and Ice to the entire Surface of the Body.* Reported by B. DARRACH, M. D., Resident Physician.

CASE I. Daniel Murphy, æt. 29, native of Ireland, labourer, was admitted August 14, 1857; brought by the police from a drug-store, without any history of his attack. Comatose; skin burning hot, but moist; respiration 40 per minute, stertorous, laboured, and sighing; pulse 160, and feeble; pupils contracted, and insensible to light; eyes congested; has great difficulty in deglutition; vomited before admission. Ordered an enema of carbonate of ammonia and brandy, ice to the head, and sinapisms to the chest, abdomen, and thighs. While under this treatment, the intense heat of skin seemed to indicate some attempt to reduce it. Ice was accordingly applied to the body, at first cautiously. In half an hour he vomited some green fluid, and had a tetanic convulsion of the muscles on the back of the neck. Pupils dilating, and show signs of sensibility. Ice applied more freely, being rubbed over the whole body and down the arms and legs. Sinapisms removed. Three hours after admission he had a violent convulsion; pupils dilated, and sensible to light; the heat of skin still kept down by ice.

Aug. 15. The ice was applied at short intervals through the night. Patient was conscious this morning; but now, 11 A. M., is semi-comatose. Pupils contracted; skin comfortable; tongue dry and brown, with sordes on the teeth; pulse 108 to 120, feeble. Treatment suspended early this morning.

16*th.* Yesterday afternoon he was able to give his name and age, but spoke so indistinctly that nothing else could be understood. Has an idiotic expression of countenance.

20*th.* Notices himself a difficulty in his articulation, though it is very much improved. Says that on the day he was taken sick he had felt bad all day; had drank four or five glasses of brandy, and freely of cold water.

28*th.* Still speaks indistinctly, but is otherwise perfectly well. Discharged.

CASE II. Marie Hass, æt. 19, German emigrant just arrived, admitted June 26, 1858. Had been running about all day. Was said to have had a slight attack of sunstroke in the morning. Ate a hearty dinner, and drank freely of beer and ice-water. Was taken suddenly sick on the Erie Railroad Pier about 3 P. M., and brought to the hospital about three hours afterwards. On admission she was insensible, with violent convulsions; skin

burning hot, and pungent; pulse 135, small; pupils contracted; respiration hurried, but easy; vomiting. Ordered ice and iced brandy freely; carbonate of ammonia, grs. v, every quarter of an hour; to be rubbed from head to foot with ice constantly; ice kept to the head. 7 P. M. Pulse 116; respiration 36; skin cool while the ice is applied, but becomes hot as soon as it is stopped; convulsions less. Continue treatment. Fell asleep at 7½ P. M. Respiration easy; pulse much less frequent. Treatment suspended. Woke in a convulsion half an hour afterwards, but slept immediately after it, and was not disturbed through the night.

June 27. Pulse natural. Appears and feels well. Wants to go. Discharged.

CASE III. Unknown man, apparently a German baker, æt. about 22, admitted June 28, 1858. Was taken suddenly in a store in Seventeenth St., nearly two miles from the hospital. Admitted three hours and a quarter after. Insensible; pupils contracted; pulse 150, small and feeble; respiration 34, laboured and sighing; skin hot and pungent; has sinapisms on his feet, wrists, and abdomen; tongue hot; unable to swallow. Gave two ounces of brandy by stomach-pump; rubbed the surface with ice from head to foot; ice to the head. Forty minutes after his face became livid, with irregular respiration, dilated pupils, then a short struggle and death.

CASE IV. Mary Duffy, æt. 40, native of Ireland, married, admitted June 29. Distressed looking, ragged creature. As ascertained afterwards, she had been complaining for a week, but kept at her work as washerwoman; and to-day, though suffering a pain in the stomach, took a long walk in the sun. She drank freely of water, and once of gin. Was taken with blindness about the middle of the afternoon, while in the street. When brought in by the police she was so far insensible as to be unable to answer, but repeated the question until asked another. Pulse 160; respiration 40, sighing, but not very laboured; skin hot. Took brandy ℥vj in the course of two hours, and was rubbed with ice as the others, keeping the skin cool. An hour after admission she could tell her name and express her gratitude. Pulse 112; respiration 18; surface cool. Complained of pain in the head; relieved by ice. Had some spasmodic respiration for a few minutes, but afterwards became natural.

June 30. Slept well; feels sore this morning.

July 2. Feels well; gaining strength. Has a constant tremor of the eyes.

10*th*. Eyes well. Discharged.

Remarks.—Of all the cases—four in number—treated in the hospital during the last two summers, by stimulants, sinapisms to the extremities and body, and ice to the head, none were more grave on admission than those recorded above, and none recovered. Dr. H. S. Swift, in a paper published in the *New York Journal of Medicine*, vol. ii., 1854, gives an account of sixty cases which occurred in the hospital under his care, of which thirty-three died. He has also collected the cases in the hospital for about thirty years back, and found 150, of which 78 died. None of the cases admitted under my observation were of a mild form, the first stage of Dr. Swift. Two of those which recovered had contracted pupils, a uniformly fatal symptom with the doctor. In the other recovery the pupils

were not noted. In *Braithwaite's Retrospect*, vol. xxxvii., page 363, is a paper from the pen of Dr. Beatson, army surgeon in India, whose treatment, as given there, was affusion of cold water to the head, chest, and epigastrium, with moderate stimulation; all ligatures about the neck being removed, and the patient, if possible, carried to the shade. He was uniformly successful in eight or ten instances, in all of which the treatment appears to have been applied immediately. In my own cases the ice was rubbed over the surface until it became cool and the pulse diminished in frequency. Of those that recovered, it was continued on one of them, with little or no interruption, until she slept, a period of one hour and a half; in another an hour, when she was able to give a correct account of herself, and the pulse was reduced from 160 to 120 beats per minute; in the other it was continued at intervals for twelve hours. Any increase of the heat of skin or permanent frequency of pulse being considered an indication for the ice. All the cases of the past season were admitted after 3 P. M., and after the daily visit of the Attending Physician, Dr. Joseph M. Smith, though the treatment met his subsequent approval. Case I. was treated by Dr. Thos. B. Dash, then Resident Physician, at the suggestion of the writer.

NEW YORK HOSPITAL, September 17, 1858.

ART. IV.—*The Use and Abuse of Uterine Tents.*

By HORATIO R. STORER, M. D., of Boston.

EXPANSIBLE tents, for dilating the os and cervix uteri, though of comparatively recent introduction into this country, are already somewhat extensively in use. Like all other obstetric agents, they are still too often neglected; and, on the other hand, like the speculum, the caustic, the pessary, they are too often misemployed. In consequence of these facts, and in the hope, by elucidating one point of medical responsibility, directly to save human life, the present paper has been written.

Uterine tents, when expansible, are intended for the following purposes:—

I. To induce premature labour;
II. To assist accidental abortion;
III. To aid or to hasten the progress of labour at the full time;
IV. To expose the uterine cavity for purposes of diagnosis or treatment, in diseases puerperal; and,
V. In diseases non-puerperal.

In each of these classes I propose briefly to consider their use, and in each their abuse.

I. *The Induction of Premature Labour.*—Tents for this end, first suggested by Kluge and Brunninghausen, long used on the continent of Europe, and in 1844 introduced into Great Britain by Simpson, have their advantages and their disadvantages. They are comparatively speedy in their action; this action is in imitation of natural processes, both as regards dilatation, and in keeping the membranes entire; they are unattended with much risk to the life either of mother or child.

On the other hand, their introduction is not always easy, more especially if pregnancy be near its close, and the os not readily reached; their action is often accompanied by much uneasiness and severe pain, entirely unconnected with that from uterine contractions; their rapid decomposition, if of sponge, may predispose to or occasion puerperal disease.

They must be compared with other means, to show their real and relative value.

All so-called oxytoxic medicines, not even excepting ergot, are unreliable or dangerous, either to mother or child.

External manipulations or frictions, forced exercise, the use of electricity or galvanism, mammary irritation, are alike unscientific; if they succeed, as is by no means always the case, they are alike dangerous. They would excite contractions of the uterus before any dilatation of its orifice has taken place, or any detachment of the membranes from its walls; processes preliminarily essential to safe and normal labour. The tendency of such premature contractions is, as is shown by innumerable cases on record, to destroy the fœtus, by cerebral effusion from long-continued pressure, and to destroy the parent, by rupture of the uterus, or by hemorrhage attending or subsequent to delivery, or by the wear and tear of a lingering labour.

The objections now alleged apply with even more force to puncturing the membranes, the process formerly and probably still most frequently resorted to. Here, it is true, there is generally some slight dilatation from the passage of the instrument used, whatever this may be. But the supplementary aid of the wedge-like bag of water is lost; and the pains, more quickly as more certainly occurring, find the uterus and its contents equally unprepared. Statistics from eminent practice prove the great mortality, both fœtal and maternal, attending this method.

Dilating the os and detaching the membranes by the injection of water, whether direct or secondary in action, has also its objections. By the first of these processes, I mean where the fluid at once and directly enters the os; and by the second, its reception and retention by the vagina, acting on the os, not, as has been supposed, by relaxation from imbibition, but only by pressure from below: cruel, because extremely painful; unadvisable, because tedious and uncertain; unjustifiable, because endangering laceration of the vaginal attachments to the cervix. The direct douche is liable to cause fœtal rotation and malpresentation, is tedious and must generally be repeated a number of times, and involves as much and even more danger

than the subsequent method, of premature detachment of the placenta and serious hemorrhage.

Compared with the above operations, tents are best. They should, however, be preceded in use by some slender staff, sound, or catheter, introduced to detach the membranes, too early separation of the placenta being guarded against by careful auscultation.

Wherever, in these cases, the tent does not seem absolutely necessary, its use should be dispensed with. If required, it should be as accompanying the process last described, the two mutually aiding and reacting upon each other. These points are already familiar to the profession; they are here recapitulated, as bearing upon a question too often lost sight of, that of *medical responsibility.*

If the premature labour, actually an abortion, and attended, in proportion to its period, with the general risks of such—is not absolutely indicated to save one or other of the two lives at stake, it becomes criminal; and accountable, morally and legally, as such.

II. *The Assistance of Spontaneous Abortion.*—We have above alluded to the abuse of tents in inducing intentional abortion.

If this, however, be accidental, or from natural causes; if, having commenced, its progress seems unavoidable; preventive measures having failed, or being out of the question from the presence of pressing and dangerous symptoms, while the uterus undilated cannot discharge the ovum, it may become not merely justifiable but imperative to use tents. In this instance, they may act not only as a mechanical dilator, but also as an uterine plug or tampon, restraining hemorrhage by its own backward pressure. The state of things, in early abortion particularly, being very different from that in labour at the full time, little danger is thus incurred; the uterine cavity small, and its walls unyielding, its tissue turgid, and thereby the entrance to the Fallopian tubes closed.

III. *The Assistance of Timely Labour.*—In but a single instance are tents likely to be of aid, after a labour at the full time has fairly commenced; and this, not forgetting its complications with convulsions, placenta prævia, and the like, where there is excessive rigidity of the os uteri. If pains are strong, however, it is almost impossible to introduce, or to keep in position, a tent of any size; and if they are not strong, it is often a better practice, unless some complication is present or threatened, to delay for a time instead of hastening the progress of the labour. In almost every instance, other irritation—direct, by digital expansion or stimulating applications, provoking increased discharge of mucus, or sympathetic, as from enemata, especially of tartrate of antimony[1]—is of much more marked and immediate

[1] The above treatment, first practised in this country by the writer (*Boston Med. and Surg. Journal*, Feb. 1856, p. 38), has since been successfully followed in very many reported cases.

avail. Better often, by judicious interference, to subject a patient to its trifling risk, than by delay to let her incur the proportionately increasing one in such cases usually obtaining; and this despite any advice that may be offered to the contrary.[1]

IV. *Dilatation in Puerperal Disease.*—Hemorrhage subsequent to labour, and continuing for several days, over-abundant or extremely fetid lochia, accompanied, perhaps, by excessive after-pains, are not uncommon; and if the discharges are profuse, they become at once alarming and dangerous. They may be owing to retention of a fragment of the membranes or placenta, loose or adherent, or to that of a blood clot; much more rarely to persistent want of general uterine contractility, or to a true secondary hemorrhage from the wound left by detachment of the placenta. In the instances referred to, the retained foreign body, whatever it be, is sometimes thrown off by nature unaided, at other times not.

A better practice than to exhibit drugs or throw up injections, is to dilate by tents, and then, if necessary, remove whatever may be found by long and slender forceps. Three cases have now been treated in this manner by the writer, two of them in consultation, all of them successfully.

In other instances, acute or chronic, of malpractice or accident, as where the head or other fœtal fragment may have been left in the uterus; or of simple disease, as of retention, in whole or in part, of the product of conception, whether as mole, hydatids, skeleton, or mummy, the applicability of tents is no less evident.

V. *Dilatation in Diseases unconnected with Child-bed.*—Except during delivery, and for a short time immediately subsequent to it, and in a few cases of organic disease, attended with shortening or loss of the cervix and abnormal laxity of the os, the diseases of the interior of the uterus, of its walls, and of the lining membrane, were, till 1844, utterly beyond every power of diagnosis, and of treatment, unless empirical. The evidence, only afforded by functional or reflex symptoms, or by morbid discharges, often equally misleading, was wholly insufficient for the one; the use of uterine injections, the only topical application then possible, was too blind, and, therefore, too dangerous for the other.

At the present time, however, the cavity of the uterus has been brought by tents almost as completely within reach as its cervix, and I repeat even more emphatically than formerly, that as "without the sound no man can satisfactorily diagnose some of the diseases of women, without the tent there are others that no man can cure."[2]

Intra-uterine malignant disease, polypi, sessile tumours, are thus brought under control, and local applications, medicinal or otherwise, understandingly made. In its proper place resort may be had to various instru-

[1] As for instance, in this Journal, Oct. 1858: "Evils of Meddlesome Midwifery."
[2] Preface to Simpson's Obstetric Works, 1855.

mental aid, by the curette, knife, ligature, simple or in the double-action ecraseur, applied by Drs. Savage[1] and Priestley,[2] of London; or to powerful caustics, as the hydrate of potash, proposed by myself,[3] and put into successful practice;[4] used of course, all of them, with extreme care and circumspection.

In one rare class of cases, vesico-uterine fistulæ, tents serve the diagnosis, not by their dilating power, but as a plug; retaining the urine till, by an appreciable quantity, its escape is made to show its source. They may, perhaps, ultimately render possible some method of treatment. That tents for uterine dilatation, as generally made of sponge, are attended with certain risks, unless of the softest and finest quality, evenly shaped, and in use cautiously watched, and that a substitute for this material, of equal efficacy, would be a valuable acquisition to practice, I have elsewhere endeavoured to show.[5] Of slippery elm, then proposed, use has been made in mechanical dysmenorrhœa, with the advantage claimed and expected; but in other cases the desideratum remains unsupplied. Suggestion of metallic dilators, in many forms, has been made by an ingenious friend, Dr. Graham Weir, of Edinburgh; but in practice they have been found useless, irritating, dangerous.

It might seem superfluous to add a caution, lest by tents abortion be accidentally and unintentionally induced. Two cases in the practice of friends, however, have satisfied me that the risks are much greater than they might seem. Upon this point I shall speak more fully in another connection,[6] and here merely state, as the safer rule, that since it is absolutely impossible to be sure of the absence of pregnancy in the earlier months, and since an abortion thus produced is not merely a loss to society in itself, but also in far greater proportion as encouraging others by seeming carelessness of fœtal life, tents should not be used, or the uterus otherwise disturbed, where the woman is at all liable to pregnancy by marriage or other chance, till the short time sufficient to establish the diagnosis has been allowed to elapse. Of course cases of hemorrhage, of an os partly open, or occupied by a foreign and protruding mass, afford evident exceptions to the rule now proposed.

To sum up then this subject, it may be stated that to produce premature labour and necessary abortion, to assist accidental abortion and the progress of labour at the full time, to aid or rather to render possible the

[1] Lancet, Nov. 1857, p. 524.

[2] Med. Times and Gazette, Jan. 1858, p. 115.

[3] Bost. Med. and Surg. Journal, July, 1856, p. 500.

[4] Ibid., Nov. 1857, p. 288, and Oct. 14, 1858.

[5] Association Med. Journal, London, May, 1855, p. 446; Boston Med. and Surg. Journal, Nov. 1855.

[6] Contributions to Obstetric Jurisprudence. No. 1, Criminal Abortion. N. A. Med.-Chir. Rev., Jan. 1858.

diagnosis and treatment of various forms of puerperal and of non-puerperal disease, the use of uterine tents become not only justifiable, but at times imperative; that when resorted to carelessly, rashly, or for a criminal purpose, their use becomes an abuse; and that when being indicated, they are knowingly neglected or omitted, and in consequence, as still not unfrequently happens, a human life lost, the physician in charge directly becomes, and should be considered responsible therefor.

CHESTER PARK, 25 Oct. 1858.

ART. V.—*Remarks on some Affections of the Spinal Column.*

By JOHN H. PACKARD, M. D., of Philadelphia.

THERE are several reasons for the paucity of our definite knowledge in regard to the diseases and injuries of the spinal column; one of the chief of which is that many of those affections are either so easily reached by ordinary remedies, or so hopeless from the outset, that their careful investigation seems a matter of no moment to those who have them in charge. Moreover, *post-mortem* examinations in these cases are attended with more difficulty, and require more laborious and careful dissection, than almost any other; nor is it always easy to obtain permission to make them.

The vertebræ, from the atlas to the coccyx inclusive, are to a great degree protected from the ordinary forms of violence; and this is due not only to their form, connections, and situation within a muscular mass, but also to the fact that in the falls which constitute the most common source of injury to them, the head and extremities are very apt to exhaust the force of the shock. But there is a variety of indirect violence not mentioned in the books, which is nevertheless of some importance; I allude to powerful pressure at or near each extremity of the chain of bones. The following case will serve to illustrate its effects upon the false vertebræ.

A coal-miner was sitting upon a large piece of coal, and bending forward to his work, when a mass was detached just over his head, and came down upon him. The force thus brought to bear was immense, and its results were in proportion; the sacrum was fractured transversely as well as longitudinally, and its lower extremity was comminuted, as was also the coccyx; the right sacro-iliac symphysis was forced open; the horizontal ramus of the pubis of each side, and the ascending ramus of the ischium of each side, were fractured. There was also a fracture of the left tibia, and a complete rupture of the urethra.

When force of this kind acts upon the true vertebræ only, it may have a very singular effect. A young man, 17 years of age, was admitted into the Pennsylvania Hospital, in September, 1855; he had been sitting upon a

log, beneath a staging upon which there were a good many people, when the staging gave way. His spinal column had thus been subjected to great force at each extremity. When brought to the hospital, he was much collapsed, and suffering extreme pain; his back presented a striking prominence at about the eleventh dorsal vertebra. The pain extended all around his body; neither the sensibility nor the motions of his lower limbs were impaired. He was laid in bed upon his right side; reaction having occurred, counter-irritation and diaphoretics were employed, and his bladder emptied once by means of a catheter. No bad symptom ensued; a week after his accident, he was able to stand up, holding by a chair; and he gradually gained strength, although the deformity of his back remained. At the end of six weeks he was well enough to be discharged.

Now, what was the lesion in this case? The eleventh dorsal vertebra formed a very marked projection backwards; or, to speak more correctly, the spinous process of that bone constituted the apex of the angle made between the upper and lower portions of the vertebral column. There could not have been any great degree of compression of the cord, without some symptoms; but such compression would seem inevitable, if luxation had been present. Nor is it at all certain that luxation can occur in any but the cervical vertebræ; no instance is recorded in proof of such a possibility.

The supposition of a fracture was excluded by the impossibility of lessening the angular bending of the spine, by the want of crepitus, and by the rapid recovery; and besides, had such deformity been the result of fracture, injury to the cord would most certainly have been sustained.

Sir ASTLEY COOPER relates a very similar case to the above, except that in it there were two or three spinous processes broken also, and a laceration of the muscles on one side; complete recovery ensued.

It was suggested by a gentleman who saw the case of which I have given the details, that the injury was neither a fracture nor a luxation, but a squeezing out forwards of the inter-vertebral substance, the mechanism being the same as when the body of a vertebra is crushed by indirect violence. The inter-vertebral fibro-cartilage is held in place by very close and strong attachments to the bones above and below; it is, moreover, confined on every side by the ligaments, and especially in front by that one which usually receives the name of *anterior common* ligament; so that such a displacement of it would seem almost impossible practically. This explanation must, therefore, be looked upon as purely theoretical, until an opportunity occurs for verifying it by dissection.

Two cases of somewhat similar deformity have come under my notice, although, as will be seen, their attendant circumstances were different; I call them similar, because they likewise concerned the eleventh dorsal vertebra, which was abnormally prominent, and because the precise nature of the lesion could not be detected. In one of them, the child was a stout and healthy girl, 4 years of age; five or six weeks previously to my seeing

her, she was known to have fallen down some steps. When her mother brought her to me, she told me that she had noticed, a few days before, something peculiar in her walk, and was led to examine her, when she found a lump in her back. This lump proved to be the spine of the eleventh dorsal vertebra, projecting very slightly to the left, and maintaining perfectly its relation to the transverse processes and to the ribs; as if the lower part of the spinal column had been displaced forward *en totalité.* The child walked quite feebly, and carried its shoulders a good deal backward. No effect had been produced upon the bladder or rectum. Unfortunately, this very interesting case has passed beyond my reach.

The other case was that of a girl $3\frac{1}{2}$ years old, not at all healthy in appearance, and of small stature. About a year before she was brought to me, she had a very serious illness of some kind, and never perfectly recovered from it; at about the same time she fell down some stairs, and to this fall her mother seemed disposed to attribute the affection of her spine. She began to be quite lame about six months afterwards. Upon examination, three months ago, the eleventh dorsal vertebra was seen to project backward to a marked degree, much as in the preceding case; and this child, like the other, walked feebly, and carried its shoulders a good deal back. There was, however, some difficulty in urination in this case, and the right lower extremity seemed shortened. An accurate investigation was almost impossible, from the extreme fretfulness of the child. A stimulating liniment was ordered, with tonics, and a simple but nutritious diet; but, as might have been predicted, no change has taken place in regard to the local affection.

Now, in these two children, fracture may be at once excluded from consideration in making a diagnosis, for obvious reasons. Luxation seems equally improbable. Might there have been a displacement, partial or complete, of the intervertebral substance, or possibly a destruction of it by disease? And if so, how are we to explain the peculiar deformity, and the carrying back of the shoulders? The recent date of the first case, and the robust health of the child, excluded from my mind the idea of disease of the bones or fibro-cartilages; while in neither was there the tenderness on pressure, to say nothing of the symptoms connected with the spinal marrow, which usually accompany such affections.

To explain the cases now related, it seems to me that we must assume the possibility of some as yet undescribed lesion of the vertebral column; that neither luxation, sub-luxation, nor fracture could have existed without symptoms quite different in degree, if not in kind. The exact nature of this lesion will probably remain obscure until an opportunity occurs for studying it by dissection.

In regard to the pathology of the spinal cord, although authors have given very positive data as to the effects of injuries sustained at different

parts of its extent, and although much has been written upon its diseases, there are still points which have hitherto escaped attention.

Thus, in fractures of the cervical vertebræ, a very remarkable phenomenon is sometimes observable, viz: an intensely pungent heat of the entire surface below the seat of the injury. An instance of this is related in MORGAN'S *First Principles of Surgery*, as having occurred at St. George's Hospital, London. The temperature of the skin was 111°, while the respirations were only five or six in the minute.

DUPUYTREN mentions in connection with one of his cases, that the skin was hot; but he seems to mean merely the heat of a febrile movement, which is but slight compared with the phenomenon now under consideration. Two cases have fallen under my notice, in which it was present in a very striking degree; in one particularly, that of a man who was struck upon the back of the neck by a bale of cotton, weighing two hundred pounds, and falling from a height of four stories, the sensation of heat communicated to the hand was actually painful. Unfortunately, it was not in my power to follow up this patient, who died in about a week from the time of his accident.

How are we to explain this circumstance? Nothing is easier than to say that the rise of temperature is due to deranged innervation; but in what does the derangement consist, and why should the result be a rise, and not a fall, of temperature? It would seem as if a check were removed from some heat-generating agent—possibly the sympathetic system of nerves—which, no longer controlled by the regulating influence of the cerebro-spinal axis, carries on its functions to an inordinate and excessive degree. A rise of temperature similar, and perhaps equal to this, takes place in some fevers, in which the nervous system is very evidently one of the chief seats of the disordered action; but we have then another obvious reason for its occurrence, in the rapidity of the chemical changes which are going on. That this latter is at least not the sole cause of the abnormal heat in injuries of the cervical portion of the spinal marrow, is evident from the fact that respiration is so much diminished in frequency in these cases. Whether the same phenomenon is present when the dorsal or lumbar vertebræ are the ones involved, I am unable to say, but I am inclined to believe that it is not.

Paraplegia, as is well known, often exists when it is impossible to account for it; but in such cases its onset is generally slow and insidious. This rule does not, however, always hold good; sometimes the loss of power is almost instantaneous. A singular case of this kind occurred in the Pennsylvania Hospital about two years ago. The patient was a remarkably stout seaman, 22 years of age. He said that two months before his admission he had been a good deal exposed to cold and wet, while at sea, in painting the vessel; and that he felt pain and numbness in his limbs while engaged in this work. He had had no difficulty of urination; was

perfectly comfortable, with a good appetite; his bowels were regular. The reflex action of his lower limbs seemed entirely set aside, and their sensibility was greatly impaired; sometimes his feet would become quite blue from the languid circulation in their vessels. He could just manage to walk, with great difficulty and uncertainty, holding by chairs, beds, or whatever he could grasp. Various plans of treatment were successively adopted —iodide of potassium, oil of turpentine, very active counter-irritation, tonics, strychnia, mercurials—but after many months of alternate improvements and relapses, he at last left the hospital in a condition very little better than when he entered it.

In another case, that of a little girl, æt. 13, at present under my charge, the affection seems to be possibly of a rheumatic origin. The child had had several attacks of rheumatism, and was under my charge for paraplegia, in the fall of 1857. After several other plans had been tried, she recovered the power of walking, while using a mixture containing phosphate of iron, in conjunction with powerful counter-irritation by means of moxas applied over the spine. I say *while using*, because it cannot be positively asserted that the improvement in her condition was altogether due to the remedies employed. Shortly afterwards she had a very severe attack of rheumatism, involving both the upper and lower extremities, and the pericardium; but after a hard struggle, she shook off this affection also. In September last, the paraplegia returned, and she is now in much the same state as when I first saw her. Her condition is in other respects good; she is fat and plump, has generally a good appetite and digestion, and sleeps well. She has not now, nor has she ever had, any difficulty either in passing or in holding her water; her bowels are very often constipated. There is some tenderness over the dorsal and lumbar spines, and some pain there, according to her account, all the time. Her lower extremities are entirely powerless, and destitute of sensibility; no reflex action occurs when the soles of her feet are tickled. No effect seems as yet to have followed from the treatment, which has consisted in the phosphate of iron, strychnia ($\frac{1}{20}$ gr. *t. d.*), and moxas to the back.

Now it is by no means difficult to conceive of rheumatism attacking the fibrous envelop of the spinal cord; but in this child's case the parts affected in the marked accessions of the disorder are the extremities; and as yet the hips have been exempt. Here is certainly something curious; a child occasionally losing all power and sensibility in her lower limbs, without any apparent cause, and liable also to very violent attacks of rheumatism. Can we assume any connection between the two? And if so, why is it that pain is so insignificant a symptom in the former, and so excruciatingly severe in the latter? Why is it, moreover, that the two conditions come on at different times—yield to different remedies—and disappear independently of one another?

A gentleman, about 30 years of age, a lawyer by profession, had just

come home from a summer trip, the last two weeks of which he had spent at the sea-shore, when he consulted me on account of extreme languor and debility, affecting especially his lower limbs. He had not taken cold, nor been fatigued in any way; nor could he assign any reason whatever for his disorder. His liver being somewhat torpid, I gave him a blue pill; and then put him upon the use of tr. ferri chlor. and quiniæ sulph. This seemed to benefit him slightly; but although his digestion was good, his bowels regular, and his urine normal, he still complained of excessive weakness, particularly in his lower extremities, and of numbness almost amounting to pain, in his knees. I, therefore, ordered him $\frac{1}{16}$ of a grain of strychnia, thrice daily. As soon as he began to take this, his condition improved, until he became strong enough for horseback exercise; he then gradually left off the medicine, and has had no return of the complaint. He told me that on days when he expected to go into court, he had to omit the morning dose of strychnia, because if he took it, he could not control the starting and twitching of his legs.

I have reported these cases because they seem to me to present some points of peculiarity, and because they belong to a class of affections which are as yet obscure, although of great importance; involving, as they often do, the lifelong discomfort and even misery of the patient, and a world of annoyance and discouragement to the surgeon.

ART. VI.—*Remarks on the Anatomical Diagnosis of Cancer.* By J. J. WOODWARD, M. D., of Philadelphia. (Read before the Biological Department of the Academy of Natural Sciences of Philadelphia, Nov. 15, 1858.)

WHAT is the boundary between the so-called benignant and malignant growths? How shall we define cancer? To answer these questions correctly is of great importance at the present time, since they form the basis of all further knowledge of a fatal and imperfectly understood class of maladies. The attempt to offer a scientific solution becomes the more important since, to put ourselves in a condition to do so rationally, it is necessary to go profoundly into a complete investigation of the anatomical characteristics of *all* pathological neophytes.

We are accustomed to regard cancer as incurable, or if we look forward to a time when our therapeutical measures shall be more successful, it is from some newly discovered remedy, or some novel combination of agents, that we expect a cure. The majority of physicians are apt to regard the years of study and patience which too few are willing to devote to investigations into the minute structure of abnormal tissues as quite thrown away.

They regard the succinct statement of detailed facts with which the practised microscopist presents them, as of much interest in a general way, and possibly as of some value in diagnosis, but turning with a sigh of relief from such subtleties, they exclaim, "Give *us* something *practical.*"

But it is from exact modes of investigation, undertaken often simply from the love of truth, that the practical ever takes its birth, and should careful study enable us at some future time to fix definitely the boundary between innocent and malignant growths—should exact observations enable us to recognize the *conditions* essential to the development of either, there is every probability that we shall require little ingenuity to enable us, by interfering with the essential conditions, to prevent the further increase of the new formation.

It is often asked, Can cancer be diagnosed positively by means of the microscope alone? In other words, if we present a microscopist with a morsel of some growth, and without telling him anything of its general appearance or of the clinical history of the case, expect him to answer categorically the question, Is this cancer or not? Are we likely to receive, and is he justified in giving a direct reply?

Our answer to such a query will depend upon our idea of the structural characters of cancer. Those who believe that the elementary forms of cancerous growths are specific and invariable, must unhesitatingly reply in the affirmative. And hence such microscopists as have believed in a special cancer-cell have attempted to diagnose morbid growths in the manner mentioned. The results have been variable; growths have often been pronounced cancerous, which have failed to return after extirpation; while others have been considered innocent which have returned a few months after the operation with fatal effect. As these unfortunate failures in diagnosis have occurred to some of the best microscopists in the world, the microscopic diagnosis of morbid structures has lost the confidence of many respectable and well-meaning practitioners.

Yet a thorough acquaintance with the history of pathological histology will show satisfactorily that these failures were not due to the imperfection of the instruments employed, or to their inapplicability to the subject, but were the necessary result of the stand-point from which the observer regarded cancer.

The fundamental error from which all others flowed was the notion that cancer was a heterologous formation; that it was, as it were, a foreign organism, a parasite on the economy, developing itself at the expense of the victim. This idea, prominently insisted on by Lobstein, originating at a time when nothing was known of minute structure, and when external appearances were all in all, has survived even to the present time, and is still taught in most of the text-books and in the majority of the schools.

True, Johanes Müller long ago had flung out the broad proposition, since largely developed by Virchow, Carl Wedl, and others, that there is

no heterologous growth, but that every neophyte is a repetition, or attempted repetition, of some normal, and especially of some embryonic structure; but this idea was not at first favourably received, and although of late it has gained ground with philosophic investigators, yet even at the present moment it is far from being *generally* believed.

Looking upon cancer as a heterologous structure, diverse totally from any normal tissue, nothing was more natural than that those who first applied the microscope to its study should have expected to find some special elementary form which should be definitely characteristic, just as there are special elementary forms belonging to muscular tissue, adipose tissue, white fibrous tissue or bone. Accordingly, when the growth was found to be made up in part at least of nucleated cells, these were regarded as characteristic; and as a peculiar cell of spindle shape with a large nucleus, since famous as the caudate cell, was observed in several of the cancers first studied, the idea was promulgated that the caudate cell was diagnostic, and cancer was defined as a growth made up in great part of caudate cells.

Now, it is to be especially observed that although this idea was erroneous and led to error, yet that the error was the fault of the interpreters and not of the microscope; for when numerous mistakes in diagnosis had shown that growths containing caudate cells frequently ran the course of innocent tumours, while many in which no caudate cells existed presented the clinical history of cancer, new investigations were undertaken, and a broader survey of normal and pathological structures revealed that the supposed specific caudate cell was in fact simply a young connective tissue cell, identical with that of the gelatinous connective tissue of the embryo, identical also with the lymph-cell in a state of transition into fibres, and that although caudate cells frequently occur in cancers, just as do also bundles of white fibrous tissue, the perfected result of the development of such cells, yet they can no more be regarded as characteristic than bloodvessels, which also, and far more invariably, enter into the composition of the cancerous mass.

It is also to be observed that this error of the microscopists thus shown to have arisen from generalizing upon too limited a number of facts, was the means of leading to important results; especially it acquainted pathologists with a class of neophytes best described perhaps by the term of pathological growths of "unripe fibrous tissue," which formerly had been confounded with carcinomatous disease, or referred indefinitely to the vague group of sarcoma.

It was shown, in fact, that the cancerous parenchyma was held together and contained in an areolar framework of fibrous tissue, sometimes ripe, sometimes unripe. The unripe fibre-cells of an undeveloped framework or stroma were the caudate cells.

But the idea of a special cancer-cell was not therefore abandoned. It was observed that in many cancers the parenchyma or pulp which lay in the areolæ just described was composed chiefly of cells, and as these, in

very many cases, were large and granular, oval, oblong, or various in shape, with one or two overgrown nuclei and large nucleoli, these characters were assigned to the cancer-cell.

At a later date, when it was found that cells were to be observed elsewhere, as occasionally on the ocular conjunctiva, in the tubuli uriniferi in Bright's disease, and sometimes in the flabby and overgrown granulations of old ulcers, which imitated exactly any one form selected as typical, the characters supposed to distinguish the cancer-cell were yet further modified, and it was asserted that it was not so much by the appearance of one cell as by the conjoined characters of a group of many cells that a diagnosis was to be made. Reliance was especially to be placed on the great and apparently purposeless variety in the form of the cells to be observed in any given specimen, on the large size of the nuclei, on the presence of two or more nuclei in one cell, or of two or more nucleoli in one nucleus; on the transparency of the nucleus immediately after the growth is removed, and its becoming granular in the course of a few hours, etc. etc.

There can be no doubt that all these characters exist in many cases in the cells of cancer, but if we regard them as pathognomonic, wherein shall we see the cancerous nature of such growths, undoubtedly cancerous from their history, as I described in two former numbers of this journal, in which *no* cells existed, the growth having had its development arrested at the nuclear stage?

It is thus seen that the essential error which lay at the base of all others was the idea of the heterologous character of cancer, and the consequent notion that there must exist some heterologous form element, the index, as it were, of the heterologous nature of the growth, which should serve for its diagnosis.

The progress of microscopic investigation, however, has more and more confirmed the notion, which originated in the scientific mind of Johanes Müller. The careful and minute study of morbid epigeneses, more and more assures us, that disease introduces no new element into the economy; that when a modified exudation takes place in sufficient abundance to serve as the blastema of a new growth, organization takes place, according to the invariable laws which guided the original development of the organism, and not according to any new laws devised for the exigency. The careful micrologist, who studies disease, is more and more disposed to speak *less of pathological laws than to see in pathological law the invariable, constant physiological law working out its inevitable results under new conditions.*

A severe survey of pathological neophytes assures us that as a general rule, they imitate more or less closely some normal tissue. There can be no doubt of this in many neophytes. Adipose tumours, fibrous tumours, epithelial tumours, and a host of other products of disease furnish examples. I know that high authorities have spoken of such resemblances as acci-

dental; but this is, to say the least, unscientific. There are no accidents in nature; all phenomena, even those that seem most erratic, are governed by *law.*

It surprised none who had duly appreciated this fact, to learn that pathological tissues did not spring at once into existence in full perfection, but that in their development they passed through a series of stages exactly similar to the embryonic history of the imitated tissue; that the new tissue was in fact produced by just the same processes as was that it resembled.

Nor was it astonishing to find that the development of the growth might be arrested at any stage; just as in the development of the embryo, interfering causes may check partly or completely the several processes.

Moreover, the announcement of the law of analogous formation, scientifically worked out by Vogel, and which may be thus expressed, that the *new growth is prone to imitate preferably those tissues in connection with which it occurs,* could create no wonderment in minds which had appreciated the physiological phenomena of nutrition.

But even after these generalizations had been fully received, many yet regarded tubercle and cancer as exceptions: others were homologous, these heterologous growths.

With regard to cancer, with which alone I concern myself in this place, a movement in the right direction was made by Virchow, in 1856. In an admirable paper, which must ever command respect, he proclaims his assent to the doctrine of Müller, and attempts to determine the homology of cancer. Carl Wedl has followed in the same path, though with different results, approximating nearer in fact to the truth. But in America the vast majority of medical men are unacquainted with these investigations, and still adhere to the notion of the heterologous nature of carcinomatous growths.

In view of these things, I ventured to propose, Oct. 18, 1858, in the biological department of the Academy of Natural Sciences of Phila., the following questions for investigation :—

1. What special structural characteristics (in the local growth) accompany the constitutional symptoms of cancer; and if any, can such structural characteristic exist without the constitutional symptoms?

2. Can cancerous growths properly be termed heterologous; or, if not, with what tissue are they homologous?

Having been requested by the department to conduct the investigation of this subject, I take this opportunity of desiring gentlemen engaged in similar investigations to address me at the University of Pennsylvania, for the purpose of comparing results and exchanging specimens.

Engaged for some time in the investigation of the subject, I am far from prepared to publish detailed accounts of my researches; and furnish this paper simply as the first instalment of the labour I have undertaken for the biological department. Its object is rather to break the ground and begin to sow the seed, than to attempt prematurely to reap the harvest.

The idea of the homologies of cancer promulgated by Virchow may be stated as follows: The areolar stroma or framework in the meshes of which the parenchyma of the growth is contained, consists in fact of connective tissue, in diverse states of development in different growths: in some it is ripe, in others unripe. The parenchyma itself (krebssaft), is composed of large nucleated cells figured by Virchow, much as they have been figured by other observers, and which are considered by him as homologous with *epithelial cells.* There is no doubt that there are many gross external characters by which this idea is justified, and there can be no dispute as to the fact that in many cancers connected with epithelial surfaces, the law of analogous formation induces a far greater resemblance, so as to justify the designation epithelial cancer, applied to such growths. But in determining the homologies of any given pathological new formation we must jealously watch lest we be led away by mere external resemblances, and thus induced to misconceive the essential facts of the case. At any rate Virchow's researches led in the right direction, and the sanction of such a name as his gave great encouragement to those observers who had begun to consider the designation *heterologous* applied to neophytes unscientific.

Carl Wedl afterwards, in a laborious treatise, entitled *Rudiments of Pathological Histology,* modified much, and it appears to me for the better, the doctrine of Virchow. The essential diversity between Wedl and Virchow is that the former regards the large nucleated cells of the parenchyma as also homologous with young connective tissue, while the latter considers them homologous with epithelial cells. Wedl accounts for the apparent diversity between the young connective tissue-cell and the so-called cancer-cell, by supposing that in consequence of the very nature of cancerous development the young connective tissue elements, suffer certain pathological changes to which all cells are in fact liable, these changes being chiefly three, viz: 1. Arrest of development. 2. Hypertrophy, distension perhaps would be a better word, in consequence of excessive imbibition of plasma. 3. Simultaneous fatty degeneration in consequence of the impoverished quality of the plasma. Cancer then, according to Carl Wedl, is essentially, as he himself expresses it, in a passage quoted by me in a former paper, "a malformed (aborted) and degenerating new formation of connective tissue." (*Path. Hist.*, p. 610.)

It is not my purpose in this article to enter into an elaborate discussion of the homologies of cancer. Although this subject constitutes an important part of the duty assigned me by the department, it is my purpose to wait until I shall have collected more abundant material than is at present in my possession before entering upon it at large. My present object is rather to trace out the inevitable consequences of the establishment of Carl Wedl's doctrine in connection with the question of microscopic diagnosis,

and especially of the microscopic diagnosis between cancer, and the so-called benignant new formations.

Before so doing, I desire succinctly to express the notions to which my own study of cancerous growths have led me, because, although I might dismiss the matter with the simple statement that in the main I am disposed to assent to Carl Wedl's doctrines on the subject, yet his work (*loc. cit.*) has been as yet so little read in this country, that such an exposition will be absolutely necessary to make my subsequent observations generally intelligible.

If cancer be a heterologous growth, as is commonly asserted, the heterology must reside essentially, as Wedl indicates, in one of two catagories, either, 1st, the secondary arrangement or grouping together of the form elements must be unlike the manner in which form elements are grouped together in any normal structure, or, 2d, the form elements themselves must be *essentially diverse* from any normal form elements. If, however, severe investigation shall reveal that the secondary grouping obeys the grand law of secondary grouping in all normal structures, and that the diversity between the form elements and those of some normal element, say young connective tissue-cells, resides in unessential and not in essential particulars, how shall we predicate heterology of structure ?

I. Now, *first*, with regard to the secondary arrangement or grouping together of the form elements. Carl Wedl has shown, as indeed has been long known and described by Bennett, Virchow, and others, that the essential type of secondary arrangement in cancer is the areolar type, and that the papillary or dendritic type is a *non-essential* modification of this. *But the areolar type of secondary arrangement is the* USUAL PLAN in all the organs of the body, the only modification being into the papillary plan in the case of normal as well as of pathological structures. The mode of secondary grouping, in a word, in the case of all normal and of all pathological structures, is the *areolar type*, or some non-essential modification of it arising from local exigencies. We may also go a step further, and set forth that the stroma or framework, built according to the areolar type, in which the parenchyma of all tissues, normal or pathological, is situated, *consists invariably of connective tissue in some stage of development;* and that the bloodvessels, and nerves and lymphatics, where these exist, of all structures, normal and pathological, are distributed, except in their most minute ramifications, in the substance of the stroma, and not in the substance of the parenchyma itself.

The vascular stroma of connective tissue thus constituted forms an important part of the human frame. Anatomists speak of its larger and more visible portions under various names—such as fasciæ, aponeuroses, sheaths, capsules, &c.; and from many of the descriptions one might overlook the fact that there is no real separation between these several portions, but that the stroma or framework of the whole body is everywhere continuous, and

forms a connected and uninterrupted scaffolding, in the freely communicating areolæ of which the several form elements—such as adipose cells, muscular fibres, &c.—repose. This arrangement is readily demonstrated by studying thin sections of any part of the body with a moderate magnifying power. The shape and size of the areolæ vary much in different tissues. Thus, for example, the areolæ in the muscles are much more elongated than those of adipose tissue; and these, again, are far larger than the areolæ of the cutis veræ. But the areolæ vary also infinitely in shape and size in the same tissue, as is conspicuously seen in the study of thin sections.

The stroma of pathological growths, including cancer, is demonstrated, by means of thin sections, to present identical characters, and similar relations to the form elements of the parenchyma; and if in the case of cancer the large size and variable shape of the areolæ be insisted upon, the above remarks will indicate that no heterology can be predicated on that ground.

The stroma of normal structures in the adult consists invariably of bundles of well-developed connective tissue. In the embryo, however, there exists a period for every tissue, in which the stroma is composed of undeveloped connective tissue, of caudate cells, in a word, varying in the degree of their development with the age of the embryo. In the case of pathological growths, also, it is common to find the stroma consisting of embryonic connective tissue elements; and if, as has been asserted above, pathological growths are developed in the same manner and according to the same invariable laws as normal textures, we would, *a priori*, expect this to be the case whenever the death of the patient, removal of the growth by the surgeon, or *any other cause*, arrests development in the embryonic stages. We cannot, then, predicate a heterologous character of a cancer, or any other pathological new formation, because the connective tissue of the stroma consists of caudate cells. Such a condition is merely an expression of the fact that the development of the growth is in the embryonic stage.

It is also to be observed that there exist in the adult, textures (such as the fasciæ proper, many capsules, &c.) in which the vast majority of the areolæ are *empty*, or contain only the exuded liquor sanguinis in which all the tissues are bathed. Moreover, in certain textures, some of whose areolæ contain other elements, others are empty. Thus, in the transition ground between the skin and the subcutaneous adipose tissue some of the areolæ are empty, while others lodge fat-cells. If the development of the part is completed, the boundaries of such empty areolæ consist of fully developed connective tissue bundles; but there has always existed a period for every such areola, when attached to the inner margin of its boundaries of connective tissue bundles were embryonic forms (the caudate cells), in several stages of development, the most developed being situated peripherally, and the least developed towards the cavity of the areola. At a still earlier

period no cavity existed, and the whole areola was filled with embryonic connective tissue elements, the least developed occupying the central position.

There is good reason to believe that the history of the development of an empty areola, thus chronicled, is the history of the development of those areolæ which contain other form elements, if we add to what is above written the further statement that the elements last formed in the interior of the areola, in virtue of the unexplained force which regulates the development of the organism, develop, not into connective tissue, but into the elements of the tissue forming, whatever it may be. A good example, which will illustrate my meaning, is the history of the normal development of the skin and subcutaneous adipose tissue of the human embryo. At an early period the skin and subcutaneous adipose tissue are apparently homogeneous; the one is covered by no epithelium and contains no glands or hair-follicles, the other contains no fat-cells; both are alike composed of caudate cells in various stages of development into fibre. Examination of sections shows that the areolar arrangement above indicated obtains. At a later period sections reveal the presence of fat-cells in the subcutaneous areolæ; certain areolæ in the skin are found occupied by little groups of cells, which are eventually transformed into glands or into hair-follicles and hairs, and an epithelium has made its appearance upon the surface. There is every reason to believe that this is, in the main, the history of the development of all normal structures. There can be little doubt that it is also the history of the development of all pathological growths. Especially interesting, in this connection, is the study of the development of bulky inflammatory exudations, and of that interesting class of tumours long spoken of vaguely as albuminous sarcoma, which are best described as pathological growths of unripe connective tissue.

If, bearing in mind the above facts, we proceed to the investigation of cancer, we are driven to conclude that *the term "heterologous" is in nowise justified by the secondary arrangement of carcinomatous growths.* It only remains, then, to consider the character of the elementary forms of the parenchyma.

II. In studying, *secondly*, the elements of the parenchyma which are contained in the areolæ of the cancer stroma, I have been driven from the belief that these elements are in truth heterologous; for although at first sight the cells or nuclei which may be present are dissimilar to the elements of any normal tissue, yet the *dissimilarity is not of essential nature*, but such that it can be readily accounted for by supposing the cells to have been subjected to certain morbid processes which are not peculiar to cancer.

Among the opinions advanced of the homologies of the elements of cancer two only deserve notice in this place, that which regards them as homologous with epithelial cells, as advanced by Virchow, and that which

regards them as diseased connective tissue elements in the embryonic stage, which is the doctrine of Wedl.

With regard to the first doctrine, it is to be remarked that the shape of the normal elementary forms of any texture vary considerably; cartilage cells, the cells of the parenchyma of the liver, nerve-cells, &c., may especially be quoted in connection with this observation. In mere shape a given elementary form of one tissue may more or less resemble the elementary forms of another tissue; yet, the situation and general relation of any given series of forms will readily determine their homology. No one, for example, will deny the epithelial character of the layer of cells lining the small arteries; yet, any microscopist can in a few moments satisfy himself of the correctness of the observation of Kölliker (*Micros. Anat.*, p. 678, Phila. edit.), that these cells, when isolated, "present no small resemblance on the one hand with the fusiform cells of pathologists," as well as "with the formative cells of the elastic fibres and of connective tissue." It is by the situation and obvious purpose of these cells that their epithelial character is determined.

When we come to apply similar criteria to the cells of the cancer parenchyma, we cannot, it appears to me, fail to *deny them the epithelial character, however single cells may resemble epithelial cells.*

It is the habit of epithelial cells to be placed in a single or several layers upon free surfaces. We find them nowhere piled together, filling up completely the areola of a connective tissue stroma. If the cells were found simply lining the areolæ, and these were empty, or simply filled with fluid, we might call them epithelial cells; but now, not so. *If they be really epithelial cells, there is heterology of grouping, and cancer is a heterologous formation.* For this reason, alone, I would reject the theory of Virchow. Still more do I reject it, when I find among the flattened forms, upon whose external resemblance to epithelial cells Virchow based his doctrine, numerous other elements far more resembling young connective tissue forms, and observe *no forms* which cannot be explained by supposing embryonic connective tissue elements to have been more or less modified in consequence of the three morbid processes alluded to by Wedl (*loc. cit.*); and above all, when I consider the grouping of the elements, and their relation to the stroma. With Wedl, then, I regard cancer to be a malformed new formation of connective tissue. In degree of development it corresponds in its totality, as a general rule, to connective tissue in the stage in which, as above described, all the areolæ are filled with embryonic forms, though sometimes the areolæ, or some of them, are filled only with fluid, giving us alveolar cancer; or if the communications between the several areolæ have been obstructed, cysts make their appearance.

The influence of the three morbid processes mentioned by Wedl, in producing the forms observed in cancer, may be thus stated:—

1. Development is arrested while growth proceeds. The history of the

development of connective tissue, is that of a series of transitions from round to oval, from oval to spindle-shaped elongated cells, with final fibrillation of the cell content, and ultimate metamorphosis into the well known bundles. In the round or oval state these cells are capable of multiplying by division. Now, the young connective tissue elements in cancer may have their development arrested in any of these stages, and as a general rule it may be stated that the more malignant and rapidly developed the cancer, the earlier the point at which the development of the single forms is arrested. Arrest of development may even occur in the stage of nucleus, before cells are formed. Its arrest in the earliest period, after the formation of cells, gives round or oval forms; a little later (when the cells are preparing to multiply by division) it will give double and plural nucleated cells; a little later we will have elongated cells, more or less approaching the caudate shape. The arrest, as a general rule, does not affect all the cells alike; but one cell may proceed further in its development than its neighbour, and hence arises an infinite variety of forms.

2. The diversity of form in the elements of the parenchyma which arises thus, is still further increased by the fact that growth proceeds, although the development of the cells has been arrested. Nay, from the excess of blastema furnished, the cells may overgrow and become hypertrophied, or, perhaps, it would be better to say distended, so that they exceed the ordinary size of connective tissue cells at a like period of development. The same thing occurs frequently in the nuclei, and hence the great size of the nuclei in the so-called cancer-cells. In consequence of mutual pressure among the form elements resisting this endosmotic distension, the form of the cells is yet further modified, and a more infinite diversity in the shape of the elements results.

3. Lastly, to complete the picture, the same perverted and impoverished character of the blastema, or of the forces organizing it, which determine arrest of development, determines also atrophy; fatty degeneration, in a word, which, proceeding simultaneously with the growth of the cells, terminates finally in their destruction, and in that ulceration and sloughing which is so characteristic.

To this cause is due the granular appearance of the cells in cancer, and it is to be observed that this character is frequently most conspicuously seen in growths which were enlarging rapidly at the time of their removal from the body.

The three processes thus enumerated, acting on young connective tissue-cells, are, then, competent to produce all the varieties of form noticeable in cancer; but other facts, besides anything observable in cancer, can be brought forward to prove the same thing; especially significant is the fact that in flabby, juicy granulations the connective tissue-cells on the surface, which are supplied with an excessive quantity of blastema, while their forces are impaired, and their vitality diminished by contact with the atmo-

sphere, may suffer metamorphoses of a similar kind; and huge, misshapen cells, with distended nuclei, and filled with oil-granules, may result, which, viewed by even skilful observers, might be confounded with cancer-cells. A similar distension of the cell and nucleus, with degeneration of the contents, has been observed in certain epithelia, especially in the case of that of the tubuli uriniferi in Bright's disease. The cells, then, present a great resemblance, in mere external characters, to those of cancer. Virchow used such facts to justify his doctrine; but, properly interpreted, they would lead rather to *shake any faith in characters based upon form alone, without regard to situation and other relations.*

If it be regarded as an established fact, that the three morbid processes named *can* modify connective tissue elements, so as to cause them to present the characters of cancer, the question at once arises, how account for the simultaneous occurrence of the three processes in cases of carcinomatous disease? If the view of cancer above presented be correct; if its homologies have been rightly understood, the answer to this question will complete our knowledge of cancerous growths, and undoubtedly a correct determination of the conditions which serve as momenta, will be the basis of new and satisfactory therapeutical measures.

It is not my purpose in this paper to enter into any elaborate discussion of these momenta; indeed, at present there are not enough facts to justify it. Suffice that the momenta cannot be of a purely local character; such would fail to account for the return of the disease in internal or other organs, both before and after extirpation. Pending their satisfactory determination, we may continue to speak of a cancerous cachexia, or modification of the blood, as the starting point, and regard the local processes as the inevitable result of the perverted character of the circulating fluid. But we must avoid dogmatism on this point; for many reasons, such a solution is far from satisfactory, and it is seldom safe to imagine unproven anomalies to account for observed phenomena.

With this brief statement of the homology of cancer, we are prepared to enter into the discussion of the grounds on which its anatomical diagnosis is to be based.

And first, it may be remarked that the initiatory step to be taken in attaining data for microscopical diagnosis, is to reject the unscientific notion which I have shown to be the natural result of a belief in the heterology of cancer; I mean the notion of a special cancer-cell.

He who regards the presence of any one characteristic form to be pathognomonic, may be favoured, fortunately, with some accidental success in isolated cases; but in many instances, he is doomed to inevitable disappointment.

It is after a profound investigation of the anatomical characteristics of the whole growth, alone, that a diagnosis, based upon microscopic appearances, is justifiable. It is not enough to scrape a little juice from the

surface of a scrap, and to predicate a definite conclusion upon the shape of a few cells. The microscopist, whose views permit him to adopt such a course, if long experience and great practice have matured his powers of observation, will frequently present an opinion upon such data, which will coincide, fortunately, with the after history of the case; but he will as certainly commit frequent errors, which will bring discredit upon the microscope as a means of clinical investigation.

I propound, then, the proposition that, to justify the microscopist in his diagnosis, the whole growth must be submitted to him; he must be enabled to study not only the isolated elementary forms, but their secondary arrangement and their relation to each other. Where possible, so much of the surrounding tissues should accompany the growth as shall enable him to determine minutely the relations of the neophyte to the normal textures, and the question of the healthy or diseased character of these. It is also essential, for the purpose of facilitating the diagnosis, that he should be informed of the seat of the growth; for although a practised anatomist will often determine the organ from which a morsel may be presented to him, yet diseased organs so lose their normal characters as to render it often difficult, or even impossible, to decide which of two similar organs is concerned; and without due information, it will readily be conceived by any one who has read the above paragraphs, a portion of a normal texture transformed by disease may be mistaken, even by a practised observer, for a new formation, thus giving rise to necessary error in the interpretation.

I propound the above proposition the more emphatically because many practitioners act upon very different ideas. They hand the microscopist some pathological morsel, often half putrid from the length of time which has elapsed since its removal from the body, or injured by maceration in water and alcohol, and even if he be not informed as to the size or seat of the growth, they expect him to reply categorically with regard to its nature and future history. The microscopist, whose time and inclinations permit him to investigate thoroughly such specimens, will occasionally be enabled on rational data to arrive at satisfactory conclusions. But he who has not courage to refuse any reply, except where his *data are in every respect satisfactory*, must necessarily fall into continual error.

Nor this alone. As I conceive it, the microscopist, whose time permits, has not done his whole duty by a morbid growth when he merely decides on the question of diagnosis. Were not the whole growth necessary for diagnosis, it should be furnished, to enable the observer to gain the broadest views of the minute structure of the given new formation. None of these points are so thoroughly understood as to preclude the necessity for further study.

The whole growth having been submitted, must be subjected to careful study; especially is it necessary not only to examine the isolated elements obtained by scraping or teasing, but to make and observe thin sections for

the purpose of recognizing their secondary arrangement. These data having been exactly ascertained, it will be *proper to call the growth a cancer whenever they show it evidently to be a new formation of connective tissue, aborted or deformed* by the three anomalies above indicated, viz : (1) Arrest of development at diverse stages in the several elements, with (2) progress of growth in the elements, amounting even to distension, continuing after the arrest of development, and accompanied by (3) a simultaneous fatty degeneration.

It would be foreign from the purpose of this article to enumerate the infinite variety of external and of microscopical appearances, which can all nevertheless be interpreted by reference to the above data. These will be found in the several treatises on cancerous diseases. Looseness in the adhesion between the form elements, permitting them to exude, or scraping the cut surface as the so-called cancer juice, diversity in the external characters of diverse parts of the growth, and many other characters supposed at different periods to be pathognomonic, are, when present, the result of the relative degree to which the three essential anomalies have been carried.

I must also remark that, in my opinion, so far from the abandonment of the doctrine of a special cancer-cell invalidating the results of microscopical diagnosis, *we are now for the first time in a condition to give an opinion which shall deserve the name of positive.* And we have gained in certainty all that has been lost in facility by substituting the result of a thorough analysis of the growth for the hasty and flippant examinations on which microscopical diagnoses were formerly based.

In answer, then, to the question, Can cancer be positively diagnosed by means of the microscope alone? I answer unhesitatingly it can, in the majority of cases, provided the whole growth, with the knowledge of the normal texture involved, be submitted to the pathologist. In very many cases, a diagnosis can be made by no other means. At the same time, I am disposed to admit the existence of transition forms, of which, standing as they do upon the boundary line, no dogmatic opinion can be given. But the microscopist can always answer positively this is cancer, this is not cancer, or this is a transition form.

In this connection, it must be observed that the growth above described as cancer generally pursues a definite clinical history. This has been frequently described, the growth and final ulceration and sloughing of the tumour, the implication of neighbouring lymphatic glands and of internal organs, the return of the disease after excision, and the fatal termination. There can be no doubt that this is the course of the vast majority of cases, and in these the microscopic diagnosis becomes the basis of a sound prognosis. But the question has been propounded, Whether these structural characters can exist as a purely local disease? Analogy is in favour of this possibility. We know, for example, that in the case of pus, its formation may be on the one hand the inevitable result of a pre-existing constitu-

tional condition, as is seen in the pustules of smallpox; or, on the other hand, it may be a strictly local process, as in the case of a suppurating wound. In like manner, it might be supposed that local conditions acting upon a new formation of connective tissue might so modify it as to cause it to present the cancerous characters; and that, as Rokitansky has dogmatically expressed it, carcinomata may "originate and subsist" as local evils. (Rok., *Path. Anat.*, p. 196, vol. i., Phila. edit.)

The possibility of this occurrence cannot be denied on theoretical grounds; it is one of those questions as to fact which can only be determined by observation. There are upon record a number of cases which would indicate that this possibility sometimes occurs. But even if we receive all these cases unquestioned, their number is extremely small, and the general rule will be, to use again the words of Rokitansky (*loc. cit.*): "That far more commonly, however," such growths "are associated with a dyscrasis." But it is exceedingly questionable whether we ought to receive the record in every case. If the history above given of the progress of *microscopic diagnosis* (*which alone is reliable*) be correct, we shall regard with great doubt a diagnosis unjustified by clinical history, which has been made in the faulty and unscientific manner practised till so recently.

For myself, without attempting to contradict on theoretical grounds the existence of a "*local cancer*," or to deny it dogmatically, as some have done, I must confess that I have yet to see a growth which, corresponding in characters to what I have above described as cancer, has failed to pursue the usual clinical history. I cannot at present, therefore, regard this *possibility* as seriously vitiating a careful prognosis.

In concluding this article, I shall present a brief account of two cases, to illustrate the grounds and method of a microscopical diagnosis.

CASE I. The first case to which I would refer is that of a tumour of the breast of a woman 43 years of age, removed at the clinic of the University of Pennsylvania, October 16, 1858, by Prof. Henry H. Smith. The details of the general history of this case will be found in the "Illustrations of Hospital Practice," of the *Medical and Surgical Reporter* for Oct. 29, 1858. It is not essential to our purpose to present these details in this place. The breast at the time of the operation was enlarged to once and a half its normal size. It was smooth, regular, hard, and adherent to the skin, which, however, was healthy in appearance. *There was no retraction of the nipple.*

After its removal, a section of the growth showed that all parts of the gland were alike involved, and the cut surface—which was yellowish-white in colour, and exhibited a reticulated fibrous appearance—yielded, when scraped with a knife, a thin watery serum, rendered turbid by small particles mixed with it, which, though not so homogeneous or cream-like as we usually see the so-called cancer juice, might readily be mistaken for it upon superficial observation.

The general history of this tumour, and many points in its external appearance, rendered it improbable that it was of a cancerous character. Yet

it appears to me that a conscientious microscopist, who believed, as many yet do, that cancer is a heterologous growth, characterized by a special cancer-cell, and that the leading feature of this cell is its large, clear, transparent nucleus, with a granular appearance in the rest of the cell, must have been constrained to pronounce the growth cancer, when he found that the turbid appearance of the juice scraped from the surface was due to the presence of little groups of eight to fifteen cells adhering together by their edges, and which, in fact, presented all these characters. Some doubts might, perhaps, be excited in his mind by the close adherence of the cells together, which is not usual in cancer. It was indeed difficult to obtain an isolated cell. But this fact could not influence the conclusions of one whose stand-point has led him completely to reject the study of the secondary arrangements of growths as an element in diagnosis.

A few thin sections—some made with Valentin's knife from the fresh growth, others with a scalpel from dried portions, which were afterwards soaked out, some with water and others with acetic acid—revealed anatomical conditions which would necessitate a very different conclusion.

The compressed milk-tubes were seen to be separated from each other to a preternatural distance, by connective tissue, well developed for the most part, but also presenting many fibre-cells, in various stages of development, intermingled with the fully formed bundles. The cells of the epithelium of the milk-ducts were observed to be in a state of fatty degeneration, with distended nuclei. And it will now at once be understood that the groups of eight to fifteen cells adhering together by the edges, *so far from being cancer-cells, or, indeed, any pathological new formation, were really portions of the epithelium of the milk-ducts*, modified and degenerated as a secondary consequence of the pressure to which they had been subjected by the extensive new formation of connective tissue in the substance of the areolar tissue of the gland, which constituted the primary or essential lesion.

Such groups of epithelial cells adhering together, are frequently to be observed when any epithelium is forcibly detached from the subjacent structures. And with regard to the distended nuclei, where causes inducing impaired vitality—as indicated by the consequent fatty degeneration—are accompanied by an excessive supply of plasma to the cells, distension of the nucleus is frequently to be observed. The epithelium of the tubuli uriniferi, in Bright's disease, occasionally affords an example of this. It is probable that the distension in such cases is, to a great extent at least, the physical consequence of an endosmotic act.

The lesion in this breast was, therefore, pronounced to be simply a pathological new formation of connective tissue in the normal connective tissue of the gland; an opinion which, of course, became the basis of a favourable prognosis.

CASE II. The other case I shall mention in this place, derives its chief interest from the fact that its cancerous nature could only be safely announced on the grounds of the minute structure of the growth. It occurred in the private practice of Prof. Henry H. Smith. The patient, aged about sixty years, presented, in her left breast, an ulcer the size of a half-dollar, which involved the nipple, and had gradually attained its present size during a period of about eighteen months. During this period, it had been carefully treated, but refused to yield to any of the measures adopted, and as the patient suffered much inconvenience from the pressure of her dress upon the sore, it was deemed best to dissect out the ulcer, including with it the whole induration, which was accordingly done.

The parts removed were submitted to me for study, through the politeness of Prof. Smith. They constituted a mass about three and a half inches long, two and a half broad, and one and three-quarters in thickness, and contained the whole mammary gland; some fibres of the pectoral muscle to which the growth was adherent being upon its back, and its margins being composed of the healthy adipose tissue of the surrounding parts.

In order to appreciate the conditions presented, it will be necessary to recur for a moment to the normal appearance of the mammary gland in a person of this age. It is well known that, after the menstrual discharge ceases, the mammary gland undergoes atrophy, and is finally more or less completely replaced by adipose tissue. At the age of the patient in question, we would expect to find that "all the gland vesicles have disappeared, and nothing but the more or less persistent lactiferous ducts, with their epithelium in a state of fatty degeneration, are to be found in the adipose cushion which supplies the place of the glandular tissue." (See Kölliker, *Mic. Anat.*)

That the involution of the gland had fairly progressed in this case was evinced by the condition of the opposite healthy breast, which was even more completely atrophied than usual.

Bearing the above conditions in mind, I proceeded to the investigation of the case. On cutting into the growth from behind, I observed that the ulcer rested upon a grayish-white, firm mass, extending from the skin to the pectoral muscle which it involved, some of the fibres of the pectoral actually passing through its posterior part. Everywhere else it was bounded by adipose tissue into which it extended itself irregularly in several places. When the cut surface was scraped with a scalpel, an abundant, homogeneous creamy *juice* exuded. This juice presented the following form elements when studied with an objective of $\frac{1}{8}$ inch focal length.

(*a.*) Large oval free nuclei, with a single or double vesicular nucleolus. These nuclei were perfectly transparent when first examined, but became finely granular shortly after the addition of water.

(*b.*) Oblong oval, or variously shaped cells, sometimes exceedingly irregular in contour, containing a single nucleus, identical with the free nuclei above described, except in the occasional absence of the nucleolus. These cells were granular, and contained many Aschersonian vesicles of minute size, indicating the presence of well-marked *fatty degeneration.*

(*c.*) Cells identical with the last, but with two nuclei.
(*d.*) Caudate or fibre-cells differing only from those of healthy young connective tissue in the rather larger size of the nuclei.
(*e.*) Every possible transition between the cells described at *d*, and those described at *b*.
(*f.*) Innumerable fat-globules and granules (larger and smaller Ascher-sonian vesicles).

Numerous thin sections, cut with a Valentin's knife, showed that the above elements were contained in a loose stroma of connective tissue, constituted partly of well developed bundles, but partly also of the forms described above at *d*. These areolæ were large and irregular, and communicated freely. I failed to find anywhere the slightest trace of lactiferous ducts, and must regard the new formation as having completely supplanted, or replaced the normal textures, as frequently occurs. The pectoral fibres among which portions of the growth were infiltrated, were in a state of fatty degeneration.

If the account of structure here given be compared with the views expressed in preceding portions of this paper, it will be perceived that the data afforded by this case justify from the anatomical point of view, no other name than cancer for the new formation.

The appearance of the patient, however, is far from what would be expected in a case of carcinoma. This, however, will not invalidate the diagnosis; emaciation and impaired general health are frequently absent in cancer, until a very short time before the fatal termination.

The operation was performed for the removal of this ulcer in October, 1858. It is therefore yet too early to look for a return of the disease. Should the patient escape this fate, I shall be justified in believing that I have seen one case of *local cancer;* that is, a growth with the minute structure of cancer without its clinical history. I look forward, therefore, with much interest to the issue.

Numerous other instances might be adduced, but I trust the two above given will be sufficient to make my meaning clear. And, I therefore conclude the present paper, hoping that it may be instrumental in securing the co-operation of other labourers in the future promotion of the important task assigned me by the biological department.

Art. VII.—*Blood-Stains.* By Andrew Fleming, M. D., of Pittsburg, Pa.

In the trial of criminal cases, especially where the evidence is of a circumstantial character, it is frequently of the greatest importance to determine of what certain spots are composed, in order to fix the guilt or attest the

innocence of the accused. To this end science has been employed, and frequently, by the weight of her testimony alone, has acquitted the innocent or condemned the guilty. Without any desire to magnify her office, it is a high tribute to science to know that she can by her powers aid in fastening the guilt on him who, under covert of darkness and in the stillness of the night, steals upon his victim while in calm and peaceful slumber, and for sordid lucre or revenge commits the crime of murder. This duty naturally and properly belongs to the medical man, who possesses a knowledge of physiology, chemistry and microscopy, which befits him for the examination of organic substances. The vocation of the expert thus employed, who stands as an acolyte to justice, is one of honour and high responsibility, for by insufficient knowledge or experience he may, by his evidence, form a link in the chain of testimony which consigns the innocent to punishment or sets free the violator of law.

To arrive at conclusions fraught with such salutary and terrible consequences, it is a sacred duty to use every means in our power; to take advantage of every circumstance, however trivial, in order to prove, beyond the question of doubt, of what certain colouring matter is composed, which has stained clothing, weapons, &c., that may be brought into court, corroborative of crime. Before advancing to the plans and formulæ by which these ends are to be attained, it would be well to have in the mind the character of the substance whose correlative it is proposed to examine. The blood is an almost homogeneous fluid, endowed, while under the catalytic influence of the vessels, with a property closely resembling vitality, and bearing the elements out of which the various structures are to be developed or sustained, and from which the glands elaborate their special secretions. The variety of its numerous attributes indicates the complexity of its nature. Owing to its complexity, and the absolute certainty required in medico-legal investigation, arises the difficulty of the examination of suspicious stains.

Appearance of Blood-Stains.—The colour of blood-stains depends on their age, the material on which they are deposited, the quantity of blood effused, and the circumstances of moisture and temperature to which they have been subjected. If the substance upon which blood is allowed to fall is highly polished—such as metal, porcelain, and varnished wood—the stains, which will be dark-brown shining masses, easily removed, present cracks, radiating from the centre. Shallow stains upon white or porous substances are of a lighter shade when of the same age; yet, when the clots are large, the blood forms for itself a base, upon which the surface presents the same appearance as when on a polished substance. It is impossible to know the age of blood-stains by their appearance, as it is so much modified by various contingencies. The colour is altered by being deposited on substances which modify it; and the presence of blood-stains, when small, upon clothing, furniture, &c., cannot in some cases be ascertained by examination

with sunlight. Dr. Ollivier, d'Angers,[1] was summoned one evening, in March, 1833, to examine without delay certain premises, and furniture therein, where it was thought a murder had been committed upon a woman, whose body had been found upon the street, and there deposited, it was supposed, some days after death. Accompanied by Dr. Pillon, he proceeded forthwith to the house, supposing that it would be impossible to discover blood-stains by candlelight; but, fortunately, this proved the means of their discovery. The furniture, the paper-hangings, which had a pale-blue ground, and the chimney-piece, which was painted black, had been carefully examined in daylight, without anything peculiar being noticed. On bringing the candle close to the wall-paper, a large number of small spots, of a dirty red colour, the fourth of a line in diameter, were discovered, that by day had the appearance of black points, which were confounded with those making part of the figures on the paper. By the same means spots were found on the furniture, and a large one on the chimney-piece. The next day these observations were verified by MM. Lesueur and Barruel, who were obliged to resort to artificial light to find the spots.

I. CHEMICAL EXAMINATION OF BLOOD-STAINS.

The chemical properties by which the presence of blood, whether belonging to man or any other warm-blooded animal, is established, are owing chiefly to the reaction of its peculiar colouring matter. In the examination of suspicious stains it is well to note their absolute and relative position, their form, size, and thickness, and, when upon linen, woollen, or other stuffs, to number or designate them in some way as belonging to some particular part, especially of clothing, for the position of them is frequently corroborative of the manner in which the crime has been committed. It is necessary to put a mark on weapons which have been examined, in order to again identify them.

When the stains are upon cloth, a piece is cut out, containing the stain, or part of it, to be examined, and suspended by a thread in a test-tube; and if the stain is recent, the red colouring matter will soon be imparted to the water, but if it is old, a longer time is required for its solution. If porous substances, such as wood, stones, mortar, and bricks, with blood-stains upon them, are put into water, the blood is dissolved and carried into the depths of the material without giving the colour to water. To overcome this difficulty, it is necessary to rasp or reduce them to powder, and add to water. Stains upon hard substances, whose size renders it inconvenient to dissolve by putting them in water, can be scraped off and then dissolved. In all cases, when upon iron or steel, care should be taken, in removing the stains by water, not to allow the instruments to remain long enough for oxidation to commence.

[1] Archives Générales, ii. sér., t. i. p. 431.

The characteristics by which blood-stains are distinguished from those produced by other substances are the following:—

1. They are soluble in distilled water, and impart to it a beautiful red colour, more or less intense, as the proportion of the size of the stain and water vary; of a very feeble alkaline reaction, changing the red litmus to blue.

2. When ammonia is added to the aqueous solution, no *change* takes place in the colour, but an alteration from red approaching to brown is found, in proportion to the degree of concentration of the ammonia.

3. When the solution is heated, coagulation takes place; the bright red colour is destroyed, and grayish flocculi are formed.

4. These flocculi are quickly dissolved by solution of potassa, and the liquid assumes a green tint by reflected, and red by transmitted light. The dichroism produced in this manner, according to M. Gaultier de Claubry,[1] is *a certain indication of the presence of blood.* When the solution is very dilute, to produce this phenomenon an advantage will be found in using caustic potassa.

5. Blood-stains are insoluble in alcohol, ether, chloroform, and oils.

6. Dried blood is slowly soluble in strong sulphuric and muriatic acids, forming dark-brown solutions; it is more rapidly acted upon by nitric acid, which dissolves it with effervescence.

A little experience in testing shades and tints of colour, whether of a simple or compound kind, soon demonstrates the difficulty in distinguishing them with accuracy. To avoid this, M. Boutigny[2] has proposed an application of the properties which liquids present in their spheroidal state. Having found that when a drop of water is thrown into a capsule heated to 171° Cent. and higher, the liquid forms a sphere, which neither touches nor moistens the capsule—the temperature of the drop of water is always 96°.5 C., upon whatever surface the phenomenon is produced, and the evaporation, when the capsule is heated to 200° C., is fifty times slower than by ebullition at 100° C.—he applied these principles to blood-stains, and takes, for example, a stain having a diameter of 0.001 millimetre, or 0.03937 of an English inch. Next a glass graduate of 0.020 millimetre in length, and 0.002 millimetre in diameter inside, is taken, and the stain then cut out and introduced into the graduate, at a distance of 0.005 millimetre from the bottom, and, by the aid of a pipette, 0.10 gramme, or 1.54 English grains, of cool distilled water is poured upon it. When the stain is completely discoloured, a flat silver capsule is heated to redness over an alcohol lamp, the red liquor is removed by a pipette, and thrown upon the capsule by blowing gently at the extremity of the tube. This operation is scarcely finished before the liquid has lost its transparency, and acquired a

[1] Méd. Légale, p. 783.

[2] Annales d'Hygiène pub., 1844, ii. p. 217.

grayish-green colour. This liquid is then touched with a glass rod, previously dipped in a solution of caustic potassa, and it immediately regains its transparency, and presents a colour, *sui generis*, green by reflection, and red by refraction. If the liquid is now touched with a rod which has been dipped in hydrochloric acid, it loses its transparency, which it regains by the addition of potassa, and thus almost indefinitely, provided from time to time a drop is added to preserve the original volume of the liquid. This very simple and ingenious method, M. Boutigny says, is equally applicable to large quantities of blood.

Analysis.—From the indirect method by which the expert proceeds in this kind of investigation, it is easy to discover that the results are frequently not of so manifest a character as desirable. Chemical analysis is, however, the only means in our power to determine the composition of suspected stains where putrefaction has taken place, or an attempt has been made to wash them out with a reagent of a kind suitable to destroy the integrity of anatomical elements of the blood. In order to more easily and thoroughly comprehend the analysis of blood stains, it is advisable to consider on which of the components of the blood it is founded, and to have in the mind the characteristics by which they are recognized.

Of the great number of constituents of the blood, but five are essentially concerned in the chemical examination, viz: *Hæmatin, Iron, Nitrogen, Fibrin,* and *Albumen.*

Hæmatin.—It is owing chiefly to the presence of this substance in the blood, that the chemical tests are available. This pigment, discovered by Lecanu, and as yet not fully known, gives to the blood its vermilion colour, and, circulating in the minute capillaries of the body, tints the surface with carnation hue. It is contained within the corpuscles, and it is not possible to obtain it in precisely the same condition in which it exists in the blood. When perfectly pure, it is a pulverulent, amorphous mass, of a rich dark-brown colour, and, *when prepared by sulphate of soda and alcohol*, acidulated with sulphuric acid, is *insoluble* in ether, alcohol, water, fatty and volatile oils, but slightly soluble in chloroform. Sulphuric and hydrochloric acids do not affect it, and water combined with either of these, does not dissolve it. Alkaline solutions readily dissolve hæmatin in different proportions. When chlorine is passed over hæmatin, moistened with water, white flocculi are formed, the chlorine combines with it, and chloride of hæmatin is produced. The brilliant colour of perfectly pure hæmatin is instantly destroyed by contact with hypochlorous acid.

It is essential to know if the characteristics of this pigment, as exhibited in blood stains, are susceptible of such modification, by the presence of other substances, as to make it impossible to discover them by the ordinary methods of procedure. The most important, from the frequent examination required of steel and iron weapons, is to know that rust formed on these can act in this manner.

J. L. Lassaigne,[1] among the earliest contributions to the literature on this subject, gives his opinion, based upon experiments made with blood stains upon instruments of iron and steel, that the results differ according to the circumstances under which they have been placed. When stained instruments are put in a dry atmosphere and an elevated temperature—conditions favourable to evaporation—the stains are in the form of scales, which present no change in the physical properties of blood; but when in a cold and moist atmosphere—the water of the blood combined with that of the air—there will be produced a film of rust, in which it will be impossible to find the physical properties of dried blood.

By experiments made during a period of nearly thirty years, M. Lassaigne[2] has confirmed these results, and says that this difference is owing to the combination of the colouring and albuminous principles of the blood with the peroxide of iron formed by contact with air and moisture.

The discoveries of M. Lassaigne have been fully attested, in a practical manner, by Prof. H. Rose, of Berlin,[3] in the examination of a knife which was supposed to have served to commit a murder. The crime took place during the summer in a wheat field, where the knife was found a long time afterwards. The blade of it was, by the long-continued exposure on the damp ground, so thickly covered with rust that the metallic polish was to be seen in but a few places. A small quantity being removed by scraping, and slightly heated in a test-tube, developed ammonia, which changed moistened red litmus paper to blue, but, when heated strongly, gave out neither a disagreeable odour nor traces of empyreumatic oil. It was a clasp knife, open, likely, in the manner in which it was found, and perhaps the rain had washed away all the traces of blood.

The inside of the handle of the knife was filled with a dark or almost black substance, which immediately after its extraction was soft, but soon became hard and friable. A small quantity heated in a test-tube behaved as dried blood, developed a strong and disagreeable odour, formed an empyreumatic oil, and from the heated residue, by treating it with carbonate of soda, was shown a considerable quantity of Prussian blue.

When a large quantity of this black substance was treated with cold water, it *did not impart a red colour to the solution.* The digestion was continued for a long time, and aided by a gentle heat, but not sufficiently high to coagulate a solution of albumen, without change. After filtration, a very slight trace of albuminous substance was detected. When this black substance, treated with water and caustic potassa, was boiled, the solution assumed a greenish colour, the filtered liquor showed the characteristic dichroïsm, and acted with reagents exactly as a solution of colouring matter of the blood with caustic potassa. This solution, digested with

[1] Archives Générales de Méd., t. viii. 1825, p. 289.

[2] Annales d'Hygiène pub., 2me sér. t. v. 1856, p. 206.

[3] Vierteljahresschrift für prak. Phar., III. B. 2 Heft. s. 209.

hydrochloric acid, dissolved a considerable quantity of oxide of iron, which, after supersaturation with ammonia, fell in the form of a precipitate.

The black substance in the handle was, therefore, chiefly composed of dried blood and oxide of iron, the latter formed as rust upon the iron which lined the sides of the knife. On account of the presence of a large quantity of oxide of iron, the dried blood lost one of its chief peculiarities —solubility in cold water. By a series of comparative experiments, Prof. Rose found that the colouring matter of dried blood is completely precipitated from its solution by hydrated oxide of iron.

A proof of the accuracy of these observations was given by a closer inspection of the knife, when a little piece of wood was seen in the inside, which was probably placed to prevent the point of the blade from striking on the handle. This piece of wood, particularly at the end of it, was covered with stains of blood which probably had not come in contact with the rust. It was transferred to a test-tube containing water, and, after a short time, from the wood, red streaks could be observed falling to the bottom, whilst a flocculent, voluminous matter, of a reddish tint, remained upon the wood, which became whiter the longer the action of the water was continued. The red matter thus obtained proved, by experiment, to be identical with that of blood.

After a long series of experiments, Prof. Rose came to the following conclusions:—

1. When freshly-prepared hydrated oxide of iron is digested for twenty-four hours, and at the same time frequently shaken, at a low temperature, with a solution of a blood stain, the filtered solution contains no colouring matter of blood; whilst, by boiling the residue of the filtration with solution of caustic potassa, it is easily detected by the proper reagents.

2. If, in place of hydrated oxide of iron, calcined oxide of iron is treated with a like solution of colouring matter of blood, a considerable quantity will be extracted.

3. Hydrated alumina acts upon blood in precisely the same manner as hydrated oxide of iron, but requires a larger proportion for the same quantity of blood.

4. The detection of colouring matter of the blood is more difficult when it has been allowed to fall upon ground composed of rich garden mould. A weak solution of the colouring matter of the blood with earth of this kind was allowed to digest for several months, when it was found the filtered liquid was colorless, which, evaporated on platina foil, left a slight residue containing no trace of blood. This soil, boiled afterwards with solution of potassa, gave a dark-coloured solution, which was dark-brown after filtration, but, on account of the large quantity of earth taken up by the alkali, it did not show the dichroïsm which is peculiar to a solution of blood stains with potassa. The solution of earth with potassa, when saturated with acids, forms brown precipitates resembling those formed without

blood. In order to recognize the presence of blood in such an alkaline solution of earth, it is saturated with an excess of concentrated chlorine water, when white flocculi appear as in an alkaline solution of blood, whilst in a solution of earth and potassa no such flocculi are seen. If the blood is concentrated, which falls on the ground, these difficulties are not found.

The extended researches of Rose have lately been verified by Dr. G. C. Wittstein,[1] of Munich, who was required to examine several articles of clothing, and an axe with its handle, found in the dwelling of a man supposed to be the murderer of a woman, whose body was found in a forest with several wounds on the head and a severe fracture of the skull. The axe and handle appeared to have been washed with some care, and but few stains were noticeable, which, treated, did not furnish satisfactory results. The handle was carefully removed, when it was found, as conjectured, that blood had flowed between the wood and iron. The blood, mixed with rust, in this situation, was found to be insoluble in water, proving the assertions of Lassaigne and Rose to be correct.

To avoid error in the examination of blood stains when mixed with rust or hydrated oxide of iron, I propose the following formula, and by this means, at the same time, add an additional test for blood, based upon the property, which Verdeil[2] has shown hæmatin possesses, of forming, in its alcoholic solution, a lake which is insoluble in a mixture of alcohol and water. Scrape off the mixture of rust and blood, add thereto the smallest quantity of soda, and, with water, make into a thick paste, which transfer to a test-tube containing alcohol; boil for a few minutes, and filter. The filtered liquor contains the colouring matter in solution, which, upon the addition of quicklime in fine powder, falls in the form of a *green* precipitate. For convenience, the solution may be divided in two portions, one of which can be submitted to the ordinary tests for blood. The specific chemical character of the colouring matter of the blood, compared with that of pure hæmatin, varies slightly, owing to the presence of the salts contained in the former.

In the year 1829, M. Morin, of Rouen, was called before a tribunal to decide, whether certain red stains found upon clothing were produced by human blood, or owing, as pretended by the accused, to fish blood, when he asserted that stains produced by the blood of fishes cannot be confounded with those made by the blood of mammifera. He founded this extraordinary assertion on making some experiment upon the blood of the salmon, by which he found, that acting upon it with sulphuric acid, and supersaturating the latter with magnesia, then treating the coagulum thus formed with boiling alcohol, the colouring matter was dissolved, whilst that of the blood of the mammifera is completely *insoluble* in that vehicle.

[1] Vierteljahr. für prak. Pharm., V. B. 3 Heft. s. 382.

[2] Traité de Chemie Anat., t. iii. p. 383.

It has been shown that M. Morin fell into a grave error in his research, by M. Lecanu,[1] who not only exposed the mistake but used the same formula to isolate the hæmatin of human blood.

Iron is found in many parts of the body; in several secretions, normal and morbid, and in the blood, where it is intimately associated with hæmatin within the walls of the corpuscles.

Upon superficial examination it would appear that the iron in the blood is in such small proportion that it could not embarrass the chemical analysis of blood stains.

M. Persoz, Prof. of Chemistry at Strasburg, communicated to M. Orfila that, in the year 1836, to recognize blood-stains, he had recourse to hypochlorous acid, which, he said, immediately destroyed all other stains except those formed by rust or blood, which become dark brown by contact with this acid. After receiving this information, the latter with Mr. Cottereau, applied this knowledge practically, and recommended this reagent to MM. Magouty and Loust, of Bordeaux, who were charged with the examination of stains found on the lining of a vest. During the investigation, these gentlemen found that direct stains, or those made by a jet, or by dipping cloths in blood, were different in their action from those produced by contact with a stained body.

From a great number of experiments made on the action of hypochlorous acid, prepared after the formula of Balard—by shaking pure chlorine gas with binoxide of mercury, moistened with water—upon various substances, M. Orfila[2] concluded:—

1. That stains made by a mixture of fat and alkanet, fat and charcoal, and madder and oil of poppies, behaved almost in the manner as blood stains.

2. Hypochlorous acid is completely inefficacious to distinguish blood-stains from those made by rust, colcothar, and fat, because the latter remain after a prolonged action of the acid; but they disappear, as Persoz has shown, by the use of a solution of chloride of tin, while blood-stains were unaffected by it.

3. That hypochlorous acid is altogether incapable of establishing positively, that a stain is formed of blood, though it can be employed as an accessory means, provided it remains in contact with parts stained, but one or two minutes.

Brame, who performed these experiments, thought that the hypochlorous acid should be perfectly free from perchloride of mercury, as it is easily obtained by Williamson's method, by agitating fresh chlorine with peroxide of mercury. The same author advises removing the stains with faintly alkaline solutions, and then performing the experiments in a test-tube.

[1] Annales d'Hygiène pub., t. ix. 1833, p. 226.
[2] Annales d'Hygiene pub., t. xxxiv. 1845, p. 112.

Buchner states that the presence of mercury does not interfere in the least with the reaction of the acid, and that chloride of lime, chloride of soda, and an addition of muriatic acid, may also be employed.[1]

I have tried the above experiments with hypochlorous acid made after the method of Balard, Williamson, and Pelouze (the last, by passing dry chlorine over precipitated *red oxide of mercury*, when chloride of mercury and hypochlorous acid are formed; the latter of great purity and concentration), and found the results agreed perfectly with those of Orfila, &c.

Above, I have shown that hæmatin when exposed to the action of hypochlorous acid, is completely decolourized, then to what constituent of the blood is the reaction of this acid owing, and how can the resemblance of its action on blood, colcothar, and rust be accounted for? To the presence of iron. This is easily seen by repeating the following simple experiment of Dr. F. F. Runge,[2] who has shown that iron can be detected in the most minute particle of blood.

A single drop of blood is received on a linen rag, which is then plunged into a solution of chlorinated lime, which contains hypochlorous acid in small quantity; the red colour is soon changed to yellow and afterwards to a dark brown. The cloth is now carefully washed with pure water, to remove all traces of lime, and the spot treated with an acid (acetic) solution of ferrocyanuret of potassium, when it will be rapidly changed to blue—ferrocyanuret of iron.

To detect the presence of blood by the iron contained therein, when the fabric has been washed, Verghauss[3] has devised the following plan, which, he says, is capable of doing so, indubitably even with the smallest trace of blood. The portion of stained cloth to be examined, is calcined in a platina capsule, the cinder treated with pure sulphuric acid, and the extract tested for iron.

Nitrogen.—This substance, with which we are surrounded on all sides, exists in the body of animals in a gaseous and solid state. Combined with two equivalents of carbon, it forms one of those quasi-simple radicals, cyanogen (C_2N), which has the property of producing salts. The azotized matter of the blood, when submitted to a high temperature, is capable of yielding this in a state suitable to form bases.

C. Wiehr[4] has taken advantage of this and adopted a method to prove blood-stains on coloured stuffs, where the solution, tinged by their colour, does not permit the use of reagents; which consists in the generation of cyanide of potassium from the blood-stains on the fabrics. After having convinced himself of the absence of wool in the cloth, he calcines a red-

[1] Liebig's Annale. American Journ. of Pharm., N. S. vol. xiii. p. 319.
[2] Grundrisz der Chem., Th. II. s. 221.
[3] Cannstatt's Jahresbericht für 1845, 1er B. s. 116.
[4] Handbuch der gericht. Med. von J. L. Caspar. 1857.

coloured, stained piece of the stuff in a porcelain crucible, pulverizes the residue, mixes the powder with carbonate of potassa and heats the mixture to redness. The mixture is then extracted with water, and to the filtered solution, a small quantity of solution of the salts of protoxide and of sesquioxide of iron, is added: a precipitate of undefined colour is produced, containing the constructed ferrocyanuret (Eisencyanür-Cyanid) of iron and protoxide and sesquioxide of iron, precipitated by the excess of carbonate of potassa used in the process. Dilute sulphuric acid is now added, which dissolves the protoxide and sesquioxide of iron, and leaves behind the ferrocyanuret of iron, undissolved, showing now its blue colour. The operation is said to be successful, if a piece of the stained stuff is boiled in caustic lye; the liquid evaporated to dryness and the residue treated in the same way with the salts of iron and sulphuric acid.

Dr. Wolff[1] employed the process, with a successful result, in the examination of blood-stains which had remained on linen for a period of twelve weeks.

Fibrin.—This protein body is found in animals in two conditions, solid as found in muscle and liquid, and perhaps more natural state, in blood and various fluids. When blood is allowed to stand, spontaneous coagulation takes place, and soon after, owing to its presence, a mechanical analysis is found separating the serum from the solid portion.

M. P. Denis[2] was the first person to point out some singular properties possessed by fibrin and its similarity to albumen and casein. By maceration in water containing a neutral salt, for instance, nitrate of potassa, for twenty-four or forty-eight hours, or even longer, according to the proportion of the salt, it will be dissolved. The new product resembles serum and albumen; it precipitates bichloride of mercury, and is coagulated by heat and alcohol. If this saline solution is diluted with water in large quantity, the fibrin will reappear with all its original properties.

The characteristics of fibrin differ, in some degree, from the different sources whence it is derived, and it is held that a difference exists between that of venous and arterial blood. When the former is triturated in a mortar with $1\frac{1}{2}$ times its weight of nitrate of potassa and the mixture is left for twenty-four hours at a temperature of 100°—120°, it becomes gelatinous, slimy, and eventually liquid; in this condition it exhibits all the properties of a solution of albumen, which has been neutralized by acetic acid. With arterial fibrin no such liquefaction happens, and even the fibrin of venous blood, when long exposed to the air or oxygen gas loses the distinction.[3] This discriminative quality belonging to fibrin as obtained from fresh blood, could scarcely be made to serve any purpose in a question as to the origin of blood-stains.

[1] Canstatt's Jahr. ber. für 1853, 1er B. s. 15.

[2] Archives Gén. de Méd., t. i. 3me ser., 1838, p. 171.

[3] Liebig, Handwörterbuch der Chem. I. B. s. 881.

When a piece of any material stained with blood to a notable degree, is suspended in water, the colouring matter is quickly dissolved, leaving the fibrin in the form of a grayish mass, with a slight reddish tinge, but which becomes white after continued maceration. This can be removed and submitted to microscopical inspection to determine its identity.

There is one circumstance where it is of the highest importance to be able to discover traces of blood, in which M. Morin[1] thinks he has succeeded. The assassin, in his haste to destroy that which is frequently an essential portion of the evidence against him, washes his clothes with boiling water, sometimes even with the addition of soap, with a view of hastening the disappearance of this indubitable evidence of his crime; whence results the fixation of certain matters of the blood on the tissue. M. Morin experimented with tissues stained with blood, by boiling in water with soap, when it was found the stains were duller than before. Their consistence was always greater than that of the tissue itself; the washings had not perceptibly dissolved the elements of the blood. After reaction for some time with solution of caustic potassa a liquor is obtained, which is precipitated white by nitric or pure hydrochloric acid, which indicates the solution of one or more of the (protein) matters of the blood. By this alkaline treatment the stain loses some of its colour, but what, then, is the matter which is found in some measure indelibly fixed on the tissue? To solve this question, it is only necessary to put the stained tissue in contact with pure hydrochloric acid, which dissolves the matter of the stain, and forms a solution which, carefully reduced to dryness, furnishes a residue having the property of acquiring a very clear blue colour, with ferrocyanide of potassium, and a blood-red colour, with sulphocyanide of potassium, indicating the presence of iron.

Fibrin has the property of attaching itself to the texture of clothes. Sulphuric acid has the property of dissolving textures made of hemp or linen without altering the fibrin. If, then, a texture of this sort is suspected of being stained with blood, it is to be plunged into concentrated sulphuric acid, which dissolves the texture and leaves the fibrinous part of the blood, presenting a network, where may be distinguished the impressions made by the texture on which the blood was fixed.[2]

Albumen.—In an examination based upon the chemical constituents of the blood, it is necessary to inspect the character of this substance, which is presented in large proportion in the blood, and, like fibrin, variable in quantity in different morbid conditions. The presence of albumen in any fluid can only be proved by its coagulability with heat and nitric acid, because the coagulation by heat can be prevented by numerous substances, both acids and alkalies.

[1] From Journ. de Chim. Méd. Annual of Sci. Dis., 1855.
[2] Lancet, 1848, vol. ii. p. 18.

When blood-stains have been submitted to the action of water, the albumen is dissolved, and, with the colouring matter, remains in solution. When the solution is heated to 145°, coagulation is seen to take place, and, at the same time, decoloration of the red pigment, one of the chief distinctions of blood. From a number of experiments which I have made upon the decoloration by heat, there appears to be some analogy between it and the coagulation of albumen. If chloride of zinc, corrosive sublimate, tannic and arsenious acids are added to a solution of blood-stains, it is changed to a bright-red colour, and, when heat is applied, the decoloration takes place as usual; but when those substances which are known to prevent the coagulation of albumen—such as potassa, soda, lime, baryta, and tartaric, acetic, gallic, citric, oxalic, benzoic, and meta-phosphoric acids—are added, there is no decoloration produced by heat, and, after boiling with these reagents, liquors are obtained of various colours and tints. Thus far the analogy is complete; but when sulphuric acid is added to it, coagulation takes place, but no decoloration is found from the application of heat. The fact that decoloration of a solution of blood by heat may be prevented by the action of various articles, should be remembered by the experimentalist, for frequently attempts are made, with different substances, to destroy the evidences of guilt, which might render invalid the test (3) described above, and produce confusion in the result of the analysis.

H. Zollikofer[1] was a short time ago required to examine whether certain reddish-brown stains, which were found upon a knife, a pair of scissors, on linen, wood, and on the ground, were owing, in a greater or less degree, to the presence of blood. The character of the stains required the employment of a very delicate test in order to obtain reliable results, and which was not possible but by adopting as the point of departure the method of H. Rose. His attention was directed chiefly to two of the principal constituents of the blood, viz., albumen and hæmatin. By means of his experiments he found a new and specific reaction of hæmatin, and he is of opinion that by this means he is enabled to generalize the process of H. Rose, which, according to him, is the only one which can be advantageously employed.

When it is required to examine stains upon rusted iron, two varieties are presented for consideration, according to the circumstances under which they have been placed, viz., that where the blood has remained *less* than a month in contact with the rust, and the other where the two substances have been mixed for *more* than a month.

A. *Less than a Month of Contact.*—The rust is carefully scraped into a small porcelain capsule, and allowed to digest for some minutes in cold or slightly warm water. The filtered liquor will then contain the soluble salts of the blood, albumen and hæmatin.

[1] Journal de Pharmacie, t. xxviii p. 209.

1. The solution is now heated to ebullition. According to the proportion of hæmatin and albumen, a dirty-reddish coagulum or a simple opalescent cloud is formed. The liquor being most alkaline, it is necessary to neutralize it with dilute acetic acid.

2. When the coagulum is dissolved in caustic potassa, the hæmatin liquefies in such a manner as to render the solution dichromatic, green by transmission, and red by reflection.

3. By adding chlorine water in excess either to the dichromatic liquor or to the simple solution (No. 1), there are formed white flocculi (albumen and chloride of hæmatin), which soon separate to the surface of the liquid.

The reaction of No. 2 is indicative of hæmatin alone; the others indicate, at the same time, hæmatin and albumen. When the quantity of blood is very small, the dichromatic appearance is not manifested, even when the chlorine water still produces a perceptible precipitate. In such cases, and to dispel doubts regarding the reagents of H. Rose, he advises to recur to the following consideration, viz: hæmatin is the only substance which contains iron, and, according to Mulder, $C_{44}H_{22}Az_3O_3Fe$. When hæmatin is dissolved—or, rather, simply suspended—in water, if it be acted on by a current of chlorine, it is precipitated in the form of white flocculi, and loses its iron, which remains in solution in a state of chloride. It is only necessary, in order to detect the latter, to use sulphocyanuret of potassium, which, as is known, is its most reliable and delicate reagent. In operating in this way upon a blood-stain which was only two lines in diameter, this chemist has obtained a manifest reaction by the use of sulphocyanuret of potassium, although chlorine water gave simply a whitish cloud, hardly appreciable, which required several hours to precipitate as distinct flocculi.

B. *More than a Month of Contact.*—When blood remains a long time in contact with rust, there is formed, as Rose has shown, a veritable combination, by which hæmatin is rendered insoluble in water. In boiling this compound, it is necessary to avoid a large excess of the alkali, because the saturation will become more difficult.

When this method is employed, it is required to discover, in the first place, if no soluble salt of iron is contained in the spot to be examined. This is easily done by means of sulphocyanuret of potassium, which is agitated with a simple aqueous solution of the stain before it is submitted to the alterative action of potassa. If this reagent detects the presence of iron, two experiments are needed: one which consists in treating the aqueous solution by chlorine, to detect the presence of hæmatin and albumen; the other, by treating the spot first with pure caustic potassa, and supersaturating the solution with chlorine. The iron, separated from the hæmatin by this means, will be found in the product of filtration.

To show that a stain contains iron, cyanogen (materials from which it can be procured), fibrin, or albumen, gives no positive proof of the presence

of blood, since these substances are found in many animal and vegetable structures, in various forms and combinations; but to be able to detect them is a valuable auxiliary means to confirm chemical analysis. Of the various tests and methods for the detection of blood, those which are based upon the characteristics which hæmatin possesses are alone of specific value, and in medico-legal investigation should first occupy the attention of the expert. The reaction of hæmatin is the same, whether obtained from arterial or venous blood, and chemistry affords no means of discrimination between these two forms of the vital fluid.

Menstrual blood, in its normal condition, would appear to contain no fibrin, as has been proved by Dr. Letheby,[1] who had an opportunity of examining forty ounces of it in a case of imperforate hymen, and Jul. Vogel, who procured it in a state of purity in a case of procidentia uteri.[2] The instances are frequent in which menstrual fluid possesses all the physical properties of blood, and hence they might be confounded. Nevertheless, where a charge of rape or infanticide has been preferred, and blood-stains produced as evidence of the deed, which, on examination, were found to contain no fibrin, the absence of this important constituent of ordinary blood would have some weight in favour of the accused.

To distinguish Human from Animal Blood.—Several methods have been proposed to distinguish, by chemical means, human from animal blood, and the blood of one class of animals from that of another. The most remarkable of these is the plan elaborated by M. Taddeï, of Florence, under the name of *hæmatalloscopy*,[3] which is as follows:—

The spots upon weapons, soil, or on furniture, are detached by scraping, the product is weighed in a delicate scale, after which the smallest possible quantity of distilled water is added, and to this a solution of crystallized bicarbonate of soda, containing the same quantity, by weight, of the salt as is represented by the weight of the blood. If the liquid has been deposited upon a fabric, it should be separated by water; and, to determine the quantity of it, it is dried at 60° Cent., the cloth to be cut in strips, after which they are macerated in water, or, better, triturated in a mortar with water, and on drying them, and weighing again, the exact quantity of blood will be found, to which the soda, as before, must be added.

A fabric of linen or cotton, which contained hardly 28 to 30 centigram. (5 to 6 grains) of dried blood, furnished a quantity sufficient to determine its nature.

After having well shaken the blood with the solution of the bicarbonate, a solution of the sulphate of copper, in very slight excess, is poured into it, and, after ten or twelve hours of repose, the mixture is filtered and

[1] Todd and Bowman, Physiol. Anat. and Physiology of Man, Am. ed. p. 848.

[2] Lehmann's Phys. Chemistry, vol. i. p. 631.

[3] Manuel de Méd. Lég., p. 795.

washed with care. The filtered liquor is bluish, and the product found in the filter, which is of an olive-green colour, contains the organic substances and the carbonate of copper. The filter is now placed upon bibulous paper, and dried in the sun or in a stove, between two porcelain plates or capsules; the product is detached and triturated in a porcelain mortar before desiccation is completed. M. Taddeï designates this product *powder of interposition.* As this powder is exceedingly hygrometric, it is necessary to protect it from the moisture of the air.

When it is required to determine if a certain quantity of blood belong to man or a vertebrate animal, it is effected by comparison. Ten grains of powder of interposition are accurately weighed, to which are added, in the same capsule, fifteen grains of dilute sulphuric acid, formed with equal parts of acid at 66°, and of water, a mixture to which the author gives the name of *acid liquor.* The capsule is covered with a glass plate, leaving only room at the side for the movement of a glass rod for mixing well the acid and the powder. In operating at 25° or 30° Cent., the powder of interposition, hardly moistened with the acid, changes to olive-green or garnet-red, and from being granular, as it was at first, it now becomes homogeneous, tenacious, pulpy, plastic, and very elastic.

This product, deposited upon a large horizontal sheet of glass, remains in the same state for ten or twelve hours, after which it spreads, adheres to the surface of the glass, becomes shining, and assumes the bright appearance of a melted mass. This appearance is shown upon the lower side of the mass, after four or five hours in summer, and longer in winter. The whole falls lower and lower, the area extends, becomes ordinarily circular, and the substance softens, taking the consistence of an extract. If the continuity is destroyed by means of a glass rod, the rent will soon be filled and the elevations disappear; by gently applying a metallic seal, or a piece of money anointed with oil, the impression is only momentary, and the mass soon regains its first form; on touching it with the finger, it adheres thereto like honey; bibulous paper applied with care to the surface, cannot be elevated without raising some of it, and insects which fall upon the substance remain attached as long as the fresh paste not only can be touched with the finger or blotting paper, but till it can be compressed without adhering. The fluidification increases progressively, the product becomes semi-fluid, and, on inclining the pane of glass to from 20° to 40°, it flows from 80 to 100 millimetres in three or four hours. All the above phenomena are manifested in the space of a day and a half, at a temperature from 25° to 30° Cent., and the fluidification becomes such that, in the space of thirty or forty hours, by inclining the glass to 45°, the mass travels 135 to 160 millimetres in a short time; in fine, after three or four days, the fluidification is complete. In using a rectangular sheet of glass, with a graduated scale upon one of its sides, it is easy to determine the degree of fluidity during a certain period of time at a given inclination.

If the pane of glass upon which the product has been placed is left horizontal until the mass be completely liquefied, it retains its opacity, but becomes so brilliant, and reflects all objects so well, that one can see, as in a mirror, all the points of any body which is presented. If, then the sheet of glass is turned vertically, and under it a horizontal one, the mass falls on the latter, leaving hardly any trace on the former, so that the objects are delineated behind the vertical sheet in the whole course of the product, which can be made to flow again in the same manner. When the paste is laid on a pane of glass exactly horizontal, after a few days another phenomenon is observed. In the area occupied by the fluid mass, two substances are shown; one solid, granular, whitish, and opaque, the other liquid, diaphanous, of an amber tint, which separates to the periphery, enveloping on all sides the opaque substance, and forming a zone of eight to ten millimetres, with fringed edges. The better to note this effect, the glass should be placed before a window. These substances are easily separated in the following manner: the tare is taken of a trapezoidal, or, better, a hexagonal plate of glass, to which is attached, with a little sealing wax, a very fine brass wire; to this is fixed a piece of filtering paper, cut in a hexagon, a little smaller than the plate of glass, which has been weighed; by means of a pipette, some of the acid liquor is allowed to fall upon it—in quantity, a little more than what is necessary to cover the paper, but still not sufficient to flow beyond—and thereon is sprinkled the powder of interposition. Having taken the whole from the scale, the powder is mixed with the acid by means of a glass rod, and, after several days, the plate of glass is inclined so that the liquid portion flows upon another plate of glass, and is spread on a printed sheet, on which the letters can be easily read through the liquid. The latter is so transparent, that in passing the sun's rays through it, and receiving the image in a camera obscura, there will be found a circle of a beautiful colour, red as fire, bordered by a colourless circle, which is itself surrounded by another obscure one.

On letting some drops of the amber-coloured liquid fall into alcohol at 78° to 82° Cent., the latter is troubled, and a deposit takes place of numerous filaments, of an albuminous appearance, ashy-white or slightly gray, and the liquid becomes a fawn colour; whence it can be concluded that the amber liquor is a combination of acid and hæmatin, with an albuminous or protein substance. One is soluble, the other insoluble, in alcohol.

If some of the liquid is spread upon a plate of glass with the pulp of the finger, it adheres like a fatty or oily substance. If, now, this plate is put into a tumbler filled with distilled water, so that one of its sides touches the bottom and the other the edge, at an inclination of forty-five to fifty degrees, lines traced by the finger will be seen, and, the vessel being in repose, this substance is observed to unite by degrees with the fluid, and form a mass composed of layers which fall to the bottom. If, now, the plate is

withdrawn from the liquid, it is seen uniformly covered with a layer of a pearl-white substance, which, rubbed by the finger, unite in small opaque filaments of a deep gray colour. The same result is obtained when the extremity of a glass tube, filled with the fluidified matter, is plunged into water and held vertically in the centre. In placing the latter between the eye and the light, from the end of the tube is seen flowing a very fine filament, which, on breaking, forms small wreaths, attached one to another, that, falling slower and slower, and increasing so much in diameter, they lose their colour, and acquire a refrangibility, almost equal to that of water, which does not longer allow them to be seen. Then the filament which at first, coloured and transparent, occupied the centre of the tube, is opaque, and to the wreath have succeeded white flocculi, which rise and fall in the liquid. This white thread, which remains for some minutes intact and mobile, like the filaments, is formed of albuminous substances of the serum.

If the paste resulting from a mixture of powder of interposition and acid liquor, in the proportion of 1 to 1.5, is allowed to fluidify in the bottom of a conical vessel, and steam directed over it, or a little hot water poured upon it before it is completely liquefied, and allowing the same to digest with occasional agitation for some time, the solution operates without any clots or threads remaining. If sufficient carbonate of lime, in fine powder, to saturate all the acid, is poured into this, and the solution afterwards filtered, the liquid presents a very beautiful lilac or blue colour, arising from the oxide of copper. By washing the residue upon the filter until the liquid be scarcely coloured, and throwing some ammonia upon it, a fluid passes through of a deep colour—red by refraction, and greenish-brown by reflection. If the phenomenon is observed in a glass vessel, as the ammonia evaporates, there is deposited a slight layer of opaque matter on the sides of the glass, which, when dried, is of an ashy-gray hue, and dissolves without effervescence in water acidulated with hydrochloric acid. When completely dried, it is in the form of a crust, very friable, and of a bottle-green colour, and which, detached, looks black, and has a metallic lustre. The powder thus obtained is insoluble in alcohol, but is soluble in that liquid and water when thereto is added an acid, or, better still, some caustic alkali. Reduced to paste with one and a half times its weight of acid liquor, it imparts a garnet-red colour, but does not form a coherent mass like that formed with the powder of interposition.

Heat exerts so great an influence upon the fluidification, that the paste made with the powder of interposition and the acid liquor, in the proportion of 4 to 5, and, consequently, hard and dry, becomes not only of a soft, shining, and semi-fluid appearance, but is completely liquefied after a few days, when kept at a temperature from 35° to 40° Cent. On the contrary, if the paste is made in proportion of 1 to 1.5, when at 15° Cent., the mass remains without change, or takes the consistence of an extract.

Characteristics of the Blood of Animals.

Ox Blood.—In operating as above described, the plasticity and coherence are found to be less. The mass which is reduced to clots—elastic, but hard and dry, when placed upon a plate of glass, shows no change whatever after thirty hours, either in summer or winter; preserves its form and diameter; neither assumes the consistence of an extract, nor reflects images; and, after some weeks, loses its shape when the plate of glass is inclined, takes a darker colour, and alters in firmness so much that the clots become agglutinated, and form masses without consistence, and always granular, whence flows a portion of acid liquor.

Pigeon Blood.—The powder of interposition does not mix with the acid liquor so as to form a homogeneous, plastic, and coherent paste; only a mass of hard and tenacious clots is obtained, which, divided and without cohesion, after some days—aided by a temperature of 25° or 30° Cent.—reunite into a sticky, extractiform, and homogeneous mass.

Green Lizard Blood.—This is with much more difficulty detached from tissues, than the blood of any other animal; whence the cause of the almost indelibility of the spots by water. The powder of interposition does not furnish a coherent and homogeneous mass, but only a pile of clots which do not adhere; at first slightly elastic, they become by degrees moist, flabby, and of a deep colour, afterwards take the brightness and appearance of a semi-fluid substance, and, when the agglutination increases rapidly—the temperature being from 30° to 35° Cent.—the clots are united into one brilliant mass, black as pitch, and of an extractiform consistence.

Tench Blood.—The mass formed with the powder of interposition and acid liquor, is formed of small clots without cohesion, which do not form a plastic and homogeneous substance.

In comparing human blood with that of animals of different classes, it is observed that with the first the powder of interposition yields a consistent, elastic paste of a garnet colour, that softens rapidly and falls like dough in process of fermentation, and having become brilliant, extractiform, dark, and pitchy, it liquefies like syrup, forming large spots with fringed edges, when maintained at a temperature from 30° to 35° C., in a horizontal position; that this paste divides spontaneously into two parts, one liquid, diaphanous, of an amber colour, flowing like water; and the other solid, white, and opaque.

Blood is not human when there is formed an elastic, consistent, and tenacious paste, reducible by pressure to fragments which do not agglutinate, neither fluidify by any means, nor furnish two distinct substances, such, for instance, as ox blood, as the type of the mammifera. In like manner with various other bloods distinct differences are found. In fine, it is human blood alone, which, not forming a homogeneous and coherent mass, whatsoever the proportion of acid liquor may be, gives only isolated clots,

not susceptible of forming an emplastic body until several days have elapsed.

To appreciate the degree of fluxility (*fluidifiabilité*) M. Taddeï uses a tube 0.50 millimetre in length, and from 0.006 to 0.008 millimetre in diameter, closed at one extremity and expanded at the other, like a smoke pipe, and curved at an obtuse angle. When the mass introduced remains several hours, it becomes soft enough to adhere to the glass; the tube is then inclined at an angle of 45°, the mass then flows insensibly and, by a graduated scale divided into 200 parts, the distance travelled, after three or four hours, is measured. The different kinds of blood by this means are divided in the following manner:—

Coagulable Blood.	Non-fluxible	Ruminantia (Ox, deer, &c.).
	Tolerably fluxible . .	Rodentia (Guinea-pig, rabbit, &c.).
		Solipedes (Ass, horse).
		Pachydermata (Hog).
		Quadrumana (Monkey).
		Carnivora (Porcupine, pole-cat).
	Very fluxible	Carnivora (Cat, fox, dog).
		Bimana (Man).
		Rodentia (Rat).

The blood of the dog, man, and the rat, are found placed in the same category, and to distinguish them it is indispensable to compare exactly their degree of fluxility.

To attain the same end, a process has been shown by M. Casanti, who uses as a reagent for the distinction of blood belonging to various animals, phosphoric acid at a density of 1.8, in the following manner: The first step was to establish a distinction between an animal belonging to the mammalia and another vertebrate animal, say a bird. For this purpose, both kinds of blood were carefully dried and treated by an excess of phosphoric acid; the mammalian blood became agglutinated, and formed into a brilliant, homogeneous, and coherent mass; whilst that of the bird (gallinaceous) did not present these characters at all. As to man and other mammalia, six grains of finely-powdered, dry human blood, were put into a glass, and nine grains of phosphoric acid added. The blood, on being agitated with a glass rod, swelled and softened, turned into a brilliant mass of a hepatic colour, and as consistent as a common extract, but glutinous and devoid of plasticity. On being pressed with the rod, it yielded without dividing, and became more homogeneous; when allowed to stand it became hard without losing its lustre. The blood of the horse gave very different results. The acid first swelled and softened the powder, but the particles, far from forming themselves into a mass, turned into hard and shining lumps, which did not adhere to each other, and even broke asunder when attempts were made to unite them. The blood of the ox, calf, mule, pig, goat, &c., gave the same results as that of the horse. The blood of the

cat formed a single mass like that of the man, but it broke at the slightest touch. Human blood always exhibits definite characters, notwithstanding differences of age, sex, health, or disease; except, however, as regards catamenial blood, which, although it gathers up into a mass, divides very soon into dry and swelled particles, that show no tendency to reunite.[1]

A simple means of distinguishing the blood of different animals was proposed by Gallicano Bertazzi,[2] who based his researches on the behaviour of iodine water on the contents (hæmatin and blood casein) of blood corpuscles. After preparing a saturated solution of iodine in water, he experimented in the following manner: A circular piece, five lines in diameter, is cut out of the spotted cloth and covered with 20 grains (1.25 gramme) water; when the colouring matter is dissolved, the stuff is removed with the forceps, pressed out and treated with 10 grains of iodine water. A solution of bird's blood prepared in this manner, according to him, will change to reddish-brown, be troubled, and yield an abundant precipitate; a solution with that of carnivora turns reddish without showing any cloudiness or deposit, whilst with that of the herbivora, it only takes on a colour resembling cyprus wine. In order to produce the same effect with the blood of carnivora and human blood, 20 and with the herbivora 40 grains of iodine water must be added. With the blood of birds and carnivora the precipitate obtained is reddish-brown, and becomes red in the air, by degrees resembling cochineal; that of the last (herbivora) at first dark red, becomes changed to chestnut brown in the atmosphere.

By Specific Odour.—For the purpose of discriminating between different kinds of blood, on several occasions recourse has been made to the odour evolved by the addition of sulphuric acid. M. Barruel,[3] in 1829, when trying to obtain some of the colouring matter from ox blood, was struck with the peculiar animal odour developed on the addition of sulphuric acid to fresh blood, and subsequently in treating human blood in the same manner, the smell, resembling human sweat, was so powerful as to drive him from the laboratory. This discovery led him to make several experiments, with the following conclusions:—

1. That the blood of every animal contains a peculiar odorous principle; that of the male strong, of the female like it but more feeble.

2. This principle is exceedingly volatile, and has a similar and peculiar smell of the sweat or vapour of the skin or lungs of the animal under consideration.

3. This principle is intimately combined with the blood, and not perceptible as long as the combination remains, but when this is destroyed the odorous principle of the blood evaporates and develops the characteristic smell of the animal from which it is derived.

[1] Lancet, 1849, vol. i. p. 348.

[2] Annali Univer. di Med. Aprile, 1839.

[3] Annales d'Hygiéne pub., t. i. p. 267.

4. This development is best obtained by means of sulphuric acid. The result is obtained by adding to one part of blood one and a half part (by measure) of concentrated sulphuric acid, and stirring the mixture with a glass rod. There is an elevation of temperature of the mass, during the process, when the peculiar smell is produced.

The researches elaborated by Barruel were in some degree confirmed by Taddei de Gravina,[1] who experimented with the blood of the ox, cow, and very young calf, an old and a very young hare, the goat, sheep, hog, horse, and mare, man and woman, and numerous species of birds. From his various experiments he arrived at the following results: 1st. That it is true that the blood of every vertebrate animal has in it an odoriferous principle, identical in all individuals of the same species, and similar to the odour of the cutaneous transpirations, or more properly speaking, of that part of it which gives to each animal its characteristic smell. 2d. That the notion of those who pretend to recognize to which, among a number of individuals of the same species, a given portion of blood belongs, is false.

Carl Schmidt[2] made numerous experiments upon the odour of the blood of man, the dog, cat, calf, sheep, hog, goat, cat, besides those of hens and frogs, together with that of various animals of the different classes. To avoid the difficulties in this kind of investigation, the examination took place in the presence of six intelligent persons who were requested to communicate their decision. During the trial they detected many kinds of blood with great accuracy and agreement, but throughout there was not much constancy in their opinions. In fine, that the method of Barruel is, under all circumstances, characteristic only with the blood of the goat, sheep, and cat, whilst with all the others very doubtful results are given.

A case occurred in which a man was charged with an assassination, at whose house was found a bucking cloth presenting many grayish stains. It was required to decide if these spots were owing to human blood, or as the prisoner contended, they were produced by meat which he had enveloped in the cloth the year before to shield it from the flies. MM. Chevalier and Barruel were charged with the examination, and among other tests used that by odour. After making a solution of the stains, concentrating it at a low temperature, and mixing this with concentrated sulphuric acid, a slight odour resembling the smell of mutton was eliminated. While this opinion went far to admit the declaration of the accused, these gentlemen stated before the tribunal, the difficulty of deciding by this means, and the insufficiency of their evidence, requiring the court to rely upon other testimony for a verdict.[3]

[1] British and Foreign Med. Review, vol. ii. p. 226.

[2] Die Diagnostik verdächtiger Flecke in Criminalfällen. Mitau und Leipzig, 1848, s. 19.

[3] Annales d'Hygiène pub., t. x. 1833, p. 160.

Some few years ago MM. Tardieu, Barruel, and Chevalier, were required to decide if blood found in the house of a woman, who was accused of murder, was human, or, as she said, that of a sheep. The substance in question was submitted to the action of sulphuric acid in order to produce the peculiar odour. The disagreement between these experts was so great as to produce complete confusion in their own minds, and to destroy the confidence which had for a time been reposed in this test.[1]

It would be of the greatest value to be able by any means, however difficult or tedious, to distinguish the blood of one animal from that of another. Barruel[2] himself has admitted that his formula does not afford the certainty required in medico-legal investigation, and even for an approximation to a satisfactory result a large quantity of fresh blood is needed. In the majority of assassinations a great length of time elapses between the commission of the deed and the examination of the suspected stains, and the quantity of blood presented is exceedingly small. To succeed by Barruel's method would require a more exquisitely cultivated sense of smell than is possessed by many persons. The difficulties and embarrassments by which this formula is surrounded, render it, for almost all ordinary cases, nearly useless, and in the few in which it can be employed, it holds a doubtful position. There have been many experiments performed according to the method of Barruel, with different kinds of blood, and the results of the observations by Couerbe, Leuret, Rudekind, Erhard, Merk, Soubeiran, Denis, and Chevalier, have shown, by the present means it is not possible in a medico-legal investigation, to distinguish human blood from that of animals by the odour evolved from that fluid.[3] Dr. Alfred S. Taylor takes occasion to say that after many trials with Barruel's process, he could come to no other conclusion than that it furnishes no criterion whatever, and that it would be dangerous to rely upon it in any case.[4]

Stains which may be Confounded with Blood-Stains.

The substances producing stains resembling, in a greater or less degree, those produced by blood are exceedingly numerous, and it is very difficult to classify them satisfactorily. The appearance and reaction of stains made by metallic and vegetable dyes are often so modified by the presence of mordants or other chemicals as to be difficult to recognize, and on this account each medico-legal examination, to a certain degree, must be studied by itself according to the circumstances which are presented.

Insoluble in Water.—From the chief characteristic of blood-stains—

[1] Annales d'Hygiène pub., t. xlix. 1853, p. 413.

[2] Annales d'Hygiène pub., t. xxiii. 1840, p. 396.

[3] Journal de Chimie Méd., tom. v. 2me ser., p. 493.

[4] Remarks on the Trial of Thomas Drury. Guy's Hospital Reports, vol. vii. p. 372.

solubility in water—the substances of this class are readily distinguished. The principal of these are dragon's blood, Venetian red (Indian red), red ochre (Spanish brown), vermilion, alkanet, precipitated carbonate of iron, colcothar, and iron rust.

Soluble in Water.—Madder. This substance, used very much in dyeing, imparts to water a brownish colour, which is made crimson by addition of ammonia, and yellow by sulphuric and muriatic acids. This dye has obtained more notice since M. Raspail,[1] in a memoir read before the Académie Royale, contested the value of the chemical characters of blood, and, according to him, these properties very well suffice to prove that a stain of blood is not a stain of rust, citrate of iron, cochineal, madder, &c., but they are not sufficient to show that the stain is truly of blood. He gave as proof, that having spotted linen and glass with white of egg which had remained for several hours in a linen bag with madder in powder, which had been previously moistened, the stains behaved towards reagents in the same manner as it had been said blood-stains reacted. These assertions of Raspail were completely refuted by M. Orfila,[2] who showed the difference of reaction by several means, and among them:—

1. That solution of alum and proto-chloride of tin only dilute the colour of blood, while the mixture of albumen and madder is rendered yellow by their solutions.

2. That the colour of a mixture of albumen and madder is not destroyed by heat, when boiled together, as is seen constantly in dye-houses, where madder would be nearly useless if its brilliancy were so easily injured.

In comparative experiments with a mixture of albumen with madder and colour of the blood, it is necessary to be careful that the latter contains no agent which prevents the loss of colour by heat, for, by neglect of this, the discrimination may be doubtful.

Sanguinaria readily yields its colour to water and the solution bears a great similarity to blood. Its solution is coagulated and rendered bright crimson by addition of sulphuric and nitric acids, and completely decolourized by the addition of ammonia and potassa.

Brazil Wood.—The colour of an aqueous solution of Brazil-wood is a dark brown, and is made crimson by ammonia, sulphuric and muriatic acids, and is deepened by bichromate of potassa. Logwood, the dye most frequent in domestic use, readily yields its colour to water, and the solution, which is a dark brown inclining to purple, is deepened by ammonia, reddened by sulphuric acid, instantly changed black by solution of sulphate of iron, and blue by acetate of copper.

Camwood and Red Saunders are but slighly soluble in cold, more soluble in hot water, and rapidly so in acetic acid, alcohol, ether, and alkaline

[1] Archives Gén., 1828, t. xvi. p. 299. [2] Ibid., p. 161.

solutions. The colour is rendered violet-red by ammonia and potassa, and rose-red by sulphuric acid.

Archil (Cudbear). Fruit stains, and the stains of plants and flowers, are changed to blue and green by ammonia.

Sulpho-Cyanate of Iron, mixed with albumen, gives to water a striking resemblance to a solution of blood, but is instantly decolourized by addition of ammonia.

Stains soluble in Water and unchanged by Ammonia.

Citrate of Iron.—When blood in a thin stratum is dried upon a porcelain plate, it shrinks and forms scales which are not unlike this preparation. The colour of the solution of citrate of iron is deepened in the same manner as blood, but is quickly destroyed by addition of sulphuric acid which decomposes the salt.

Anatto, dissolved in water, imparts an orange-yellow colour to water which is not decolourized by heat. The stain produced by this colouring matter is instantly changed to a dark blue by addition of sulphuric and nitric acids.

Catechu, Rhatany, and Kino.—These substances form with water dark-brown solutions, and owing to the presence of tannin in large proportion, they can be with ease distinguished from blood. By the addition of a salt of iron, the latter becomes green, while, with the first two, the solution is changed to black.

By a careful and minute comparison of the action of the various colours simulating blood, when treated by the different reagents, with the characteristics of blood under the same circumstances, it is seen that as yet no substance has been found which cannot be distinguished from it without difficulty, where sufficient care and accuracy have been regarded in the experiments.

II. MICROSCOPICAL EXAMINATION OF BLOOD-STAINS.

For some years past, the microscope has been resorted to, in order to distinguish fluids resembling blood, reddish stains of a suspicious character, and the blood of one animal from that of another. In no kind of investigation has the use of the microscope met with less opposition, and its merits so rarely called in question as in medico-legal evidence. Without this instrument many criminals would have escaped punishment, for, by its use, evidence, which was surrounded by doubts and difficulties, has been made substantial and clear, and frequently, things that were supposed of no moment, by its employment have been found connecting corroborative circumstances, which, till then, seemed to have no relation whatever. The rapidity and accuracy with which blood can be recognized in a microscope of good construction, commend themselves in a question requiring all possible certainty for its solution. The microscope not only furnishes the

means of examining and defining objects invisible to the eye, but enables us to make, with facility, microscopico-chemical researches of the highest importance to the chemist and physiologist.

It is needless to add, that familiarity with the instrument, and the substances to be examined, is requisite to derive the advantages which it possesses.

BLOOD.—The blood is a slightly tenacious fluid of a specific gravity of 1.055, and, when drawn from the vessels, presents to the eye a bright cherry-red colour, uniform in aspect, and separating, after a time, into crassamentum and serum. When examined in the microscope, its peculiar bodies, the corpuscles, are seen floating in the *liquor sanguinis*, giving it the appearance of an emulsion, which, when the blood is traversing the vessels, permits us to note the course and its rate of movement. In the blood, three different forms of bodies have been discovered constantly present—the red globules or corpuscles, the white, and a smaller kind called molecules. For these, many different names have been given by distinguished observers, but here I shall retain those in common use.

Red Corpuscles.—The red corpuscles, which bear the colouring matter of the blood are, in man, shining, circular, slightly bi-concave cells, without a nucleus, and so numerous that to observe them satisfactorily, the blood requires to be diluted in some degree, or spread in an extremely thin layer. The colour of a single corpuscle, when viewed by transmitted light, is a dusky-yellow, but when piled one above another in mass, they are red. When the corpuscle lies on its edge, a bright-yellowish line only is seen. The diameter of a human corpuscle averages, according to the measurement of Hassall,[1] about $\frac{1}{3500}$ of an inch, and sometimes they can be found much larger. The number of corpuscles which exceed in size or fall below the standard, has been estimated by Carl Schmidt, to be only two per cent., so that the deviation scarcely affects the appearance presented to the eye in the field of the microscope. During the coagulation of the blood, the globules form into piles resembling coins, and by desiccation the walls shrink upon their contents, giving them a stellar form; the same result occurs when blood is mixed with urine and allowed to remain for some hours. The membrane of the corpuscle is very elastic, exceedingly delicate in structure, and is quickly affected by the action of reagents. In common with animal membranes, the corpuscles possess, in a high degree, the physical property discovered by Dutrochet, of endosmosis and exosmosis, which renders them susceptible of rapid change in size and shape, and consequently in diameter, on the addition of a fluid having a different specific gravity from that of the blood, that demands an attentive consideration from the medico-legal examiner.

[1] Microscopic Anatomy (Am. ed.), vol. i. p. 91.

On the addition of a small quantity of water, the corpuscles become thicker, and when more water is added they swell, assume a spherical form and burst, discharging their contents, leaving behind a circular line, as it were, its border. The same result follows when alcohol, ether, and creasote are added, but, at the same time, the corpuscles are rendered so transparent as with difficulty to be found. Potassa, soda, and ammonia in dilute solutions dissolve them. Phosphate, carbonate and sulphate of soda preserve their shape and slightly increase their size. Acetic acid extracts the colouring matter from the corpuscles, renders them white and perfectly transparent, and by degrees dissolves the membrane completely. By the tincture of iodine they are not altered in form, but the outline becomes more distinct. In a strong solution of corrosive sublimate, the corpuscles undergo no change of shape, but they have a more sharply defined outline and can be preserved in this for a long time. Chloroform makes the globules bright yellow, causes the centre to appear and the outline to be indistinct.

The blood-corpuscles are distinguished by peculiarities of form and size in the different classes of animals, that may enable us to recognize to what animal a certain specimen of blood belongs. In this respect, the microscope alone can aid us, since chemistry furnishes no means of estimating the shape and diameter of the corpuscles. In the mammalia, the corpuscles are circular, and without nuclei, except in the elephant, camel, dromedary, and lama, where they are found elliptical, biconvex, and containing nuclei. In birds, the corpuscles are elongated and fusiform in shape, with well-defined outline and nucleus; and in the amphibia, they are oval, plainly convex, and have perceptible nuclei. The blood-corpuscles of embryonic life[1] are larger, and sometimes as large again, as in the animal after respiration has been established, and present themselves in the shape of soft, round, but often irregular bodies, of a pale red colour.

In order to obtain precise knowledge of the exact size of the corpuscles in different animals, resort has been made to micrometry by many celebrated observers, and the measurement of a number of specimens of each variety noted, to furnish data for accurate comparison. Latterly, for legal purposes, Carl Schmidt[2] has, with great assiduity and astonishing perseverance, accomplished the task, and below will be found the result of his experiments with forty different specimens of blood of the animals named, except with that of the rat, mouse, frog, and chicken, in which there were twenty.

The plan he at first adopted, was by drying blood in very thin layers upon glass plates. The size is given in millimetres, and the measurements of Gulliver are appended.

[1] R. Wagner's Physiology by Willis, Lond. 1841, p. 248.
[2] Op. cit., nebenstehende Tafel.

1. Blood-globules Dried in thin Layers, on Glass Slides.

	Man.	Dog.	Rabbit.	Rat.	Pig.	Mouse.	Ox.	Cat.	Horse.	Sheep.
Mean	0.0077	0.0070	0.0064	0.0064	0.0062	0.0061	0.0058	0.0056	0.0057	0.0045
Minimum . . .	0.0074	0.0066	0.0060	0.0060	0.0060	0.0058	0.0054	0.0053	0.0053	0.0040
Maximum . . .	0.0080	0.0074	0.0070	0.0068	0.0065	0.0065	0.0062	0.0060	0.0060	0.0048
AFTER GULLIVER.										
Mean	0.0074	0.0072	0.0070	0.0068	0.0060	0.0067	0.0060	0.0058	0.0054	0.0055
Minimum . . .	..	0.0056	0.0051	0.0048	0.0048	0.0048	0.0048	0.0054	0.0048	0.0032
Maximum . . .	..	0.0088	0.0095	0.0085	0.0071	0.0085	0.0071	0.0064	0.0072	0.0064

		Mean.	Minimum.	Maximum.
Chicken	Breadth	0.0076	0.0070	0.0081
	Length	0.0127	0.0120	0.0135
Frog	Breadth	0.0154	0.0142	0 0157
	Length	0.0211	0.0201	0.0220

To more closely approach standard measures for criminal cases, he experimented with blood dried in mass, and obtained the following:—

2. Blood-globules Dried in Mass, on Wood and other Substances.

	Man.	Pig.	Ox.	Horse.	Sheep.	Chicken.	
						Breadth.	*Length.*
Mean	0.0040	0.0034	0.0030	0.0028	0.0022	0.0040	0.0074
Minimum	0.0037	0.0030	0.0028	0.0026	0.0020	0.0038	0.0070
Maximum	0.0045	0.0037	0.0031	0.0031	0.0025	0.0042	0.0078

White Corpuscles.—With the red globules are found colourless corpuscles, which are far less numerous, but subject to great increase and diminution in their relative proportion to one another under various circumstances, and more numerous than would be supposed from the careless observation of a single specimen of healthy blood. The white corpuscles are very delicate and highly elastic cells, globular in form in all kinds of blood, when not subjected to pressure, with a clear, shining border, and slightly granular appearance in structure, containing nuclei varying in number. The term white, only serves as a distinction of the different bodies in red-blooded animals, for in insects the globules are all colourless, yet they are of different sizes. The white corpuscles have an average diameter of $\frac{1}{2570}$ of an inch; in the mammalia they are larger than the red, while in frogs they are smaller.

When submitted to the action of acetic acid, the outline is rendered transparent, the internal structure is coagulated, and the nuclei—which, having a slightly reddish-yellow tinge, become visible—seem to unite and

adhere together. By the addition of water, they enlarge in size without alteration in form, when the granules plainly appear, resembling pus-globules in a striking manner. When to blood under examination, water is cautiously added, the red corpuscles soon swell and burst, while the colourless ones are made perceptible, though before hid from the eye by the mass covering them.

The number of white corpuscles is small compared with the red, and, according to the careful enumerations made by Moleschott[1] with the blood of different individuals, was 1 : 314; so that a specimen of healthy blood shows but a few of them in the field of the microscope. This proportion is, however, liable to great alteration; and in the disease first noted by Virchow, and denominated leukæmia, and independently, and almost simultaneously discovered by J. H. Bennett, and by him called leucocythemia, the ratio is increased in some cases as high as 1 : 3.

Some years ago, when studying this disease, or, more properly speaking, condition, of the blood, I was surprised at the destruction and total disappearance of the colourless corpuscles, when the process of desiccation had taken place, and that it was impossible to reproduce them in their normal state. My opinion in this respect, I find confirmed by the eminent microscopist, Ch. Robin,[2] who says that, "in blood which begins to dry, the white corpuscles change their shape; their surface, which is more dense than the central portion, breaks and allows the contents to escape." It has been asserted by Prof. Wyman,[3] "that by examination of blood dried on glass, painted wood, &c., to which water is added, after a careful inspection, the observer will seldom be able to find any traces of blood-disks; but transparent, colourless spots will be seen scattered through the mass, which, with a high power (say 800 diameters), may be seen to have a globular form and to contain granules—usually three or four. These are the lymph corpuscles."

It is difficult to reconcile the remarks of Prof. Wyman with the rupture of the white corpuscles by drying, and I have tried again and again the experiment as described by him, and have not yet been able to discover in the dried blood any lymph corpuscles with their nuclei. Presumptuous as it may appear to doubt the accuracy of his observation, yet as the opinion, which I believe to be erroneous, has already found place, unchallenged, in a work[4] of high standing, it is admissible to call it in question. The red corpuscles of the blood, when treated with water, become decolorized, as has been shown by L. Mandl,[5] in one of the first contributions on the subject, who says: "It is known that the globules of the blood, placed in water, are deprived of colour, and leave only a white bed formed of fibrin. The

[1] Lehmann's Physiol. Chemistry, Am. ed., vol. i. p. 611.

[2] Briand, Med. Legale, p. 788.

[3] Note by Prof. Wyman in Report of Case of Jno. W. Webster. Boston, 1850, p. 90.

[4] Wharton and Stillé, Med. Jurisprudence, p. 562.

[5] Recher. Méd. légales sur le Sang, Thèse de Paris, 1842.

blood-globules having lost their colour entirely, there remains no indication whether or not they are from the blood of a mammifera, since, in the fluid drawn from an ovipara, oblong nuclei, in great number, are seen in the midst of the white layer of fibrin." The same result takes place, I think, when dried blood is inspected in the same way, which explains the discrepancy of opinion held on the subject.

Molecules.—These minute spherical bodies of the blood do not enter into forensic investigations, and, consequently, require no consideration here.

Derivation of Blood.—It frequently happens, in murder trials, when blood has been too plainly visible on the clothing of the prisoner, there is a defence made, explaining its presence by the killing of some animal, or by some natural cause. This can sometimes be contradicted, by finding mixed with the blood some substance which serves to denote its origin. The following illustrates this, in a trial at Norwich, England, a few years ago.[1] A female child, nine years old, was found lying on the ground, in a small plantation, quite dead, with a large and deep gash in the throat. Suspicion fell upon the mother of the murdered girl, who, upon being taken into custody, behaved with the utmost coolness, and admitted having taken her child to the plantation where the body was found, whence the child was lost by getting separated while in quest of flowers. Upon being searched, there was found in the woman's possession a large and sharp knife, which was at once subjected to minute and careful examination. Nothing, however, was found upon it, with the exception of a few pieces of hair adhering to the handle, so exceedingly small as scarcely to be visible. The examination being conducted in the presence of the prisoner, and the officer remarking: "Here is a bit of fur or hair on the handle of your knife," the woman immediately replied: "Yes, I dare say there is, and very likely some stains of blood, for, as I came home, I found a rabbit caught in a snare, and cut its throat with the knife." The knife was sent to London, and, with the particles of hair, subjected to a microscopic examination. No trace of blood could at first be detected upon the weapon, which appeared to have been washed; but, upon separating the horn handle from its iron lining, it was found that, between the two, a fluid had penetrated, which turned out to be blood—certainly not the blood of a rabbit, but bearing every resemblance to that of the human body. The hair was then submitted to examination. Without knowing anything of the facts of the case, the microscopist immediately declared the hair to be that of a *squirrel.* Now, round the neck of the child, at the time of the murder, there was a tippet or "victorine," over which the knife, by whomever held, must have glided; and this victorine was of *squirrel's fur!*

This strong circumstantial evidence of the guilt of the prisoner, was deemed by the jury sufficient for a conviction, and, whilst awaiting execution, the wretched woman fully confessed her crime.

[1] Chambers's Journal, part xxxv. Dec. 1856.

When blood is said to be derived from hemorrhages of various kinds, the truth or falsity of the statement can sometimes be found, by the discovery of a structure mixed therewith, peculiar to a certain part of the body, indicating its exact source. When from the nostril or lungs, by the admixture of the ciliary, and from the stomach and intestines, the columnar, epithelium. When from the bowels, by the presence of bile and feculent matter; and from the urinary bladder, by the salts contained in urine. Menstrual blood is detected by the pavement epithelium peculiar to the vagina.

Where fracture of the skull has been produced, the mixture of blood and brain matter known by the characteristic nerve-cells, found upon a bludgeon or other weapon, marks it as the destructive instrument. Should the polygonal cells of the liver be upon a knife supposed to have been used where a man has been stabbed in that organ, it would confirm suspicion.

If the charge of stupration be brought, and blood-stains containing spermatozoa produced, it would be strong evidence that the crime had been committed, though, at the same time, it is not difficult to imagine that sperm could be obtained and surreptitiously mixed with blood in order to sustain a false accusation.

In like manner many other things and circumstances could be named where the presence of other substances with blood would elucidate the investigation of a case; but as they are so numerous, I have contented myself with naming a few of them.

BLOOD-CRYSTALS.—The colouring matter which in the blood remains in solution, when under favourable circumstances, is changed, and becomes a crystallizable material, furnishing crystals of beautiful colour and form. The merit of their discovery is granted to O. Funke, who was very soon followed by F. Kunde, who succeeded in obtaining crystals from many different kinds of blood. It is most wonderful that this peculiar substance does not yield the same form of crystal in blood belonging to all classes of animals, which places within our reach a novel mode of recognizing in some cases, by the shape of crystallization obtained, to what animal a certain specimen of blood belongs, a fact not to be disregarded by the careful analyst who brings all knowledge to bear on the question.

This interesting subject has received much attention from Lehmann,[1] who says the crystals occur in three forms—namely, in prisms, tetrahedra, and hexagonal tablets. The prismatic forms, whose true system of crystallization has not been firmly established, notwithstanding the attention which has been devoted to the subject, are peculiar to human blood and to the blood of most mammals and fishes; the tetrahedra are met with in some of the rodents—as, for instance, in guinea-pigs, rats, and mice; while the hexagonal tablets have hitherto been found only in squirrels. A glance at this attractive subject at once shows the advantage which could be gained by resort to the crystallization of blood to determine its derivation.

[1] Op. cit., vol. i. p. 344.

The method of obtaining crystals is to put a drop of blood on a glass slide, add a small quantity of water, alcohol, or ether, and then allow evaporation to take place slightly, covering it with a glass slip, first interposing a hair between the glasses, to afford room for the crystallization, when carmine-red crystals of different sizes will appear in from half an hour to a few hours or several days, according to the kind of blood and the situation in which it has been placed. The crystals are more rapidly obtained by exposure to sunlight, and more easily from defibrinated blood. The shortest time in which I have noted their formation was in half an hour, from the blood of a rat. The glass slip is not necessary, but very convenient in producing them, especially for the first essay, since they form quite well when exposed to the air and sunlight; a little difficulty lies in knowing the proper quantity of water or other fluid to be added.

Besides the bright-red crystals peculiar to blood are seen consociated those of chloride of sodium and phosphate of soda, which are found when either fresh or dried blood is submitted to the process of crystallization.

The blood from which it is desired to make crystals does not require to be taken immediately from the vessels, but they are with less trouble procured when it has stood from twelve to twenty-four hours; which is a great advantage when demanded in legal cases. It is somewhat difficult to preserve the crystals for a length of time perfect in form, and the best plan is by Canada balsam. I have, however, been able to keep them for several months, well enough to recognize them, by having so much blood and water on the plate that, when the glass cover is applied, the fluid will form a rim around the specimen, by the drying of which the object is sealed from atmospheric influence. It would appear that crystals are sometimes preserved in a like manner when they form in clots, and there is no more certain mode of identifying blood than by finding them.

In an instance where blood-stains on cloth had remained for twenty-one months exposed to the atmosphere, Friedberg[1] found this special kind of crystals, which he calls hæmatin-crystals; and so very characteristic, that, in his opinion, one can with most perfect assurance prove the presence of blood when they are seen. Care must be taken not to mistake for them minute laminæ, found in clots upon hard substances, whose accidental shape, when seen by a low power, sometimes simulates crystals.

From blood which had been kept in dried clots for several months I have tried many times to obtain crystals, and lately have been successful in making some from ox-blood, preserved as above stated; which, however, were not very perfect specimens of crystallization. At the same time this is somewhat mortifying to acknowledge, yet it gives encouragement and desire for further experiment. The plan adopted was to dissolve the clot in water, and allow it to stand until the corpuscles had all burst, then

[1] Histologie des Blutes, Berlin, 1852, s. 71.

filtering the solution, and, after the addition of ether or chloroform, proceed as with fresh blood.

DRIED BLOOD.—The vital fluid is so constituted as to possess an inherent power of self-preservation, whereby the anatomical constituent, the red corpuscles, provided no putrefaction has taken place, is retained by desiccation in such a state that, upon the addition of certain fluids, the normal condition of the globules can be almost perfectly restored. Blood has been kept by many observers for different periods of time, and some as long as nine years, with no perceptible alteration. Before me are several specimens put away for five years, not having changed at all, without any further care than to keep them dry.

The time required to soften the blood to such a degree as to reproduce the corpuscles varies from a few minutes to several hours, and is proportioned to the age of the clot and the tenuity or solubility of the reagent employed. Owing to the rapid endosmotic action which is found to take place in the corpuscles when a fluid whose density is lower than that of the serum is added to blood, by which they swell and soon burst, destroying their identity, some trouble has been experienced in the selection of menstrua which would most suitably answer the two purposes—of moistening the mass, and reinstating the corpuscles. For this purpose a great variety of fluids have been suggested and used, some of which have been esteemed by one observer, and disliked by another.

Water.—In dried blood, where no decomposition has taken place, it has parted with its water by evaporation; and it will readily recur to any one that by the cautious addition of water, in quantity proportioned to the size of the stain, a close imitation of the normal serum is made. When water is used, many of the corpuscles are sacrificed before its density has been raised, by the salts and other elements of the blood, to that of serum; and when this has been done, the globules are seen floating about, with well-defined outline. This reagent is more particularly useful when the stain is very old, for in recent specimens, when submitted to its action, the endosmosis is so violent as to destroy the corpuscles. There is perhaps no fluid in use for softening masses of blood which requires more care and experience than water, and it is well to take the precaution to scrape a small portion of a clot on to a plate, and, after placing the same in the field of the instrument, add the water, when the reaction can be observed.

Serum.—The objection to the use of serum is the difficulty of obtaining it entirely free from corpuscles; and to obviate this, the contents of hydroceles have been employed. That serum might be employed for this purpose, F. W. Böcker[1] has, very ingeniously, proposed to filter human or other mammalian blood, and to employ the serum thus obtained for the examination of frog's blood, or that of the ovipara; and, in like manner, to take the serum of frog's blood to mix with the blood having round corpuscles.

[1] Memoranda der gerichtlichen Medizin, Iserlohn, 1854, s. 281.

Albumen.—This substance, as presented in the white of an egg, has been taken for the microscopic examination of blood, and has been found free from many objections belonging to other substances.

Glycerine.—This menstruum has been highly commended by Dr. Alf. S. Taylor,[1] on account of its close approach in specific gravity to that of serum, and the slowness with which it evaporates. The ease of obtaining glycerine of purity is another advantage which it possesses. With albumen and glycerine a slight refraction of light takes place, which might disturb the vision of one unaccustomed to microscopy.

Besides the above, oil, solutions of sugar and sulphate of soda, liquor amnii, and an aqueous solution of iodine, to which sugar has been added, have been used by different individuals for bringing the corpuscles in relief, and allowing them to be quickly recognized, but none are so free from objection as albumen and glycerine.

The blood-corpuscles can be preserved so well for several years, that they retain their characteristic shape, and indicate accurately the race of animal to which they belong, by simply spreading them on a glass slide, in a very thin layer, when, by blowing upon them, or waving the slide back and forth in the air, to make vaporization take place rapidly, these bodies are fixed, and may be permanently prepared by putting a frame of gold size around them, and applying a cover. The contraction of the contents takes place equally on all sides, whereby their form is preserved.

Carl Schmidt[2] is of opinion that "the drying of blood globules of different animals, isolated or in mass, adheres to the same rule of evaporation as the pollen of a flower, and the coefficient of desiccation in all of them bears a constant relation to the diminution of their volume. The micrometric definition proves this presumption, and gives us the solution of the most difficult problem on this point, the diagnosis of certain kinds of animal blood, in a dried condition, from one another and from that of man." This plausible and philosophical opinion seems to forestall any doubts or difficulties which might be raised from inspecting dried blood, and unfortunately it is not fully borne out in all practical cases, forcing one, however enthusiastic he may be to claim for the microscope the highest merited confidence, to acknowledge there are circumstances when it fails to discriminate positively the dried blood of one animal from that of another.

Some alteration in the form of the corpuscles is produced by drying upon substances of a porous nature, whereby their integrity is injured. In the Marylebone Police Court, in the case of Wm. Styles, Dr. Hassall[3] made the following remarks directly bearing on this question : While the determination, by means of the microscope, of the nature of blood-stains, even when very recent, formed on cloth, linen, and other soft and porous textures, is usually a matter of considerable difficulty, and is often impossible,

[1] Medical Jurisprudence, 4th Am. ed., p. 239.

[2] Op. cit., s. 5.

[3] Lancet for 1852, vol. i. p. 321.

the determination of such stains, however old, as are placed on glass, porcelain, wood, and other hard or smooth surfaces, is in general unattended with difficulty, and extremely satisfactory. This difference is to be explained thus: in the one case the fibrin, albumen, and serum of the blood are in part absorbed and pass into the cavities of the hairs or fibres of the wool or linen; the blood-corpuscles are thus deprived of their preservative fluids and shrink up—become misshapen or disintegrated; while in the other case the fibrin and albumen harden around the blood-disks in drying, and thus preserve them slightly altered in form only.

The difficulty is further well illustrated in the following case: A man walking alone on the street, received a blow in the face which stunned him and caused the blood to flow; on recovering, he found himself robbed of his money. A labourer was arrested on suspicion, on whose pantaloons was found a large blood-stain, which he said was produced in helping to kill a cow. The stained garment was submitted to microscopical examination, in which Profs. Du Bois and Reymond participated. Answer of committee: Blood-corpuscles of ox blood can only be discriminated from human blood-corpuscles by their smaller size. On examining fresh blood of oxen and fresh blood of man, the human corpuscles are found to be larger, and could be easily recognized even on admixture with the smaller ones of ox blood. Some threads saturated with blood were cautiously taken out of the blood-stained pantaloons and macerated in pure bone oil; on bringing the preparation under the microscope, the form of the blood-corpuscles was found too indistinct to warrant a conclusion as to their nature. The blood-stain was at least three, if not six, weeks old, at which time the corpuscles have acquired their shrivelled appearance, which always gives uncertain results. In order to examine the opposite opinion of Schmidt, the following experiment was made: fresh human and fresh ox blood was put upon the pantaloons, and allowed to dry for a week. The blood-stains were then softened in bone oil and brought separately and mixed up under the microscope. It seemed as if the dried human blood had more resemblance to the blood found on the garment than the dried blood of the ox, but the form and diameter of both kinds of blood-corpuscles were so much changed by shrivelling, that conscientiously a discrimination could not be made.[1]

Böcker[2] says that "sometimes by the drying of blood the corpuscles are destroyed, when only shapeless little masses or roundish granules remain, whose true nature it is impossible to discover microscopically. I have, among others, a specimen of dried pigeon blood, three years old, in which I and several practised microscopists cannot discover a single elliptical or oval blood globule, whilst very many round granules of the size of human corpuscles are seen. Moreover, I have found the bird's dried blood, when moistened with a solution of sugar, or in fresh filtered serum of frog's

[1] Op. cit., von J. L. Casper, s. 157. [2] Op. cit., s. 282.

blood, contains a great many round, indeed, chiefly round, corpuscles very similar to those of human blood, and it is often necessary to search a long while before an elliptical one is found. In human and mammalian dried blood, I have never found elliptical blood corpuscles."

After numerous and carefully conducted experiments, Friedberg[1] mournfully acknowledges "that the solution of the most difficult problem—the diagnosis of the blood of man and certain mammalia, in a dried state, as Schmidt has announced as positive in all cases—is still a *pium desiderium*, and it is not possible by the present known means of examination to distinguish them, as the results of the many systematic experiments I have made authorize me in saying."

Ritter,[2] in his well digested and carefully arranged prize essay, takes occasion to remark that "in his researches he has not found the facts as interpreted by Schmidt, and agrees with the conclusions of Friedberg."

From the experiments which I have made during a period of several years with blood belonging to different animals, when dried for a length of time and moistened again, I am forced to admit that great difficulty arises in attempting to fix its origin by the comparative size of the corpuscles; and again, that the blood of ovipara, when kept for several weeks, does not present the peculiar elliptical corpuscles found in fresh blood, in a form sufficiently perfect to justify me in declaring positively whence it proceeds.

Pittsburg, Pa., Oct. 20, 1858.

Art. VIII.—*On the Occurrence of a Blowing Sound in the Pulmonary Artery associated with Affections of the Lung; on the Sounds of the Artery in Health, and on the Effect on them and on the Heart of the Act of Inspiration.* By J. DaCosta, M. D., Lecturer on the Practice of Medicine at the Philadelphia Medical Association.

In examinations of the chest, I have met with a peculiar physical sign—a blowing sound in the second intercostal space on the left side—the occurrence of which has only been imperfectly alluded to, and the importance of which is not settled. The following notes of cases will explain its significance.

Case I. Wm. Moore, age 35, a clerk by profession, placed himself under my charge September 8, 1857, for phthisis. He belongs to a family in which consumption has made sad ravages; his father and grandfather, brothers and sisters, having died of the disease. He had had, when first seen, seve-

[1] Op. cit., s. 57.

[2] Ueber die Ermittelung von Blut, Saamen, und Exrementenflecken in Kriminalfællen. Würzburg, 1854, s. 139.

ral hemorrhages; had lost flesh; and was troubled with a cough, followed by a yellowish expectoration. His pulse was 112, and feeble. The upper portion of the right lung was found dull on percussion, with crackling and prolongation of the expiratory murmur. On the left side, a slight dulness existed anteriorly at the upper portion, and the respiration was harsher than normal. It is not necessary here further to detail the case; it need only be added that he has remained under observation for upwards of a year, and that the most marked improvement, both in the symptoms and physical signs, has followed the administration of cod-liver oil and the iodide of iron. No advance of the disease has taken place in the left lung. During the period he has been under my care, I have repeatedly noticed that *a blowing sound*—at times low, at times of a higher and more whistling pitch—existed in the interspace between the second and third ribs on the left side. It was synchronous with the heart's impulse, and followed by a distinct second sound. In some examinations, in which the heart was beating slowly and the patient breathing quietly, it was not perceived, especially not at the last (in November, 1858). I do not, however, believe that it has disappeared entirely. The other sounds of the heart were perfectly normal, as was its impulse and the extent of the percussion dulness. No arterial or venous murmurs were noted.

CASE II. Charles Boyd, age 18, a shoemaker, came under my care May 24, 1858. He had had a cough for three months, attended with emaciation and night-sweats. Slight dulness existed at the upper portion of the right lung, also crackling and prolonged expiration; a friction sound was noted at the upper portion of the left lung, anteriorly, near the second rib, and dry râles were heard there, as, indeed, over the entire extent of that lung and of part of the right. The case was diagnosed as one of tuberculosis, probably of both lungs, certainly of the right, accompanied by an attack of bronchitis. The bronchitis soon yielded to remedies employed for its removal. On the 8th of June, no râles were perceived. There was then crackling in the right lung, rude inspiration, with marked prolonged expiration; whilst in the left lung the respiration was harsh, the friction sound being still heard; also—what previously (although it may have been present) had not attracted attention—*a rather short blowing sound*, at times of high whistling pitch, then again of a lower note, in the second intercostal space, and synchronous with the heart's impulse. No blowing sound existed over the heart; nothing abnormal was noted about the impulse or size of the organ. The further history of the case, until the patient was lost sight of (in September), is soon told. The crackling at the upper portion of the right lung became larger and moister, and more decided signs of infiltration in the left lung took place. When last seen, the blowing sound in the pulmonary artery still existed, and, under excitement, was almost distinctly sibilant; it was heard best when the patient held his breath in expiration.

Both these were undoubtedly cases of phthisis. The blowing sound in neither, from their not having been under observation when first attacked, can be stated to have occurred with the earliest indications of tubercular infiltration. In the two cases which follow—and for the opportunity of recording which I am indebted to my friend, Dr. Francis W. Lewis—the blowing sound was noticed when the signs of disease of the lung were far

from being satisfactory, or even unequivocally present, but the progress of the cases showed it to be associated with tubercular disease. In Cases V. and VI. which are still under observation, the physical signs and symptoms indicate phthisis in its earlier stages, and the blowing sound is, in both, quite distinct.

Case III. In the spring of 1854, I saw several times, with Dr. Lewis, Hugh Burns, æt. 28, a coachman, who had the physical sign in question very well marked. He had consulted Dr. Lewis in March, 1853, for cough and dyspnœa. He was then a man of florid appearance, not in the slightest degree emaciated, but very much addicted to the use of tobacco. He complained of having suffered occasionally from palpitation of the heart, had a slight cough, and had had a hemorrhage from the lung about a fortnight before. The physical signs at that time were negative as to a deposit. No abnormal modification of the vesicular murmur was found in the right lung; if any existed in the left, it was obscured by a very distinct, rather harsh blowing sound, heard to the left of the sternum. The patient was placed upon the use of cod-liver oil. When he was again seen, six months afterwards (in September), he had had several hemorrhages. The left lung was evidently the seat of a tuberculous deposit. I examined him about this time; he was somewhat emaciated, coughed, and presented dulness at the upper portion of the left lung with harsh inspiration. The *blowing sound* was very distinct, especially after exercise. When the heart was acting quietly, it was softer, and not always easily perceptible. Its situation was at the left of the sternum in the second intercostal space; it extended from thence slightly upwards, and occurred synchronously with the impulse of the heart. There was no enlargement of the organ, no blowing sound over the heart or in the arteries of the neck. The further history of the case showed that the tubercle gradually infiltrated both lungs, cavities formed at the apices, dysenteric complications occurred, and the patient died exhausted on September 19, 1854. The blowing sound had not disappeared when his chest was last examined, after cavities had fairly formed, and when he was sinking. Subsequent to this, he grew so weak, that an accurate examination of the chest could not well be made.

Case IV. Sarah Gray, æt. 36, married, of scrofulous aspect, had been salivated severely for rheumatic iritis, after which she fell at once into feeble health. Her symptoms, when seen on March 3, 1853, may be briefly stated as follows. She was emaciated, coughed, the circulation was feeble and irregular, and her menses had stopped. A physical examination revealed slight feebleness of respiration at the upper anterior portion of the left lung, and a distinct *systolic blowing sound* at the base of the heart, not propagated into the aorta, and not heard in the vessels of the neck. It could be localized near or over the sternum, opposite the third rib. The heart's action was frequent, and the impulse strong. In July, unmistakable signs of tubercular deposit appeared in the left lung. She grew weaker and weaker, and died in January, 1854. The blowing sound was distinctly heard a few weeks before her death. An autopsy showed extensive disease of the left lung. It was, throughout, the seat of a tubercular deposit, and contained cavities. The right lung presented only a few scattered tubercles. None of the abdominal viscera were affected. The heart was not enlarged, the valves were healthy. No abnormal state of the pericardium, nor of the vessels proceeding from it, was noted. The exact position of the pul-

monary artery to the deposit was, owing to the circumstances of the examination, not specially studied.

Case V. Sarah C——, æt. 47, consulted me October 30, 1858, with reference to a cough which she had had for nine months. She had lost flesh and had spat blood twice within the last week, not much in quantity, but red and unmixed with expectoration. Occasionally she has had night-sweats. The cough was very troublesome, especially when she laid down at night. The appearance of the woman was not that of anæmia. She did not complain of shortness of breath; her respiration was not hurried; there was no pain at any portion of the chest. On percussion, I found general clearness throughout both lungs; but at the upper portion of the left lung, anteriorly, especially between the second and third ribs, slight dulness was noticed, and an increased resistance to the finger. On close inspection, a sinking in at the upper portion of the lung was observed. In the right lung the vesicular murmur was normal; in the left lung the respiration throughout the whole upper portion of the chest, anteriorly and posteriorly, was rude; rudest, however, at the apex, anteriorly. The heart's sounds were normal; the impulse and the extent of percussion dulness not increased. Between the second and third ribs, an inch to an inch and a half from the edge of the sternum, on the left side, was heard with each beat of the heart a rather abrupt but *distinct soft blowing sound;* after it, an accentuated second sound. The murmur was not perceived at all over the heart, nor even at the left edge of the sternum. It did not extend up higher than the second rib, and was not heard with each impulse. No blowing sound existed over the aorta. No venous or arterial murmurs were discerned in the neck. The sound seemed only to be occasionally produced, and was best heard at the first few minutes of the examination; after that it was not constant. The patient complained of some pain on pressure between the first and second, and second and third ribs. It was not noticed that any acts of respiration caused marked changes. The blowing sound seemed to be more particularly distinguishable after a series of full inspirations, and to be in time simultaneous with the expiration.

Case VI. James Dogherty, age 18, is noted in November of this year as having been ailing for nine or ten months with pains in the chest and a cough. He is very liable to colds, and then the cough increases. At no time since February has he been free from cough. He has lost flesh, and has on several occasions expectorated blood, upwards of a tablespoonful in quantity; yet he neither looks, nor expresses himself as feeling very feeble.

On examining his lungs, the left lung yielded at the upper portion a somewhat duller sound than the right; the respiration at the whole of the upper portion of the left lung, anteriorly, was rude.

The percussion dulness over the heart was more extended than usual; the impulse also was stronger. The sounds at the apex of the heart, and at the aortic cartilage, were normal; at the pulmonary valves (edge of the sternum, left side) the second sound was much accentuated; between the second and third ribs, about an inch from the edge of the sternum, was discerned, with the impulse of the heart, a *short, rather musical blowing sound,* over a space which could be covered with the stethoscope. The sound was best heard when the patient was standing erect, and at the commencement of the examination. It seemed also most distinct after a full inspiration. When he was quietly seated, the sound was not perceived. No venous or arterial murmurs were observed.

That these patients all suffered from phthisis need hardly again be stated. In all of them the blowing sound existed, unconnected with blowing sounds in the heart, or in the arteries of the neck. In all, the blowing sound was synchronous with the impulse, and in five, situated in the second intercostal space. In reviewing these six cases, we are struck with the fact that the murmur was evidently among the earlier symptoms in four; and further, that although it was not always equal in intensity, as far as observed it did not disappear entirely. But on this point I do not wish to speak decidedly, since some of the cases have not been watched long enough to permit of a conclusion. In the last two observations some room for doubt as to the correctness of the diagnosis may exist, but not much; not more than is present in any case of incipient pulmonary disease in which there has been hemorrhage, and in which slight dulness, with a modification of the vesicular murmur, is detected at the apex of the lung. Were it not from having observed the next two cases, I might have arrived at the conclusion that a blowing sound in the second intercostal space absolutely belongs to tubercular infiltration of the lung; but they disapprove this idea, and show how the sound may be associated with other than tubercular affection.

Case VII. Ter. McGinnis, age 33, a labourer of healthy appearance, consulted me in August, 1858, for fugitive pains in the chest, especially of the left side. His case is noted as follows: In December, 1857, having previously been in excellent health, he was seized, after exposure, with a fever and severe pain in the left side, which confined him to his bed. His breathing at that time was short, but he recovered from the attack without any further annoyance than an occasional cough and some pain in the chest. He has not lost any flesh; never spat blood; and has no cough; indeed, with exception of the pains which trouble him occasionally, he is in good health. Neither his respiration nor his circulation is increased in frequency, nor has he difficulty of breathing. The upper part of the left lung, anteriorly, is duller on percussion than the upper part of the right. The inspiratory murmur throughout the left lung, but especially at its upper portion, is harsh; the expiration there prolonged. The voice is distinctly heard over the left lung. Vesicular murmur is normal throughout the right lung. Size of the heart normal; sounds normal; at the left side of the sternum, between second and third ribs, the second sound of the pulmonary artery is remarkably accentuated and sharp sounding, much more so than the aortic on the opposite side; in a line more outwards a short *distinct blowing sound* is heard, corresponding to the impulse of the heart, over a spot about an inch in length; it is marked when the patient is in the erect position, and the heart is beating strongly.—He was directed to rub his chest with a stimulating liniment, which relieved the pain.

On September 28, the physical signs were the same; his general health was good, and he was able to do his work. The blowing sound was heard well when the heart was acting rather strongly. I have seen him once since; he was attending to his daily duties, and in good health. No murmurs in the arteries or veins of the neck were at any time perceived.

Case VIII. A—— C——, age 22, consulted me in September, 1858, for a pain in the chest and a cough which he had contracted at a watering-place.

The physical signs indicated slight bronchitis of the right lung. The pain was mainly post-sternal. The cough was trifling, and, under the use of expectorants and a turpentine liniment, both it and the pain had in the course of three weeks entirely disappeared. The previous health of the gentleman had been excellent. He did not recollect ever to have had an acute disease of the lung. No hereditary tendency to tuberculosis existed in the family. He had, when last examined (November 17), not a vestige of cough remaining, and was in very good health, excepting a slight enlargement of the tonsils, and sore-throat, which has at times troubled him for years past. When first ausculting his chest, I was struck *with a distinct blowing sound* in the second intercostal space of the left side. It was soft, low-pitched; but became, when he was breathing frequently and his heart was excited, of a higher pitch, and more like a sibilant râle. The sound was noted at his first examination; at the last, two months afterwards, it was still present. Its more particular character may be gleaned from a note taken during one of the examinations after the signs of bronchitis, and the cough had entirely left.

On percussion, the lungs are clear throughout. There is relative dulness, with a higher pitch at the upper portion of the left lung. This is especially marked between the second and third ribs, where there is also more resistance, and a slight sinking in perceptible. On auscultation, the vesicular murmur is normal throughout the right and the greater portion of the left lung. At the upper portion of the left lung it is harsher and stronger than normal, although the expiration is not prolonged; between the second and third ribs it is feebler than higher up; and the inspiration is distinctly jerking, or rather divided. The percussion dulness of the heart is not marked until the fourth rib is reached. The impulse is natural in extent, force, and situation. The heart-sounds, at the apex, at the ensiform cartilage, and close to the sternum on the right side in the second intercostal space, are natural; on the left side, about half an inch from the sternum, in the same intercostal space, is heard a blowing sound, corresponding to the impulse, and followed by a very accentuated and marked second sound, which is more accentuated and marked than the second aortic sound. When the heart, after exertion, is beating rather strongly (the pulse counted being 78), and the breathing hurried (28 times a minute), the blowing sound becomes like a sibilant râle. It is best heard when the expiration is held, or immediately after the inspiration. When a long breath is drawn and held, the murmur can hardly or not at all be discerned, and the second sound is very indistinct, but becomes afterwards, during expiration, very sharp and distinct. The sound has the sibilant character only very occasionally if the heart is beating quietly and the breathing not hurried. Under these circumstances, also, it cannot be readily perceived with every impulse of the heart. Over the spot indicated no other sound occurs, corresponding in time to the first sound of the heart, and no pain or pulsation exists.

No blowing sound could be discerned in the arteries or veins of the neck.

The *physical sign* in question may, then, be stated as being a murmur attending the impulse of the heart, almost always soft and low-pitched, although occasionally harsher, of higher pitch, and simulating a sibilant râle. Its situation is in the second intercostal space on the left side, not an inch from the edge of the sternum. It may be audible higher up; or, again (as in Case IV.), opposite the third rib, on the sternum.

The space it occupies is usually very limited, and can be accurately circumscribed with the stethoscope. It is not heard during a full inspiration, but very distinctly after inspiration, or with expiration. It takes the place of the first sound at the spot it is heard, but is followed by a distinct second sound. When the patient is breathing quickly, and the heart's action excited, it is best distinguished. It is not always of equal distinctness or of equal pitch, but it is not transitory, as it may be observed extending over a long space of time. The sounds of the heart are not influenced by it. They are heard with the usual clearness at the apex, immediately above the ensiform cartilage; at the third rib, or second intercostal space, on the right side; at midsternum; and even at the third costal cartilage and edge of sternum, on the left side. At this latter situation the second sound is distinctly heard; the occurrence of the first is more difficult to perceive and more doubtful.

Before inquiring into the cause of this sound it will be necessary briefly to recall the anatomy of the parts, as well as some clinical points with reference to examinations of the heart. If a long needle be stuck into the chest, in the second intercostal space on the left side, immediately below the cartilage of the second rib, it strikes the pulmonary artery near its bifurcation. The valves of the pulmonary artery are seated at the junction of the cartilage of the third rib with the sternum on the left side. Near them, somewhat lower, lie the aortic valves; and lower still, and inwards, about on a level with the fourth rib, at midsternum, are the tricuspid and mitral valves. The fact that the valves are so close together has led to the seeking for points at which the sounds may be isolated, and these are, following the course of the vessels, for the aorta, the second intercostal space close to the right edge of the sternum, or the second costal cartilage; for the pulmonary artery, from its ascending on the left side, the second costal cartilage, or the second interspace near the left edge of the sternum. These points for the aorta and pulmonary artery are spoken of, for convenience sake, as the "aortic cartilage" or interspace, or the "pulmonary cartilage" or interspace. Now, the pulmonary artery is inclosed in its whole extent by pericardium, within which membrane it usually divides. The left pulmonary artery passes then through the pericardium, a layer of which is prolonged upon the vessel; surrounded by this and the reflection of the pleura, it is met with at the root of the lung. It, however, soon enters the pulmonary texture and subdivides, at a point immediately over the fourth dorsal vertebra, and, as may be readily seen by removing the sternum, opposite the junction of the lower border of the second costal cartilage with the corresponding rib.

As the blowing sound is heard so exactly in the position which we have just seen to indicate the course of the pulmonary artery, and especially at a point just before or even at the bifurcation of its left branch, we shall regard its seat as certain, and may next proceed to discuss its *cause.*

All blowing sounds in the heart or in arteries are due either to a peculiar condition of the blood, or to the presence of local obstructions; or, again, they may be transmitted. This latter supposition cannot be entertained, as there was no murmur anywhere to be transmitted, the heart in all the eight cases yielding perfectly normal sounds.

Was the murmur inorganic, the blowing sound that of anæmia? No; for, independently of the fact that in Cases VII. and VIII., as well as in Case III. when first seen, not the slightest signs of anæmia existed, it is in nearly all especially noted that there were no blowing sounds in the arteries of the neck, or none of those venous murmurs which usually occur in watery states of the blood. I am aware that a blowing sound in anæmic women may be more marked at the base of the heart over the pulmonary artery than over the aorta; but is it persistent, merely distinguishable there and unaccompanied by a venous hum? Certainly this must be of very rare occurrence. In Case IV. the blowing might be thought to have been inorganic, from the history of the case, and from its being perceived near the sternum; but even here its purely local character, and the absence of murmurs elsewhere is against this supposition, and still more so is it in the other cases.

If the blowing sound was not inorganic, and not transmitted, it must have been owing to local lesions of the pulmonary valves, or of the artery. Now, morbid states of the pulmonary valves occur so rarely, as hardly to be considered in a point of diagnosis. Moreover, the site of the greatest intensity of the blowing sounds in such cases would be at the sternal edge of the third left cartilage, which it was not in the cases reported, excepting, perhaps, in one; in which, however, an autopsy demonstrated the absence of valvular lesion. Nor were there in any of the patients signs of cardiac disturbance; hence, diseases of the pulmonary valves certainly could not have been the cause of the murmur.

The last consideration, is that the murmur was caused by local changes or obstructions in the pulmonary artery. This might be owing to deposits in the coats of the vessels, or to a healthy state of the artery itself, the murmur being produced by pressure on the artery. That it was the latter, and not the former, I believe, because,

1st. Deposits in the coats of the pulmonary artery are uncommonly rare.

2d. The cases having occurred mostly in young persons, the age of the patient excludes deposits.

3d. The murmur was usually soft, but became sometimes of a more whistling character.

On this point I lay less stress, but the other two are sufficient. It might also be added, that the autopsy in Case IV., whilst it is not satisfactory as to the exact condition producing this blowing sound, is so, as far as it showed no abnormal state of the coats of the vessels.

These blowing sounds are, then, capable of explanation by local pressure, or rather a want of yielding of the textures surrounding the left pulmonary artery, or one of its branches, during the diastole or expansion of the vessel, and when the blood is propelled through it. Masses in the pericardium have been noticed to produce a murmur in the pulmonary artery. Why should not, then, in some persons, in whom the position of the artery is such as to be easily compressed by the lung, infiltration of the surrounding textures—especially if pleuritic adhesions exist at the upper lobe, which would keep the lungs from fully expanding—prevent the vessel from fully and equally distending, and thus give rise to a murmur? This might occur the more readily, because, as will be presently shown, the first sound at the pulmonary artery is usually dull and indistinct, and would easily permit of a murmur being perceived or taking its place. Whether this explanation be adopted or not, the fact that in all the eight cases local signs of partial consolidation were met with, seems to bring these local signs in connection with the peculiar murmur. I do not claim to have discovered a new physical sign. My only wish has been to endeavour to fix the value of a sign which experience has taught me occurs in a certain number of cases. Dr. Latham, long ago, pointed out the occurrence of a blowing sound at the upper part of the left lung, in persons who were undeniably tubercular, or suspected of being so. From some reason or other, little heed has been taken of his observation; but that it is entitled to consideration the cases reported prove, although they also prove that non-tubercular deposits may occasion this murmur. On the whole, however, they show that where slight changes are observed at the apex of the left lung, with symptoms which lead to a suspicion of tuberculosis, where there is an absence of any symptoms or signs which point to a previous attack of pleurisy or pneumonia, and which would account for the physical signs at the apex, the presence of a *localized* systolic blowing sound on the left side, unaccompanied by venous murmur, ought to have a decided diagnostic significance: in what proportion of cases it occurs remains, however, yet to be fixed.

In the course of this clinical inquiry, two points have arisen on which some light may be thrown by a comparison with phenomena in health. The blowing sound took the place of the first sound over the pulmonary artery. What are the natural sounds heard there, and how produced? Secondly, full inspiration prevented the sound from being heard. What is the effect of full inspiration on the sounds of the artery?

For the purposes of an accurate standard in examinations of cardiac affections, I have studied and compared with each other, with care, the heart sounds in the different portions of the organ, and also the effect of the respiration on them. I shall insert here as much as is relevant to the subject of the consideration of sounds in the pulmonary artery, merely adding that the usual clinical positions have been selected—the second intercostal space near the sternum, on the right side, for the aorta; on the left, for the

pulmonary artery; whilst the sounds at the apex have been studied a little above the left apex, and above the ensiform cartilage. Where the term apex merely is used, it is meant to apply to the apex beat on the left side of the heart, between the fifth and sixth ribs.

Comparative Observations on the Sounds of the Pulmonary Artery.

Observations.	Sex.	Age.	Pulse.	Respirations per minute.	Character of sounds of pulmonary artery.
1	M.	12	72	19	First sound very dull, indistinct, of much lower pitch than second, which is sharp and accentuated and very marked at left edge of sternum.
2	M.	22	70	24	Sounds in all respects the same; no difference between aortic and pulmonary; first sound at the left apex is much longer.
3[1]	F.	24	96	25	Both sounds distinctly heard; first sound of low pitch; second sound unusually strong and accentuated. As compared with the aortic sounds, the first pulmonary was of lower pitch, but the second seemed in all respects equal. The first sound at the apex was very much longer and more weighty.
4	M.	13	80	23	Both sounds heard; first, dull, of low pitch; as compared with aortic, second aortic is much more distinct; no difference between the first.
5	F.	15	75	20	First sound very dull, of low pitch, and very indistinct; second sound sharp.
6	M.	22	80	19	First more like an indistinct vibration, of low pitch, no defined sound heard; second, sharp, accentuated, best heard at left edge of sternum; first aortic, also, is very dull; second more marked than second pulmonary; first sound at apex long, heavy and distinct.
7	M.	30	72	20	Both sounds distinct; first much duller than second, which is most accentuated at edge of sternum; second aortic more accentuated than second pulmonary; no difference between first.
8	M.	22	50	15	First sound very dull, almost imperceptible; second marked, most so at edge of sternum. As compared with aortic sounds, first duller, of lower pitch, but more prolonged; second less accentuated. At apex of heart, first sound is dull, heavy, and long.
9	M.	26	72	20	First sound indistinct, dull, of lower pitch, less sharp than first aortic. Second not nearly as accentuated as second aortic. First, at apex, dull, but heavy and prolonged.
10	M.	23	72	20	The same results.
11	M.	25	74	24	Both sounds distinct; first is rather duller, longer, and of lower pitch than first aortic, but as distinct or distincter; second aortic is sharper and more accentuated. First, at apex, dull, but very distinct, heavy, and longer than first pulmonic: first, at ensiform, rather sharper.
12	M.	26	72	19	First sound dull, but of low pitch; duller than first aortic. Second also less sharp and accentuated than second aortic. At apex, first is much heavier and prolonged.
13	M.	22	84	18	The same; except no appreciable difference between first aortic and first pulmonary. First, at ensiform, sharper than first pulmonary.
14	M.	21	96	19	The same as in Observation 12. First sound of pulmonary artery is not as sharp, and more difficult to distinguish, than first aortic.
15	M.	25	96	21	The same. First of pulmonary artery very dull and of low pitch, more like an indistinct vibration. First, over ensiform, sharper.
16	M.	24	72	18	First sound dull, low pitched, and so indistinct as to be almost imperceptible. Second accentuated and distinct, but not as much so as second aortic. First aortic, also, is sharper and better marked; sounds shorter. At apex, first sound is dull, prolonged, but not very heavy.
17	F.	44	82	25	First sound at pulmonary dull and indistinct; second marked, less accentuated than second aortic. No difference between first pulmonary and first aortic. First, at apex, is dull, much more prolonged, and heavy; and very distinct.
18	F.	30	72	20	First sound at pulmonary dull, but marked; no difference between it and aortic first; seems a little more distinct. Second pulmonary more marked than second aortic. First, at apex, dull, but very much more distinct and prolonged than first pulmonary.
19	M.	58	70	19	First sound at pulmonary very dull, low pitched, indistinct, hardly perceptible; second marked, but far less so than second aortic. First aortic sharper, higher pitched than first pulmonary. At apex, first is much more prolonged, quite distinct, and heavy.
20	M.	22	76	24	First pulmonary indistinct and dull, more so than first aortic. Second pulmonary less accentuated than second aortic. First, at apex, dull, but distinctly heard; much longer than first pulmonary.

[1] This person was troubled with occasional shortness of breath, but the size of the heart was not increased, the beat was frequent, the impulse distinct.

It will be seen from this table, that the first sound of the pulmonary artery is usually dull, of low pitch, and, in certain cases, more like a vibration than a sound, or so indistinct as hardly to be perceptible. Compared with the first sound of the aorta, it is equal in a certain number of instances (it was in six out of eighteen in the table); but when it differs (in eleven out of eighteen), it is noticed to be less distinct, less sharp, although, it is in some persons a rather longer sound. Compared with the first sound at the apex of the heart, it lacks the weighty, prolonged, marked character of this sound. If the stethoscope be carried up from the apex of the heart to the second interspace, the difference is observed to be very marked; the change of sound occurs almost abruptly. It seemed to me, also, as if a decided change in pitch took place; but I do not wish to speak too positively on this point, as it is a very difficult matter to distinguish changes of pitch in sounds, both of which are dull, and one very indistinct. Compared with the first sound over the right ventricle (above the ensiform cartilage), the first pulmonic is of a much duller character, and decidedly less sharp, and not of as high a pitch. In this respect, the difference—not merely in the above observations, but in others I have made—was noticed as much more decided than between it and the first sound over the left ventricle; the one on the right side being, although less strong and shorter, usually clearer, and of higher pitch even than this.

These analyses, showing that the sounds of the heart, listened to in different positions, differ in character, are thus in favour of the view that the first sound of the heart, as heard over the arteries, is not merely a transmitted sound, but is one which—to a great extent, if not entirely—is generated by the coats of the vessels themselves during their diastole. They would further tend to show that the first sound of the heart, as heard over the apex on the right side, is not transmitted from one side to the other, but is formed by each ventricle separately.

As regards the second sound in the pulmonary artery, as compared with the second aortic sound, it need only here be said that the latter is usually far sharper and more accentuated. Out of the eighteen cases compared, it was stronger in fifteen, equal in two, less marked only in one.

A second point with reference to the blowing sound capable of being somewhat elucidated by a comparison with health was, why was it inaudible during a full inspiration, and so marked in expiration? The following table, by exhibiting what effect a full inspiration has on the sounds of the heart in health, will explain it, especially if, in addition, it be borne in mind that the sound is sometimes of the same pitch as the inspiratory murmur, and that in expiration the heart's action is quickened. Ten young adults were selected, whose hearts, as far as the extent of percussion-dulness, rhythm, and impulse, proved, were perfectly normal.

Effect of a full Inspiration (the breath being held) on the Heart.

	At, or rather somewhat above, ensiform cartilage.	At apex of heart.	Second interspace on right side between cartilages.	Second interspace on left side of sternum.	Effect on impulse, and extent of percussion dulness.
1	Both sounds somewhat lessened; first most.	Both sounds much lessened.	Both sounds somewhat lessened.	Neither sound heard; second occasionally, but only very faintly and distantly.	Impulse of heart moves downward and inward towards median line; at previous seat of impulse, impulse is very feebly felt.
2	First disappears; second is heard tolerably distinctly, not quite as distinctly as previously.	The same.	First sound disappears; second faintly and distantly heard.	The same.	The same; the extent and shape of percussion dulness alters; at the upper border, it shifts nearly an inch; sounds heard distinctly over the shifted impulse.
3	First sound more indistinct; second not much changed.	Both sounds almost disappear; second least.	The same.	Nothing heard.	The same.
4	First lessens; second seems to gain.	The same.	The same.	First disappears; second heard only faintly.	The same.
5	The same.	Second disappears, in proportion, more than first.	The same; second not heard.	The same.	The same; dulness moves by an inch.
6	The same.	First disappears almost entirely; second can be heard, although faintly.	First disappears entirely; second heard as a small tick.	Both disappear.	The same; dulness moves by three-quarters of an inch.
7	Both sounds heard but are somewhat lessened.	Both sounds somewhat lessened.	First sound disappears; second heard with tolerable distinctness.	First disappears; second very faintly heard.	The same.
8	Both sounds heard; no change.	First sound duller, less distinct; second hardly heard.	First sound does not disappear, although very indistinct; second, only slightly less distinct, is well heard.	The same.	The same.
9	First sound lessened; second seems to increase.	Both sounds lessen; first proportionably most.	First disappears; second well heard.	The same; second, however, is distinguishable.	The same.
10	Sounds not much lessened.	Both sounds become very indistinct.	The same; second does not disappear, somewhat lessened.	The same; second very faintly heard.	

These observations, which have been repeated on other cases with similar results, show, then, the effect of a full inspiration on the heart sounds to be an almost entire disappearance of the sounds heard at the interspace between the second and third ribs on the left side, and a very great, but not as marked a diminution of the sounds at the aortic cartilage. At the apex, the first sound lessens very much, and becomes in some persons almost imperceptible; the second also is modified, but proportionally less than the first. Over the ensiform cartilage, the sounds lessen least. Indeed, the second sound, in four cases out of ten, was distinctly increased; and in two cases, in which the first sound became much more indistinct, the second was heard almost unchanged. These changes were all noticed in

those who were capable of taking a very full inspiration and holding it for some time, and were constant on repeated examination. I believe, although the proximity of the valves to each other renders it impossible to speak conclusively, that this accentuated second sound, heard at the spot mentioned, is that of the pulmonary valves, and has become more marked, owing to the fact that during a full inspiration, the heart's action is slower, more laboured, and the circulation of the lung probably interfered with; thus, the pulmonary artery would be distended, and the backward stroke of the column of blood against the semilunar valves be more forcible.

The fact that the heart sounds are in a full inspiration less distinctly heard at points at which they were previously well perceived, may be explained by the lung being carried in front of the heart, which it does more on the left than the right side, and, to some extent, by the relative displacement which occurs during the act of inspiration. The apex, especially, is displaced; it moves down in some persons, by several inches, towards the pit of the stomach, and becomes almost imperceptible at its previous point of impulse. This displacement seems to me to be brought about, not only by the depression of the diaphragm, but by the pressure of the lung on the left side of the heart, a fact of which I have convinced me by observations, which it is only necessary here to allude to, and which will be found elsewhere detailed.[1]

ART. IX.—*Notes on the Medical Topography, Climate, and Diseases of Panama, N. G.* By WM. P. BUEL, M. D., Surgeon Pacific Mail Co.

THE city of Panama, by reason of its geographical position, historic associations, and commercial importance, is an object of interest to the philosopher, the statesman, and the man of science. Planted on a narrow neck of land, which connects two great continents, and separates two mighty oceans, it stands like a gateway, through which must pass a great amount of the world's commerce. Already it has regular steam communication, on the east with England, the United States, the West India Islands, and the Atlantic ports of South America; on the west with all the principal ports of the South Pacific; with Central America, Mexico, California, Oregon, and the British possessions. At no remote period, Australia, the Sandwich Islands, and China, will be added to this already extended list.

Topography.—The present town of Panama is situated in lat. 8° 57′ N., and lon. 79° 31′ W.; in the northern half of that tropical belt which supplies us with coffee and sugar; spices and dye-woods; cinchona and

[1] See Transactions of the College of Physicians of December 1st.

sarsaparilla; opium and camphor, and many other most important drugs. The original city stood a few miles to the eastward of the present one. A few ruins are all that remain to mark the ancient site. A venerable old tower still survives, hoary with age; and which, probably, witnessed the mustering of Pizarro and his band of adventurers, when he was about setting forth on his memorable expedition for the conquest of Peru.

The modern town stands at the head of the bay of the same name, on a tongue or neck of land projecting into it. In shape, it is an irregular equilateral triangle, having its base towards the land, and with water on its two sides. A wall and rampart surround it, with bastions and curtains toward the sea, and a ditch on the land side. These fortifications, though they may have been a defence against Morgan and his buccaneers, serve at present to exclude nothing but pure air; and the health of the city would be improved by levelling them to the ground—a process which time and the elements are gradually, but surely accomplishing.

The sides of the triangle are about three-fourths of a mile in length, and the intramural space is regularly laid out in streets, crossing each other at right angles. As in all tropical towns, the streets are narrow; not generally more than 20 feet in width. The houses, from two to three stories in height, are built up with heavy masonry, with balconies projecting from the upper stories over the streets. There is considerable space within the walls not occupied by streets or houses, lying uncultivated, but covered with the dense vegetation of the tropics. Many old churches and convents, deserted and in ruins, furnish evidence of former opulence and large population. Outside the walls, a considerable town has sprung up. The whole population is estimated at from ten to fifteen thousand souls; all, with the exception of a comparatively small foreign population, using the Spanish language. They are by no means generally, however, of pure Spanish blood, but a mongrel race of Spanish, Indian, and African. There are but very few families who claim to be of pure blood; and even of these few, the genealogical tree will not, it is said, bear too close an inspection.

The climate is not friendly to the white races. The light-complexioned children one meets about the streets, are pale and anæmiated, with narrow chests and slender attenuated limbs. The African race, on the contrary, thrive and flourish. Their muscular frames and well-developed limbs offer a striking contrast to those of the white race. Some of the most extraordinary specimens of muscular development we have ever met, are to be found among the negro coal-heavers employed by the mail company. They would form a model for the sculptor.

The geological structure of the neighbourhood of Panama, and, indeed, of the whole isthmus, is volcanic. The mountain ridges shoot up everywhere into peaks and truncated cones; evidently craters of extinct volcanoes. The rocks are all trap or basaltic. The older rocks contain fissures, into which streams of lava, the result of more recent eruptions, containing

boulders of different sizes, have evidently been poured. The soil is a reddish loam, of no great depth, but of extreme fertility; producing, with little cultivation, in great profusion, every variety of tropical growth, whether fruit, plant, or flower. The isthmus is rich in medicinal plants, many of which are known only to the natives. As tonics and febrifuges, they employ Quassia (*Quassia Amara*), Cedron (*Simiba Cedra*), and several varieties of Gentian. The Cedron bean is also regarded as a powerful antidote against the bites of serpents, and stings of scorpions and other poisonous insects. Sarsaparilla grows abundantly upon the isthmus.

The country, in the immediate neighbourhood of Panama, is an irregular plain of several miles in extent; here spreading out into broad and beautiful savannahs, and there rising into sudden and abrupt declivities, some of them several hundred feet in height, but clothed to their summits with a dense vegetation. There are pools and spots of stagnant water in the neighbourhood, but no extensive swamp or marshes. At no great distance from the city, the country becomes broken, and rises into steep hills and mountain ridges, through which the railroad winds its tortuous way, until it reaches the summit, about ten miles from Panama. The isthmus is not distinguished by high mountains. The mighty Andes, when approaching this narrow neck of land, decreases into a ridge of hills seldom attaining the height of a thousand feet. The railroad winding through the irregularities of the mountains, has only a grade of something less than three hundred feet to be overcome.

Climate.—The year divides itself into rainy and dry seasons. The rains commence in the spring—soon after the sun in his journey northward has crossed the equinoctial line—and continuing, with more or less regularity, during the whole period that he is north of the equator, terminate not long before he reaches the line again in his journey south. South of the equator on the South American coasts, the seasons are exactly reversed; there the rains commence about the time of our autumnal equinox, and terminate about the time of our vernal. The law is the same in both cases, viz., the rains follow and accompany the sun. Farther north the rule is exactly reversed. In California, the rainy season is when the sun is in the far south; the dry when it is farthest north. It is easy to see why the cooling process, which takes place in California during the winter months, should condense the atmospheric moisture upon the sides of the mountains, and bring with it copious rains. It is not so easy to explain why the same rule should not hold good in the intertropical regions.

The rains, which at first are but passing showers, gradually increase, and are fully established towards the end of May, when they fall in torrents, sometimes for days in succession, and are accompanied by thunder and lightning of the most terrific description. With the exception of a few days about the 24th of June, called by the natives the Veranito de San Juan, analogous to our Indian Summer, the rains continue for six or eight

months. During this time, fogs, calms, and light, variable winds prevail. The air is loaded with so much moisture, that leather cleaned in the morning is often loaded with mould in the evening.

During the wet season the thermometer (Fahr.) ranges from 75° to 85°, seldom falling below the first or rising above the last. This is not an extreme degree of heat; but the extreme humidity, by preventing all evaporation, clogs the perspiration, and produces a sense of oppression not experienced in a dry atmosphere with the same or even a higher temperature.

Towards the end of December the rains diminish in frequency, and with the commencement of the new year the northwest wind sets in. An immediate change follows. The air becomes pure and refreshing, the sky blue and serene. Hardly a cloud is to be seen; and there being but little moisture in the atmosphere, the heat, though ranging from 75° to 90° Fahr., is less oppressive. The nights are almost always cool, with a strong breeze from the northwest, which makes a blanket generally comfortable to sleep under. The early morning is cool and pleasant; but about 9 o'clock the breeze dies away, and from that hour till 2 or 3 o'clock P. M. the heat is intense. The evenings at this season are deliciously cool, the air fresh, the sky without a cloud, and the stars glittering with a splendour known only in the tropics.

Diseases.—Panama has the reputation of being a place of extraordinary insalubrity—a sort of a hot-bed of disease—where the fever which bears its name reigns with undisputed supremacy. This is in a great measure unjust. Among intertropical cities, it is more healthy than the average. When it is considered that it is compactly built, surrounded with walls which exclude the air and prevent free ventilation—that it has no sanitary police, no sewerage—that the filth which in other cities is either carried off in underground sewers or received into deep cesspools, is here left to fester upon the surface —when all these facts are remembered, Panama may be considered a healthy town. A strong evidence on this point is, that no epidemic disease has ever prevailed here extensively—neither cholera, nor yellow fever, nor dysentery, nor any other form of epidemic disease. Sporadic cases of all of them are of occasional occurrence, but seldom or never in an epidemic form. When yellow fever is prevailing at the island of St. Thomas, between which and Aspinwall there is constant intercourse by the semi-monthly steamers, sporadic cases of the same disease often occur on the line of the railroad and at Panama; but the disease does not spread extensively.

The most prevailing diseases, and those which may be considered *endemic*, are dysentery and that form of miasmatic fever variously called Panama, Chagres, or isthmus fever, termed by the natives "calentura." It is comparatively a mild form of febrile disease, and perfectly controllable by quinia, with very little other medication, and no preliminary treatment. The Spanish physicians, with the attachment to ancient modes and practices so strong in their race, still insist upon bloodletting as an indispensable preliminary to the quinia; thus entailing upon their patients a tedious conva-

lescence in a climate where the vital energies are low, and the recuperative process never rapid.

Bronchial affections are very common among the native population, accompanied oftentimes with obstinate cough and a profuse expectoration. Tubercular phthisis is of occasional occurrence, but is comparatively rare. Smallpox and the other exanthems are met with, but usually in a mild form, and are seldom epidemic.[1]

The most sickly period of the year is usually towards the termination of the wet and at the commencement of the rainy season, or the months of September, October, and November. Dysentery is then very common; and also a high grade of bilious fever, which in malignity and fatality falls little short of yellow fever—and is attended by the same yellow tinge of the skin, injected conjunctiva, and irritable stomach which mark so strongly that disease. Quinia, so effectual in the ordinary fevers of the country, is in this almost or quite powerless. It often runs to a fatal termination in from four to eight days from the commencement of the attack, with sometimes hemorrhages from the mouth and gums, and symptoms of congestion in the brain, lungs, or liver. These malignant cases are, happily, not of frequent occurrence.

The foreign population of Panama is not numerous; it is composed of citizens of the United States connected with the railroad company and the Pacific Mail Company in different capacities, and a few French, English, and Germans, engaged in commercial operations. Some few of this class of the foreign residents enjoy uninterrupted health; the majority experience attacks of fever more or less frequently, and in the intervals of exemption suffer from the depressing and debilitating effects of the climate. It may be doubted whether there is any such thing, for natives of northern and temperate regions, as acclimatization in tropical climates, so as to enjoy an exemption from their peculiar diseases equal to that of the native population. If it ever occurs, it must be considered the exception, not the rule.

Hospitals, and Provision for the Sick.—Some years ago, when the tide of California emigration was at its highest pitch, there were at times as many as two or three thousand Americans congregated at Panama, waiting for a passage to California. Among these masses many were destitute, many dissipated and imprudent, and all unaccustomed to the climate. Much sickness, of course, prevailed. To meet this, private hospitals were opened by physicians, as a speculation. This answered very well for those who had funds, but afforded no relief for the large number of destitute. To meet the latter exigency, a public hospital for Americans was opened, supported in part by voluntary subscriptions, and in part by a capitation-tax upon

[1] Since the above was written I learn that about twenty-five years since there was an epidemic of smallpox, which committed extensive ravages. Many people of twenty-five years and upwards are seen about the streets, badly pitted, survivors, doubtless of that epidemic.

passengers. But, with the exigencies that required them, all these have long since ceased to exist. Sick and destitute Americans are succoured by private charity; or, if mariners, by the American consul. The French population have a small hospital, sustained in part by voluntary contributions, and in part by the French government.

For the native population there is a hospital with a permanent endowment. The hospital building was erected for the purpose about ten years since. It stands without the walls, in one of the streets of the suburbs. It is a long one-storied stone structure, and, with its tier of small windows, elevated eight or ten feet above the street, and heavily grated, has more the aspect of a prison than a hospital. It has four or five wards, with a kitchen and office, opening on a corridor with pillars and arches of heavy masonry. Over one of the doors of the main entrance is an inscription with these words: "VENITE AD ME, OMNES VOS QUI LABORATES ET ESTIS ONERATE, ET EGO VOS REFICIAM." The whole aspect of the interior is a sad commentary upon this gracious invitation. The wards are dark and ill-ventilated, scantily furnished with a few old iron bedsteads without mattresses or pillows. The patients are mostly old chronic cases, sent here merely to prevent their dying in the streets, and without the slightest hope or expectation of recovery. And as if to say to every unfortunate patient who enters the place, there is but one possible way of leaving it, a grim display of old coffins, of all sizes and styles, from plain wood to shabby black velvet, occupies the most conspicuous place in the corridor already mentioned. In this country, to be buried in a coffin is a luxury only for the rich. The poor man is borne to the grave in a coffin, the body interred, and the coffin reserved, to be used over and over; so that these hospital coffins may have borne a whole generation to their long home. This place, its inmates, and everything connected with it, wear an appearance of squalid filth and hopeless misery, which are a disgrace to any community calling itself civilized.

PANAMA, N. G., October, 1858.

ART. X.—*Case of Pseudarthrosis of the Bones of both Forearms; Failure of Brainard's Operation, and of Excision.* By R. A. KINLOCK, M. D., Surgeon of the Roper Hospital, Charleston, S. C.

NICHOLAS VANDERWICH, a Hungarian, aged 32 years, of exceedingly robust and athletic appearance, and enjoying perfect health, was admitted into the Roper Hospital, on the 8th of March, 1856, on account of false joints of the bones of each forearm, the result of fractures sustained ten months previously, while residing near Georgetown, S. C. The fractures were occasioned by a heavy chain falling from a height, and striking the forearms while the upper extremities were outstretched, and the hands

grasping a horizontal bar. He had received surgical attention at the time, but the apparatus employed became frequently deranged, and the surgeon, living at a distance, could see him only at long intervals of time, consequently the result was ununited fracture of the four bones. For many months he had been without any adjustment, and daily used his arms to the best of his ability, to procure a livelihood. The examination at the hospital revealed well-marked false joints, situated nearly midway between the wrist and elbow joints of both forearms; those of the right side were a little higher up than those of the left. There was marked shortening of both forearms, they appearing very disproportionate to the length of the arms. What was the precise amount of shortening could not be determined, as we were unable to make the comparison between an injured and a sound limb. The right forearm, however, was shorter than the left, because of greater riding of the fragments. The fragments of the right limb were only loosely held together, while those of the left were more nearly in apposition, and well secured to each other by short ligamentous union. The ends of the eight fragments were smooth and rounded off, as far as could be discovered through the soft tissues. The flexor and extensor muscles of both the forearms were shortened from permanent retraction; the muscular fibres seemed doubled up on each other; extension and counter-extension occasioned but little muscular elongation, and overcame in no material respect the riding of the fragments. The prehensile movements of the hands were perfect, and so were the functions of the elbow and wrist-joints, and the movements of flexion and extension; pronation and supination were limited. The muscles of the forearms, like those of the entire body, were powerful, and the patient could lift, with the hands, very heavy weights to a certain extent perpendicularly from the earth; when, however, the arms were outstretched horizontally, they were comparatively useless.

It was evident, then, that an operative procedure offered the patient the only chance of relief from his distressing condition. Union of the bones was all that we could expect to secure; to overcome the riding, and to restore the natural movements of pronation and supination, seemed practically impossible. The patient's robust health inclined us to expect nothing from constitutional treatment. The length of time that had elapsed since the occurrence of the fractures, and the present condition of the parts implicated, forbade us to hope anything from absolute and long-continued rest of the fragments, or from such moderate excitation as could be induced by friction repeated from time to time. We agree with Mr. Syme, of Edinburgh, that of the many operative procedures employed for the cure of pseudarthrosis, the resection of the bones involved, or, in other words, the cutting out the false joint, is the most rational, and the one most likely to be followed by success. But surgeons, as well as their patients, naturally and properly shrink from very severe operations, where there is a reasonable prospect of cure by milder ones. Of the milder means, I concluded to select the procedure lately described and known as "Brainard's operation," because of the great success attributed to it, and because, practically, it was to me a novelty. With the left forearm, I had every hope that it would succeed; but I scarcely believed that it would prove serviceable with the right. I resolved to operate first on the left member alone, and allow the patient the use of the right; by so acting, I would not subject him to as much inconvenience, and there would be more certainty of securing for the desired time absolute rest to the member operated on. These advantages I conceived to more than counterbalance the disadvantage which

would arise from consecutive operations necessitating his remaining at least double the time under treatment.

March 11. The patient being fully under chloroform, the left arm held in a semi-prone position by an assistant, I introduced one of Brainard's drills, subcutaneously, upon the posterior and radial aspect of the member, immediately over the false joint; this was then carefully forced on until the point reached the portion of the upper radial fragment, in close proximity to the abnormal articulation. This fragment was steadied with the fingers and thumb of the left hand, applied respectively upon the anterior and posterior aspects of the forearm, while a perforation was effected with the drill; the instrument was then withdrawn from the opening made in the bone, without being allowed to repass through the soft tissues, and the point fixed upon an adjacent spot, where a second perforation was accomplished, and after this a third in close proximity to the two others. The lower fragment was then attacked, and several perforations made with the drill, the left hand steadying this as it had previously done the upper fragment. Finally, the point of the drill was carried between the ends of the fragments, and turned about so as to lacerate their connecting fibrous medium; then, for the first time, it was withdrawn through the original cutaneous puncture. While perforating, the point of the drill was always directed from the important structures lying to the ulnar side of the radius. The fragments, and the false joint of the ulna, were now operated upon in the same manner; the point of the drill, during the procedure, being directed from the important parts now lying to the radial side of the bone. The operation finished, the small cutaneous wounds were dressed with collodion and lint; the member was put up as after a recent fracture; an anodyne was given, and the patient sent to bed. He suffered from day to day very little inconvenience; the arm was kept at rest by the apparatus—which was only once or twice disturbed in a fortnight, to permit an examination of the progress of the case—but he was allowed the freedom of the ward.

April 4. Considerable callus was found to have been deposited; but as yet there was nothing like consolidation. I repeated the drilling operation on the left forearm; and thinking that success would soon be attained, and the use of this member consequently permitted, I decided to operate at once, and after the same manner, on the right forearm, so as to save the patient time. The operations were conducted as before described. Considerable pain and some swelling succeeded the operation on the right member.

12*th*. The swelling of the right forearm had increased to an extent which rendered it necessary to remove the apparatus and readjust it, for fear that the circulation of the limb might be interfered with.

14*th*. The apparatus removed from the left forearm, and the parts carefully examined. Union of the ulnar fragments seemed to be promised, as the consolidation was more decided than upon last examination; the radial fragments were quite free, and no callus discoverable around them.

16*th*. Patient complains of increased pain of the right forearm; this found to be more swollen. The adjustment was removed, when careful manipulation discovered a purulent collection upon the posterior and radial side of the member. The abscess was punctured, and found to be small and circumscribed.

18*th*. Condition of right forearm much better; swelling greatly subsided.

May 13. All swelling and excitement having disappeared from right forearm, and there seeming to be no attempt at union, I resorted again to the drilling.

21*st.* Right forearm again suffering from abscess at the radial side; this I opened, and found more extensive than the last.

22*d.* Left forearm examined, and found consolidated. The drilling operation repeated.

23*d.* Another abscess of right forearm was opened posteriorly, and more towards the ulnar side than the last one.

June 3. Still another small abscess of the right forearm evacuated; the apparatus was removed, and the member poulticed while lying upon a pillow.

5*th.* Left forearm examined; callus apparently consolidating about the ulnar fragments; no favourable change with the radial fragments.

9*th.* Some appearance of erysipelas about the left hand, with general febrile excitement. Patient ordered to be purged with blue mass and castor oil, and then put on quinine.

13*th.* Patient's condition much improved; erysipelatous inflammation vanished; left forearm put up in a starch bandage, as I determined not to repeat the drilling.

16*th.* Abscesses of right forearm have ceased discharging; ulcers almost healed.

July 30. Patient discharged by request, promising to return to the hospital in cooler weather, to submit to the operation of excision. His condition was very much the same as upon entrance.

February 17, 1857. Nicholas Vanderwich was readmitted into the hospital. His health was robust, and his forearms in very much the same condition as when he was discharged.

21*st.* The patient was chloroformed, and longitudinal incisions, about three inches long, were made upon the radial and ulnar borders of both forearms, immediately over the false joints. The joints being fully exposed, the soft tissues about the bones were disturbed only sufficiently to allow of my passing the large curved and grooved resection sound successively under the different fragments, above and below the abnormal articulations; the section of the bones was made with the common amputating saw, only enough of each fragment being taken away to admit of good apposition of the opposing extremities. The incisions were closed by sutures, supported with adhesive strips, and the arms put up with rectangular splints applied laterally, as in fractures about the elbow. After the patient was put to bed, he was ordered: ℞.—Pul. gum opii gr. ij; this to be repeated in three hours, if awake and in pain.

23*d.* Patient had some febrile excitement last evening; this now passed off, and he feels comfortable; the forearms only a little sore; bowels constipated. ℞.—Hydr. chlor. mite gr. v; pul. Doveri gr. x. Take in syrup at bedtime, and in the morning a dose of castor oil.

24*th.* The dressings were removed from the right arm, as patient complained of more uneasiness from this than the left; wounds found to be suppurating a little. The sutures were cut away, and the member supported in a rectangular wire gutter, instead of the angular side splints; this arrangement gave greater facility for examining the parts, and kept them at a lower temperature. Liberal diet of milk and beef-soup allowed, with a pint of porter per diem.

26*th.* Right arm dressed; a good deal of suppuration from the superficial and deep tissues.

27*th.* Both arms dressed. Left looking well; sutures removed; union of wounds nearly complete, with very little suppuration; wire gutter applied.

28*th*. Right forearm much engorged; diffuse suppuration threatened. Patient kept in bed; limb to be covered with lint, and irrigated with cold water. ℞.—Tinct. opii gtt. xx, every three hours; diet milk, *ad libitum*, with pint of porter continued.

March 1. Engorgement greater; gangrene threatened. Free incisions made through the skin and fascia to relieve tension; tinct. opii and porter continued, with a double dose of the tincture at bedtime; diet same.

4*th*. Right arm looking better; swelling somewhat subsided; general febrile excitement much less. Water-dressing continued, and the tinct. opii diminished to gtt. x three times a day.

6*th*. Both arms dressed to-day. Left forearm looks remarkably well; wounds healed without deep suppuration. Right forearm still improving; a small abscess discharging on the posterior aspect near the elbow; same dressing and treatment continued.

9*th*. Right arm placed back into the angular gutter splint; stop tinct. opii, and give at bedtime pul. Doveri, gr. x; liberal diet, with porter, continued.

15*th*. Right forearm again disposed to inflammatory action; patient's bowels constipated. Splint removed, and member supported upon a pillow; cold water-dressing renewed; ordered: ℞.—Pil. hydr. mass and ext. colocynth comp. āā gr. v in pil. No. 2. To be taken at once, and followed in three hours by a dose of castor oil.

16*th*. Right forearm much better; abscess near elbow still discharging some; wounds inclined to heal.

20*th*. Wire splint reapplied to right arm.

24*th*. Both forearms look very well; but no attempts at osseous union manifest in either.

29*th*. Patient now walks about, with both arms well secured with splints and bandages; has best diet of the house, with porter continued.

April 4. Left forearm examined; ends of bones perfectly movable; friction of the ends resorted to, with the view of increasing plastic deposit; splints reapplied.

17*th*. Nothing like union detected in the bones of either arm.

24*th*. Right forearm somewhat more inflamed; superficial ulceration, with slight purulent discharge near the elbow. Ordered a dressing of tannin and glycerine to ulcer, and ℞. sul. magnesia ℥ss, to be taken in water.

30*th*. Right forearm much better; left forearm healed; no consolidation of the bones in either.

May 22. Ulceration of right forearm entirely healed; no union in either member.

June 1. No improvement; fragments much more movable than before operation; patient discharged by request, as he was considered incurable; directions were given as to the best mechanical support to be employed, in order to secure some degree of usefulness of the suffering limbs.

Remarks.—The above are the details that I conceive important to the understanding of the case; I have omitted many items in regard to variations of treatment and diet, in order to economize space. I will say here, that at one time the patient was kept upon the phosphate of lime, but without perceptible result, so I have given no daily notes in regard to its use. As to the whole treatment, it was unsatisfactory in the extreme. It would have been better for the patient had he never entered the hospital

ward. Recent reports from the London hospitals lead us to expect very unsatisfactory results from the treatment of pseudarthrosis by any of the usual methods. The truth of this opinion is strongly corroborated by the above case. It appeared to me a remarkably favourable one for operation, and any surgeon must have experienced some disappointment at the successive failures of persevering efforts. The patient's robust health; his great courage; his manly endurance and astonishing patience, made me hopeful in the face of authoritative and discouraging opinion. Why the mere delay of union of a fractured bone for a few weeks or months, should dispose to such a change in the character of the nutritive acts of a part, as to lead to the abrogation of the beautiful law of analogous formation, is a mysterious problem, which science has not yet solved. Many of the ancient surgeons were so impressed with the danger of the operative procedures practised for the relief of this pathological condition, that they preferred non-interference. With a good many moderns, the question is only as to the degree of risk attending the several operations. If resection be the one most likely to give bony union, it is also thought by many to occasion most danger to life and limb. The advocates for the milder plans of operation, have, probably, been too hasty in fixing their value. It will be mortifying if modern surgery be compelled to abandon operative interference, not solely from the dread of dangerous consequences, but because of the utter impotency of art. We had hoped much from the procedure of Dr. Brainard, yet this is the second time that it has signally failed in our hands. True, the successive operations with the drill in the case just reported, were not practised at the precise intervals of time suggested by Dr. B.; the complications of the case, and the results following some of the drillings, prevented a rigid adherence to his directions. But, nevertheless, a very fair trial was given to the procedure, and it failed. We are not publishing the instance, however, with any view of condemning the operation. This would be premature, as the result proved that resection was attended with more danger to the patient, and was followed by as little success as the operations with the drill. We can only assume that in the case before us there was some radical defect in the nutritive acts of the bony tissue implicated, which, as before intimated, is not explicable in the present state of science.

ART. XI.—*Obstinate Hemorrhage following a division of the Frænum Linguæ.* By A. REEVES JACKSON, M. D., of Stroudsburg, Monroe Co., Pennsylvania.

HAVING occasionally seen reported in the medical journals, cases of fatal hemorrhage from the division of the frænum linguæ in children, I am in-

duced to relate the following case, in which a very simple contrivance was entirely successful in checking the bleeding, after an operation of this kind.

Some years ago I was called to see an infant, aged eight months, son of Mr. S——, near White Haven, Pa., in consultation with Dr. H., under the following circumstances. Thirty-six hours previous to my arrival, Dr. H. had divided the frænum linguæ, and the wound had been bleeding ever since, all the efforts that had been made to check it having been unavailing. Dr. H. was not present when I reached the place, but in a note which he had left for me, he desired me to do what I could for the little patient, and stated that he had already used, unsuccessfully, cold applications, a variety of styptics, lunar caustic, the ligature, and the actual cautery.

The child was already very greatly reduced, from loss of blood, which was continuing to ooze out from the cut edges of the wound.

I procured from the father of the child (who was a deer-hunter), a few buckshot, and flattened two of them out into disks, or round plates, by means of a hammer, using the side of an axe as an anvil. I then pierced each of these through the centre, with a common sewing-needle. Then taking a piece of annealed silver wire from a double canula in my pocket-case, I tied a knot on the end of it; and having split, with a pocket-knife, half way through another shot, placed the wire in the bottom of the slit, the sides of which were then firmly pressed together with a pair of tonsil-forceps, care being taken at the same time to draw the knot, on the end of the wire, close to the shot. The free end of the wire was then passed through the hole in one of the plates, which was drawn close against the shot. The other plate was now passed up to within a quarter of an inch of the first, and a second shot, previously split like the first, placed against it, but not pressed so tightly upon the wire, but that it could be moved with a moderate force.

My instrument, which, it will be perceived, formed a clamp, was now ready to be applied, which was done in the following manner:—

The father, having taken the child in his lap, and held its mouth forcibly open, I applied the instrument in such a way, that the whole of the cut frænum was brought between the flat surfaces of the two plates. The second shot was now pressed strongly against the outer side of its corresponding plate by means of the forceps, and by pulling, at the same time, upon the free end of the wire. The plates were in this manner brought closely together, and were kept in their position by pressing together firmly, the split in the second shot. The bleeding was immediately controlled, and nothing remained but to cut off the end of the wire, close to the shot.

The child was then allowed small portions of wine-whey, every two or three hours; and, at the end of about twenty-four hours, when the clamp was removed, there was no return of the hemorrhage.

I think, in case of emergency, the wire might be replaced with a piece of stout linen thread, although the former is certainly preferable.

REVIEWS.

Art. XII.—1. *Hygiene, or Health as depending upon the Conditions of the Atmosphere; Food and Drinks; Motion and Rest; Sleep and Wakefulness; Secretions, Excretions, and Retentions; Mental Emotions, Clothing, Bathing, &c.* By James H. Pickford, M. D. London: John Churchill, New Burlington Street, 1858.

2. *Papers relating to the Sanitary State of the People of England: being the Results of an Inquiry into the different proportions of Death produced by certain Diseases in different districts in England.* Communicated to the General Board of Health by Edward Headlam Greenhow, M. D., Lecturer on Public Health at St. Thomas's Hospital, and Physician to the Western General Dispensary. With an Introductory Report by the Medical Officer of the Board (Dr. John Simon), *On the Preventability of certain kinds of Premature Death.* Presented to both Houses of Parliament by command of Her Majesty.

3. *Ventilation of American Dwellings; with a series of Diagrams, presenting Examples of different classes of Habitations.* By David Boswell Reid, M. D., F. R. S. E., &c. To which is added *An Introductory Outline of the Progress of Improvement in Ventilation.* By Elisha Harris, M. D., late Physician in Chief to N. York Quarantine Hospital, &c. N. York: Miley & Halsted, 351 Broadway, 1858.

The simultaneous appearance of these works, the first two *trans*, the other *cis*, Atlantic, is more than an intimation that the subject of public health, with its long train of important influences upon the condition and longevity of the human race, is attracting its deserved attention from the professional as well as general public in both hemispheres. The devoted attention which this subject is receiving, is the addition of another to the already long list of advancements which have marked the present as a wonderful age in the progress of human knowledge and improvement. The nineteenth century opened with a promise, in the great discovery of Jenner, of the further development of wonderful and glorious facts from the arcana of science. The promise was early redeemed by Sir H. Davy and John Dalton, who furnished the keys which unlocked the great storehouse of *Chemistry*, from which innumerable riches have been drawn. The application of steam as a propelling power both on land and sea, soon gave increased facilities of movement; while, not to mention a host of other improvements of less striking, but scarcely less important character, the discovery of Photography, of Anæsthesia, and the application of electricity and magnetism to Telegraphing, by which finally "a girdle is put about the earth in forty minutes," crown the present period as being the most wonderful in point of human progress that the stars, in their eternal courses, have ever witnessed.

Amid these remarkable evidences of advancement, many of them the emanations of medically educated minds, great achievements are being made

in the cure of diseases and the prolongation of life, and the subject of *preventive medicine* has not remained entirely unheeded or undeveloped. Many of the old scourges of our race, which were once deemed inevitable as death, have been demonstrated to be absolutely and entirely preventable and unnecessary. For example, it is but as yesterday that John Howard published to the world the astounding condition of the prisons of Europe, which showed, to use his own phrase, "how full of emphatical meaning is the course of a severe creditor, who pronounces his debtor's doom *to rot in gaol;*" but where now shall we find an instance of *jail fever?* So too *scurvy*, the sailor's scourge (by which Anson, in his celebrated voyage of 1740–2, lost within the first ten months nearly two-thirds of his crew, and during the remaining period about half the survivors; and of which disease there were admitted into Haslar Hospital, in 1780, 1457 cases.) Scurvy is now never seen by one physician in a hundred, and in fact occurs only under the most peculiar and accidental circumstances, so well has its prophylaxis come to be understood. In this category we might include several other instances, but we forbear, as the theme has been dwelt upon at length by abler pens. We merely advert to the topic to show that medical minds have not been altogether indifferent to the true spirit of their mission; at the same time, it is but due to justice to declare the opinion, that the profession has not exerted itself to the full extent that it should, in the matter of general preventive medicine. Too much absorbed in the pursuit of a living to enable them to turn from the treatment of individual diseases to the public service of their prevention, and alas! that it must be said, suffering too much from the prevalence of quackery, the public health has correspondingly suffered. But this opprobrium is rapidly wearing away, when such as they whose names appear on the title pages above quoted—names which have already become worthy of regard as labourers in the walks of chemistry, pathology, physiology, and general medicine—when such persons are seen to enter this new field, and to put forth their energies for the development of its fruits, then we may rest assured that the work will be carried forward until all that can, will, in due time, be accomplished. And it behooves every member of the profession to see that he is no laggard in the road towards a full knowledge of the science of public hygiene, even though he may not choose to engage in its practical application; for the time is rapidly approaching when the suppression of every health-impairing circumstance will be demanded by the public voice, and the medical practitioners of every vicinage will be the authorities for magisterial action. How important, then, that every one should be fully prepared to render a correct and intelligent judgment upon everything appertaining to the subject.

The three works before us belong to three several branches of the subject; the *first* to its *philosophy*, and the elucidation of the great natural causes which influence human health for good or for evil; the *second* (which has not yet been publicly issued, but with a copy of which we have been politely favoured in anticipation, by the distinguished author of the Introductory Report) is an elaborate and valuable document, the result of great labour and research among the death returns of England; and the *third*, from the pen and pencil of the distinguished Edinburgh (now of N. York) chemist and hygienist, is an exposition of the various modes by which the principles of *domestic ventilation* may be effectively carried out in practice.

The work of Dr. Pickford comes very *àpropos* to supply a great deside-

ratum to the student of hygiene—a condensed summary, in manual form, of the great facts relating to it. The discussion of its natural laws and principles have heretofore been scattered through numberless volumes, which have required the greatest toil and difficulty to collect together upon any one branch. The author's aim has been to present to his professional brethren, to the medical student, and to the public at large, a faithful transcript of accurate research, observation, and experience, a statement of facts and admitted truths, together with such inferences and deductions as appear warranted and demanded.

Very copious authorities have been consulted, and where differences in statements or opinions occur, they are placed in juxtaposition, so that the reader may draw his own inferences, or exercise his own experimental knowledge on the subject, while the very abundant references will enable every one to read more at length on any branch of the subject treated of. The *first part*, which alone is before us, treats of the physics of the atmosphere, the seasons, temperature, rain, winds, and pressure; the respiration of plants and animals, the circulation of the blood, the chemistry of respiration, and animal heat; infection, contagion, malaria, sewerage, drainage, ventilation, and climate in connection with disease.

The *second* and *third parts*, which are to follow, will embrace the other branches of the main subject.

Not only is the present volume copiously indexed, but its paragraphs are numbered successively from the beginning to the end, there being 1375 in all.

To exhibit more clearly the character and style of this timely and excellent work, we extract a few of them.

"110. *Atmospheric Tides.*—The atmosphere has its tides and currents like those of our great oceans. These are apparently of two kinds; the one the result of the heat of the sun's rays, the other of the attraction of the moon.

"111. Throughout the world there are two daily atmospheric tides. Within the tropics the flow of the atmospheric tide, according to Humboldt, takes place with the greatest regularity at 9 or 9¼ A. M., and at 10 or 10¾ P. M.; and the ebb at 4 or 4½ P. M., and at 4 A. M. The latter is attributed to the expansion of the air during the hottest part of the day; the former to the pressure of the masses of cool air.

"If the height of these tides be proportionable to the difference between the specific gravity of air and mercury, the morning tide will be about 13 feet, and the evening tide 25 feet."

"331. *Ozone* (from ὄζω, to stink).—This is one of the ingredients of the atmosphere, the discovery of which was claimed, in 1848, by Professor Schönbein, of Basle. A reference, however, to pages 342, 343, of Dr. Prout's *Bridgewater Treatise*, published in 1834, and to pages 569, 570, of the appendix of the same, will show that he had already discovered the existence of this compound, which he believed to be 'analogous to, if not identical with, the deutoxide of hydrogen.' Dr. Prout was of opinion 'that the excess of oxygen above the amount of 20 per cent. which there ought to be in the atmosphere, if its composition were, as there can be little doubt that it is, determined by the law of chemical proportions, becomes associated with the vapour of the atmosphere, and forms a deutoxide of hydrogen. The oxygen and vapour in this combination, says Dr. Prout, are feebly associated and appear to be separated by the slightest cause.

"332. Ozone is a teroxide of hydrogen, consisting of three atoms of oxygen=$16\times3=48$, and one of hydrogen=1.

"333. Schönbein believes ozone to be a regular constituent part of free atmospheric air, inappreciable though varying in quantity, and to be everywhere incessantly and naturally formed out of atmospheric oxygen, in consequence of electrical discharges constantly taking place in the air.

"De la Rine and Berzelius consider ozone to be nothing but allotropized oxygen.

"Scoutetten defines ozone to be oxygen positively electrified."

"334. Ozone, says Schönbein, is the most powerful oxidizing agent we yet know of, transforming, in the cold, even silver into the peroxide of that metal, iodine into iodic acid, nitrogen (a strong base being present) into nitric acid. The '—ous' acids into '—ic acids, the '—ites' salts into '—ates' salts, the metallic sulphurets into sulphates.

"335. Ozone destroys instantaneously sulphuretted, seleniuretted, phosphoretted, ioduretted, arseniuretted, and stibiuretted hydrogen, oxidizing their constituent parts.

"336. Schönbein has demonstrated ozone to be one of the chemical antipodes and antidotes to all oxidable miasmatic and malarious gases and emanations disengaged from putrefying animal and vegetable substances, converting them into innocuous matter, and thus purifying and sustaining the entire salubrity of the atmosphere. In short, so hostile to organic miasmata, so incompatible with them is ozone, that the presence of the latter enables us to affirm the absence of the former, and the healthiness of the locality in which it is found.

"337. Ozone is produced in large quantities over lands covered with luxuriant vegetation, and over water.

"338. Ozone is found in abundance on the sea-coast, and on mountains and elevated localities; yet, in reality, it does not progressively increase in quantity in the ratio of elevation. But its diminution or absence in the lower atmospheric strata, depends on its destruction by miasmatic emanations, with which it has come in contact, and which it has decomposed and made inert.

"339. When the mean amount of ozone indicated by the ozonometer on the sea-coast, at an elevation of 85 feet, was 2.2, it amounted inland, at the same elevation, to 0.6; at 170 to 1.3, and at 225 feet to 3.8.

"340. Ozone, like the deutoxide of hydrogen, is remarkable for its bleaching properties. Its presence is more strongly marked during the night than in the day.

"341. Pure ozone, perhaps on account of its exalted oxidizing powers, is a most powerful poison, and when inhaled into the lungs, even in minute doses, produces deleterious effects, and in large doses quickly destroys the strongest animal life.

"342. The inhalation of ozone produces great acceleration of the respiration, a painful constriction of the chest, not unlike asthma, spasm of the bronchial tubes, violent cough, irritation and inflammation of the mucous lining of the bronchiæ and air-passages, catarrhs, coryza, possibly hay fever, intense pneumonia, &c.

"343. MM. Scifferdecker and Bockel, probably on insufficient data, believe there is no connection between ozone and bronchitis, pneumonia.

"344. Drs. Moffatt, Schönbien, and Scoutetten, are of opinion that a proper admixture of ozone and atmospheric air exercises an important influence on the animal economy, and is indispensably necessary to the due accomplishment of all the vital functions, and to the relief and modification of disorder and disease.

"345. In confined places where ozone cannot penetrate, plants and men become blanched; the skin grows pallid, the blood loses colour, lymph predominates, all the tissues soften, and serious diseases of the adynamic type break forth.

"346. The presence of ozone in the atmosphere or water is readily detected by test paper prepared by saturating strips of white bibulous paper in a mixture made by boiling one drachm of white starch in an ounce of distilled water, for three minutes, in which are to be dissolved, when cold, twelve grains of chemically pure iodide of potassium. The discoloration of paper, thus prepared, to brown, on exposure to the atmosphere, and to purple, when immersed in water, indicates the presence of ozone; the degree of discoloration, its intensity and amount. The change of colour is owing to the oxidation, by the ozone, of the potassium of the iodide, and by the combination of the iodine, thus set free, with the starch, to form an iodide of starch."

"573. South winds are highly ozoniferous, and probably on this account, pro-

duce catarrhs and bronchitis. They soothe and allay a dry and irritable condition of the mucous surfaces of the air-tubes and cells, and greatly alleviate the sufferings, and indefinitely prolong the existence of the phthisical patient."

In the following paragraphs are set forth facts of great importance, not only in the treatment of diseases, but we commend the statements particularly to the attention of the keepers of some prisons we wot of, in which, the punishment by flogging having been declared illegal, resort has been had to the not less cruel and far more *dangerous* infliction of the *shower-bath:*—

"758. The pulse may be reduced 50 beats in the minute, and may be rendered irregular and quite imperceptible, by the long-continued action of cold water on the surface of the body.

"759. A shower or *douche* bath, delivering per minute 30 to 40 gallons of water at 64° or 68°, will occasion the immediate depression and reduction of the pulse to this extent.

"760. A shower-bath of eight gallons only, at 47°, reduces the *volume*, but does not affect the frequency, of the pulse. At 74° or 110° no perceptible effect is observed."

"*The Glucose Function of the Liver.*

"832. It may not be altogether irrelevant to the subject under consideration to allude to the original and important discovery which has within the last few years been made by Prof. Claude Bernard, the successor of Magendie, with reference to the functions of the liver. This gentleman has ascertained that the liver in man and mammals *secretes a saccharine matter—glucose*—which is conveyed from the secreting cells, by the inter-, intra-, and sublobular veins, to the venæ cavæ hepaticæ, and thence, in the course of the circulation, to the vena cava inferior and right auricle of the heart, and by the pulmonary arteries to the lungs, where it is burned, thereby contributing to the sustentation of animal heat.

"833. M. Bernard, after repeated experiments and the most careful investigations, could not detect sugar in any other portion of the circulating system, and he therefore felt himself justified in asserting that sugar is a normal product of the liver, and that the saccharine matter so secreted cannot be detected in health in any secretion, vessel, organ, or tissue of the body, other than in the blood of the veins above mentioned, and in the substance of the liver itself."

"837. But even admitting, for the sake of argument merely, that sugar has been found in the chyle, this neither invalidates nor detracts from the importance of M. Bernard's discovery that the liver secretes, separates, or eliminates a vegetable substance (*glucose*) from animal matter alone, or from both combined; neither does it disprove his statement that the substance so produced assists, by its combustion in the lungs, in sustaining animal heat. It might, if satisfactorily established, tend to show that even glucose may have its antecedents.

"838. Apart, however, from the interest which attaches to this discovery in a physiological point of view, it originates questions of the gravest import both to the pathologist and to the physician.

"839. If the quantity of saccharine matter secreted or separated be not entirely burned in the lungs, the surplus will circulate in the system, to be eliminated by the kidneys as diabetic urine.

"840. The question therefore arises, whether, in seeking for the cause of diabetes, we should not look to impaired, defective, or embarrassed respiration, rather than to a faulty or perverted action of the assimilating organs, or of the kidneys themselves; and whether, in the treatment of this disorder, we should not suggest such measures as may contribute to the increase, where possible, of the respiratory functions, if these be in defect, and, as a consequence, to a larger consumption of the glucose.

"841. Sugar has been constantly found in the urine of those whose respiration had become enfeebled by age.

"842. Again, if the liver, from disorder or disease, fail, either partly or altogether, in its glucogenic, as is constantly the case with its biliary, function, the question may arise whether the saccharine matter—or, rather, the substance cir-

culating with the vital fluid, whence the glucose is derived, separated, or eliminated—may not be capable of inducing disorders other than diabetes."

The following definitions of two important terms we think will be well regarded:—

"910. By *infection* we understand a contaminated condition of the atmosphere, by pestiferous miasmatic emanations from the earth's surface, by poisonous gaseous exhalations, the product of putrefactive changes of organic or vegetable substances, or by both combined, capable of tainting, polluting, or corrupting the body.

"911. Infection is a local taint of atmosphere originating without the body.

"912. Disorders produced by malaria, as intermittent, remittent, and yellow fevers, are not communicable by the sick to the healthy.

"913. By *contagion* we understand the transmission of an infectious malady from the sick to the healthy by pollution of the atmosphere by the effluvia or emanations from or by the exuviæ of their bodies, or by means of *fomites* imbued with the poison; or by immediate or mediate contact. In the latter case the humidity of the atmosphere becomes a medium of contact.

"914. It seems to be a general law of animal nature, at least among the mammalia, that the accumulation and stagnation of the exhalations of the living body produce disease. The glanders of the horse arise only in stables where a large number of horses are stabled, and the distemper of dogs in kennels. During the American war it was proposed to send live sheep from England across the Atlantic. In a few weeks, in consequence of being crowded on shipboard, they all died of a febrile disorder.

"915. Contagion may therefore be designated a specific virus originating within the body.

"916. A contagious fever or disorder is produced by an animal poison, and not by malaria."

Of the laws of malaria, our author speaks thus:—

"932. We have already seen that a healthy adult respires 20 times in a minute (844), and takes into his lungs at each inspiration 20 cubic inches of air, or 576,000 cubic inches in 24 hours (852), and that this respired air comes in contact, at each inspiration, with 201,600 square inches of mucous surface of air passages and cells (682); is it therefore matter of surprise that atmospheric air, contaminated by infectious or contagious matter, or poisoned by malarious, miasmatic, or paludal emanations, should exert its baneful influence on the blood, and on the organic nervous system through the nerves distributed to the enormous superficies, with which it comes in contact at each inspiration? The wonder is that any of us escape. Indeed, where the percentage of contamination is large, few do escape. The history of the plagues and pestilences with which this and other countries have been visited during the last few centuries, fully bears out this position.

"933. Dr. W. Furgusson states that the African and the Creole are in a degree exempt from, and very rarely amenable to, those influences which generate in Europeans intermittent, remittent, or yellow fever.

"934. The precise nature of marsh effluvia has not yet been determined, though the most able analytical chemists and microscopists have devoted much time and attention to its investigation. Notwithstanding our ignorance of its chemical and physical properties, there is every reason to believe it to be an organic compound, composed chiefly of hydrogen and carbon, abundantly disengaged from the earth's surface by the solar rays, and diffused during the day through the atmosphere, and to be precipitated, after sunset, in a condensed form in proportion to the diminution of temperature."

"956. The specific gravity of malaria is considerably greater than that of the atmosphere."

"959. Malaria, in consequence of its great specific gravity, is found in its utmost intensity in valleys, intermingled with mists and fogs; on the earth's surface combined with the aqueous vapour of the atmosphere; entangled and stag-

nant in the coarse vegetation of the jungle of the East; in the dank grass, strong weeds, and dense brushwood of the West Indies; at the bottom of moats and ditches surrounding fortified towns; on the ground floors of our dwellings, and in underground apartments.

"960. In all malarious districts and seasons the inhabitants of ground floors are uniformly affected in a greater proportion than those of upper stories. According to official returns, the proportion of those attacked in the lower apartments of the barracks at Barbadoes exceeded that of the upper by *one-third*."

The subject of public hygiene, or State medicine, which is now engaging so much attention, receives powerful aid from our author, but we have room only for the following quotations on this subject:—

"1028. In all large cities and towns there are plague spots where fever of the intermittent, remittent, or continued form always prevails in greater or less intensity. There are districts or locations in our modern Babylon which are ever emitting the poison which generates typhus fever; there are certain squares and streets, nay, particular houses, the inmates of which, family after family, for a long series of years, have been the victims of typhus fever, though the districts in which they are situated are airy, and the soils dry."

"1030. There is probably no subject so complex, so incalculably difficult to grapple with, especially if it be how to apply a remedy, as the drainage and sewerage of large overgrown cities. Yet we must perceive that unless this be efficiently done, an ultimate limit is set by the hand of man himself to dynasties, to peoples, and to nations. The air we breathe, loaded with carbonaceous matter, sulphurous and sulphuric acid, sulphate of ammonia, and sulphuretted hydrogen, is deprived by the absence of vegetation of the revivifying principle, oxygen, and is hence less fitted for the necessary changes of the blood effected during respiration. The earth which we tread under our feet, loaded with the ashes of our forefathers, and rich with the remains of animal and vegetable matter of ages long gone by, saturated with the putrefying contents of myriads of cesspools and leaking sewers of our own day, emits at certain seasons of the year the poisonous emanations which generate typhus, diarrhœa, dysentery, and cholera; whilst the waters of our principal tidal rivers, converted into open common sewers, teem with pestiferous exhalations, charged with the germ of disease, or the messenger of death. If, under these favouring conditions, a pestilential epidemy invade our shores, it finds us an unprepared and easy prey.

"1031. The government of every State would do wisely to appoint a Minister of Public Health, whose duty it should be to superintend and watch over the public health of the community at large, to see that due ventilation is observed in all large and public buildings, and in the dwellings of the poor; to ascertain that the water is pure and its supply ample; to prevent all noxious and unwholesome trades and manufactures being carried on within a certain distance of towns or dwellings; to prohibit intramural burial grounds, slaughter houses, and slaughtering cellars; but, above all, to lay down and carry out an effectual, efficient, complete, and common sense plan of drainage and sewerage for every town and city."

"1055. The bed of the river Thames, at London, is estimated approximately at 2,245 acres; therefore $2.5 \times 43{,}560 = 108{,}900$ *cubic feet* and $\frac{108{,}900}{0.1605} = 678{,}505$ *gallons of water* evaporated in *one year* from *one* acre of water; which gives $108{,}900 \times 2{,}245 = 244{,}480{,}500$ *cubic feet* $= 1{,}523{,}242{,}991$ gallons evaporated *annually*, or 4,170,000 *gallons*, or about 18,000 *tons* of water raised *daily* from the surface of 2,245 acres of the polluted Thames at London, and diffused through the atmosphere of the city and neighbourhood.

"1056. During calm, clear weather, these emanations, consisting of sulphuretted and carburetted hydrogen gases, of nitrogen and carbonic acid gas, free ammonia, and other vapours, ascend high into the atmosphere, where they are generally diffused; but on their descent, on cooling, they spread broadcast the seeds of disease and death; but during cloudy, moist weather, and particularly during calms, and the colder air of night, they are condensed into haze, mist, or

fog, and occupy the lowest districts, the inhabitants of which are the earliest and most numerous victims."

"1060. It may be laid down, if not as a law, at least as a general rule, that when the temperature of the waters of a river in a state of putrefactive foulness exceeds 60° Fahr., the minimum temperature of the atmosphere being below this, diarrhœa and cholera will ensue, and will continue to increase in amount and severity in the direct ratio of the increased temperature; and that when the temperature of the water of the river falls below 60°, so will these disorders begin to decline, and will cease altogether."

"1075. From experiments instituted by Schönbein, it appears that atmospheric air, containing but $\frac{1}{3240000}$ of ozone is capable of disinfecting its own volume of air loaded with the miasmata given off in one minute by 4 oz. of flesh in a high state of putrefaction.

"1076. Atmospheric ozone in destroying oxidizable miasmata suffers destruction in its turn. This is one of the reasons why azone, though continually engendered, cannot accumulate in the atmosphere to an extent which would be prejudicial to animal life."

"1083. Mr. Condy, of Battersea, has recently introduced to the notice of the profession and the public *a disinfectant fluid*, of which the chief constituent is '*condensed oxygen*.' It is said not only to deodorize and disinfect perfectly, but also to destroy absolutely the cause of infection. It possesses a most important advantage over chlorine, that it is not poisonous, does not evolve any obnoxious or unpleasant smell, and may be employed to purify water. In short, it is a near approximation to ozone, and promises entirely to supersede chlorine as a disinfectant. It is favourably noticed by the Board of Health as "*a true disinfectant.*"

"1149. Dr. Roscoe has shown that the beneficial action of the *brick and mortar walls* of our dwellings is not confined to the mere absorbing from, or restoring moisture to, the atmosphere, but that it extends to a very large diffusive interchange between the carbonic acid gas of the apartments and the external atmosphere; that, in fact, brick walls are powerful aids to ventilation. Dr. Roscoe ascertained, that in a closed space, the air of which contained 16 per cent. of carbonic acid gas, 3.25 per cent. escaped through the solid brick.

"1150. The unhealthiness of iron, or new and damp houses, is probably partly accounted for by the absence of all diffusive interchange through iron and wet walls.

"1151. Newly built houses, and houses how long soever they may be built, though exposed for years to the action of dry air, are unhealthy when first inhabited. The lime of the dry hydrate of the mortar of their walls combines with the carbonic acid abundantly supplied by the lungs and skin of their first occupants, and parts with and sets free as moisture the 24 per cent. of water chemically combined with it. The water thus displaced speedily evaporates and saturates the atmosphere of the various rooms, its excess being condensed on the windows and cold walls.

"This does not depend on ordinary moisture or dampness of the walls, but on the retained water of the hydrate, and must invariably occur on the first occupancy of any building into the walls of which lime enters as a component."

"1154. In any and every case the ample and free admission of fresh air and the complete and ready escape of the contaminated atmosphere, are all that can be desired or attained. If we err, better that we err with too much than too little pure air. Air is not less the food of man than the daily bread on which he feeds. An impure or vitiated state of the atmosphere of his dwelling is infinitely more injurious to the general health and vigour of body than would be the most unwholesome and corrupt food and drinks."

The second work on our list: The report of Dr. Greenhow, is a valuable exposition of the ratio of deaths per 100,000 of the population, from certain causes which are believed to be either wholly or partially preventable; and the extraordinary differences which exist in this ratio, in different districts, afford an illustration against which there can scarcely be a cavil,

that various hygienic circumstances do exert potent influences in increasing and diminishing the amount of disease and death. This is, indeed, the idea which it has been his aim to establish, or rather it is the conclusion to which his researches decidedly point.

As a specimen of this, we quote from Dr. Simon's able introduction to the report:—

"In the subjoined figures you can read at a glance that vast range of their local death-rates which Dr. Greenhow has the merit of having made evident for public information.

"1. *Annual death-rates, by diseases which are either wholly, or almost wholly, preventable, under good sanitary arrangements, have ranged in different districts as follows:*—

Cholera.	Diarrhœa and dysentery.	Continued fever.	Smallpox.
From nothing to 403	From 4 to 345	From 21 to 209	From nothing to 146

"2. *Annual death-rates, by diseases which, to some considerable extent, are inevitable, but of which the severity or the frequency may be controlled by good sanitary arrangements, have ranged in different districts as follows:*—

Tubercular phthisis in women.	Non-tubercular lung diseases in men.	Common infectious disorders of childhood.	Convulsive disorders of childhood.	Pulmonary affections of childhood.
From 229 to 588	From 66 to 869	From 694 to 2149	From 280 to 3832	From 213 to 2897

From the first of these tables (we repeat the general statement that the reader may see the full force of the fact and the argument), we learn that in one of the registration districts of England, within a certain period, only four deaths in each 100,000 of the population occurred from *diarrhœa and dysentery*, while in the same period, in another district, there occurred the proportion of 345 deaths from the same diseases. In the case of *small-pox*, there were, in some districts *no deaths*, while in one there were 146, for each 100,000 of the population. With regard to this latter disease, we have not the slightest difficulty in arriving at a conclusion as to the reason for this wide difference between the two districts from which the figures are taken—that the total exemption in the one case was due to the care exercised in extending vaccination, and to the avoidance of contamination, and in the other to the neglect of one or both these precautions.

Take another example, and hear the explanation of Dr. Simon.

"Reverting once more to the gross mortality due among young children to the conjoint action of those three classes of disease which I have now separately spoken of" (*convulsive* or *nervous disorders*, *diarrhœa*, and *respiratory inflammations*, which destroy every year about 72,000 children, and occasion about a sixth part of the total mortality of England), "I believe that *the vast range of that aggregate mortality in different districts is due to the varying prevalence of two local causes:* First, to differences in degree in *common sanitary defects* of residence; some places abounding more than others in the foul air and foul

water of undrained, unpaved, unscavenged, unwashed, unlighted, unventilated localities and houses; and, secondly, to *occupational differences* among the inhabitants; there being certain large towns where women are greatly engaged in branches of industry away from homes; where, consequently, those homes are ill kept; where the children are little looked after; and where infants, who should be at the breast, are improperly fed or starved, or have their cries of hunger and distress quieted by those various fatal opiates which are in such request at the centres of our manufacturing industry."

With these brief examples of the striking manner in which this great subject is handled by the able author of the Introductory Report, we are prepared to look into the matter and manner of the paper of Dr. Greenhow, on which it is based. Of the circumstances which led to his investigation he speaks thus:—

"No systematic endeavour, of which I am cognizant, has ever been made to investigate critically the causes of death in unhealthy places, and to refer the diseases which swell the death rolls of such places each to its special cause. Even the kind of diseases which most prevail in unhealthy places is imperfectly understood." * * * "Just as the excessive mortality of unhealthy towns has, upon inadequate proof, been referred to a few of the more prominent evils consequent upon the aggregation of men into urban communities, so likewise have the diseases that occasion this mortality been hastily inferred from data equally conclusive. The incongruous class of diseases, to which the term zymotic has been applied, is commonly spoken of, even by medical sanitary authorities, in such terms as to convey the impression that it comprises the diseases that are essentially preventable. The term zymotic is, indeed, often employed almost, if not quite, in the sense of preventable. It cannot, indeed, be doubted that certain diseases comprised in the zymotic group are, like ague, produced by causes that are in their nature removable; that others, as cholera and fever, derive at least the conditions of their malignant development from the filthy accumulations common in the neglected portions of large towns; that a third class, which are propagated by contagion, find circumstances favourable for their propagation amidst the crowded population of cities; or, lastly, that contagious and epidemic diseases find a class of persons incapable of successfully battling with illness, amidst the unhealthy and vitally-depressed inhabitants of unhealthy places. But these opinions have never been brought to the test of a sufficiently extensive investigation of facts." * * * "We have thus still much to learn, both as to the nature of the particular diseases which produce the successive mortality of unhealthy places, and of the circumstances under which such diseases arise. * * * The importance of this question was forced upon my attention two years ago. The authorities of St. Thomas's Hospital, determined to found a lectureship on public health. The subject was one which had attracted much notice during several years, and had just at that period gained additional importance from the appointment of medical officers of health for the metropolitan districts, as well as for many provincial towns.

"The council of St. Thomas's did me the honour of appointing me to the office; and it was in the preparation of my first course of lectures that I first became fully aware of the vague and imperfect nature of the information upon which the sanitary agitation of the preceding twenty years had been based. * * * The broad and striking differences of death-loss in different places which had hitherto formed the staple topics of sanitary discussion, however valuable as a means of measuring the condition of the public health, afford no direct information as to the causes that modify it; * * * of *what use to point to the fact, that the inhabitants of large towns often die twice as fast as those of country places, unless the diseases of which they perish, and the causes of those diseases, could likewise be indicated?* and such information had never been procured."

England and Wales have been divided into six hundred and twenty-three districts for the registration of births, marriages, and deaths. Some of

these consist of towns only; others are of a rural character altogether; while in other cases, a town forms a centre, and a considerable portion of surrounding country is comprised in the district. Dr. Greenhow's investigations have extended to one hundred and five of these registration districts, and the time embraces the seven years from 1848 to 1854, inclusive. This period of time was chosen because it seemed to afford a sufficiently extensive basis to obviate the fluctuations that are liable to occur from year to year, and the census was taken in the middle year of the term, viz., 1851. The death-rates have been calculated in each district for 100,000 persons; for although it is true very few districts contain that number of each sex, and the entire population in most districts falls short of it, no real objection exists against that standard, provided only the correct proportions be allowed.

The several diseases which constitute the burden of his investigation are arranged in ten groups, namely: A. Pulmonary affections. B. Contagious diseases. C. Alvine flux. D. Typhus and erysipelas. E. Croup, influenza, and ague. F. Strumous diseases. G. Nervous diseases of children. H. Apoplexy and paralysis. I. Rheumatic fevers and rheumatism. K. Carbuncle and phlegmon.

The 105 districts included in the investigation were selected on account of each possessing some peculiar character, either of position, of salubrity, or of occupation, and are believed to afford a fair indication of the valuable results that may be expected to follow from a more extended and minute investigation.

We have not space to follow Dr. Greenhow to much extent in this article, and must content ourselves with extracting a few of the more prominent results obtained by him, recommending the whole paper to the careful study of medical men (if happily it should ever be within their reach), as a model for similar investigation elsewhere, and as exhibiting the great value of a complete system of registration.

Liverpool is ascertained to be the most unhealthy town in England, its annual average mortality being at the rate of 36 in the thousand; Glendale is one of the healthiest rural districts, the deaths being only 15 in the thousand. The same ratio holds with respect to deaths from pulmonary diseases, the deaths by this class being 216 per 100,000 in Glendale, and in Liverpool 1,000 per 100,000, so wide is the gulf between the salubrity of these two districts. These are the extremes of the districts in point of salubrity, while the remaining 103 districts occupy intermediate positions. "Glendale presents, perhaps, a standard of health impossible of attainment for the whole kingdom; Liverpool illustrates the great necessity of employing some energetic and well devised means for improving the public health."

The *industrial occupations* which have an influence upon the mortality of the several districts are classified by Dr. Greenhow as follows: 1. Agriculture. 2. Commerce and maritime pursuits. 3. Mining. 4. Manufacture of metals. 5. Manufacture of earthenware. 6. Manufacture of textile fabrics. 7. Manufacture of shoes.

Each of these classes receives special attention, and copious interesting tables give evidence of the faithfulness with which the investigation has been prosecuted, as well in relation to the influence of each upon the health of the two sexes, as upon the general population, giving abundant and unmistakable proofs of the immense losses suffered by both individuals and the State, by the neglect of sanitary measures, and furnishing distinct grounds for the continued study of public hygiene, and the enactment of

positive law for its practical application. We cannot better conclude this branch of our review than in the burning eloquence of the "conclusion" of Dr. Simon's Introduction:—

"These questions are not uninteresting to the rate payers of places where high death-rates prevail. For sanitary neglect is a mistaken parsimony. Fever and cholera are costly items to count against the cheapness of filthy residences and ditch-drawn drinking-water. Widowhood and orphanage make it expensive to sanction unventilated work-places, and needlessly fatal occupations. * * * *

"But if the subject may justly claim to be considered by the government and the legislature of this country, it is on higher grounds than those. The sacredness of human life against unjust aggression, is the principle above all others by which society subsists. To have realized this principle in law and government is the first indication of a social state; and in any community pretending to be civilized, the failure of protection for life has ever been felt as a public scandal.

"But growing knowledge must bear its fruit. It has now been fully recognized that with the very centres of civilization controllable influences are working against human life more cruelly than brute violence ever worked in the first discordant beginning of society. It has been shown that in certain districts of England the operation of those controllable causes is vastly more powerful than in others; that within the rule of certain sanitary authorities, particular forms of disease undergo a multiplication—a fivefold, and a tenfold, and hundredfold multiplication, of their lowest familiar fatality.

"*To suppose that such sanitary authorities could permanently disavow an interest in this knowledge, or that public opinion could long hold them irresponsible for so monstrous a waste of life, would be to misunderstand the meaning of civilization, or to belie the humanity of England.*"

In his Introduction to the work of Prof. Reid, the last which we have now to notice, Dr. Harris lays down a postulate thus:—

"The homes of the people, or the conditions of domiciliary life, furnish most reliable indices of the state of intellectual and moral advancement in any community."

And he adds, that

"Though it cannot be assumed that the improvement of man's physical condition will alone secure his moral elevation, it is unquestionably true that it contributes essentially to that important end, and that neither intellectual progress, nor moral and social refinement, can be long maintained where the requisite conditions for physical health and comfort are not suitably provided."

If this be true—and who will doubt it?—the subject of public hygiene rises in dignity and importance, in connection with the welfare and progress of any people to a level with those of education, and the teachings of the pulpit.

The history of the progress of sanitary improvement given by Dr. Harris, of which we have quoted above the opening paragraph, is a clear exposition of the several great steps of advancement which have been made in the practice of hygiene from the time of Pericles to the present day. Did space permit, we would gladly cite further from its pages, especially some of the points in reference to the public labours of Dr. Reid, who, from the date of his appointment by the Queen in 1843, as one of the first "*Commissioners for Inquiring into the State of large Towns, and populous Districts,*" has been a zealous and intelligent labourer in the work of sanitary reform. His name, is, however, too closely identified with this and other questions of scientific character, to render any further introduction of him necessary to the medical public. It suffices to say that his profound scientific acquirements, and his great practical ability, rendered him essential to that and

other subsequent great works, and have established his name as one of authority in all matters upon which he may speak or write.

The volume before us, without any special pretensions to anything new, is written chiefly for the instruction of *the people at large* in the theory and practice of domiciliary ventilation. In its 34 chapters we believe will be found suggestions for the simplest as well as the most elaborate methods of supplying buildings fully with fresh, and the removal of vitiated air. To those readers who might perchance perceive any difficulty in comprehending a dry text, its hundred coloured illustrations will make everything plain. The plan of distinguishing the foul air and the fresh, in an apartment, by pink and blue tints, is a happy suggestion, and was first employed, if we mistake not, by Dr. Reid in some of his Parliamentary Reports.

Respecting an instance of foul air poisoning, which has recently excited a world-wide interest, Dr. Reid's testimony will be regarded as *ex cathedra*.

"The noted case of the National Hotel, at Washington, where so many hundreds suffered very lately, was not unconnected with the condition of the ventilation. Whether other causes contributed or not, is a question that is not entered on here; recent facts and statements that have been made on this point may leave this an open question, till the whole of the evidence on the subject shall be published and compared; but in the meantime personal observations, at this hotel, at the time referred to, gave proof that there was, in one part of the hotel at least, a discharge of vitiated air from drains of so intense a character that it produced instantaneous vomiting on some occasions, and affected numbers in a less degree at the moment, who were nevertheless attacked at a subsequent period." * * * "No other cause has yet been proved to have been in operation, and even if it were, it would in no way alter the conviction entertained, that the emanations from the drains constituted an evil of great magnitude, and capable of producing the most disastrous results. Let it be recollected that there are no deleterious gases that can arise from the admixture of chemicals that may meet in obstructed drains and sewers, that may not find their way into hotels, houses, and other buildings, as well as the products of the putrefaction of animal and vegetable matters. Sewers may discharge there the products found at the distance of miles, particularly if they be trapped so as to exclude the access of air in the streets. And who can estimate the emanations that may not proceed from such sources, when they arise from chemicals discharged from a manufactory, an apothecary store, a paint shop, a telegraph office, or the poisoned remains of animals that may have accumulated in the sewers? Further, the very cement or mortar may imbibe materials that discharge sulphuretted or arseniuretted hydrogen from compound mixtures on fermentation, or from the action of an acid; and these find their way by a retrograde current in the drains and sewers to any building connected with them where the drains have been injured, and the traps rendered ineffective."

Our own observations at the National Hotel, during the last session of the American Medical Association last May, justifies all that we have ever heard of the condition of its atmosphere. Some parts of the building have never been reached by the sun's rays, and have a graveyard-like dampness, while the low ceilings, and the dark, tortuous, and unventilated passages and rooms, give forth a musty odour. To our mind it scarcely required even the addition of the foul air of an obstructed sewer to produce the sad effects attributed to it, much less the drinking of water poisoned by arsenical rats.

> "The Tenement House, o'er which no friendly movement
> Has waved the enchanter's wand of 'modern improvement,'"

has become one of the *Institutions*, especially of the city of New York, and is thus hygienically painted to the death, by our author, p. 99.

"The staircase has no supply of air whatever, when the doors below are shut, except the small amount that may enter by leakage there; but sixteen doors on the different floors, as well as the basement, may all draw more or less upon the passage and stairs when there are many fires kindled, and no windows opened for a supply, in consequence of the state of the weather. The result is that one or more doors are opened on every floor to derive some benefit from the atmosphere in the staircase, and the lower floors discharge their vitiated air, which ascends and oppresses the occupants of the upper floors. Some of the chimneys overpower others, drawing down smoke which pervades the staircase. The occupants are forced in self-defence to shut their doors and open their windows, till the cure becomes worse than the remedy, when they again return to their previous position, alternating between these opposite evils so long as they do not choose to suffer quietly the inconveniences that result from their unventilated dwellings."

The first remedy for a great portion of these evils, our author suggests, is the very sensible one of supplying the entry and stairway with a current of fresh air, warmed in winter by a stove in the lower hall, by which simple measure the comfort and health of the occupants would be increased more than by any other that could be mentioned.

With plain and practical suggestions like this, adapted to almost every kind of inhabited building, this volume abounds. The proper construction and arrangement of drains and sewers, and of water-closets, the methods of avoiding the effects of noxious gases from manufactories, "*external ventilation*," including the rules which should govern in the choice of a site for a dwelling, these and many other cognate matters are well and clearly discussed, and cannot fail to impart instruction to all who desire it.

J. H. G.

ART. XIII.—*Leçons sur la Physiologie et la Pathologie du Système Nerveux.* Par M. CLAUDE BERNARD, Membre de l'Institut, Professeur de Médecine au College de France, &c. &c. Paris, 1858. 2 vols. 8vo. pp. 560—520.

THESE volumes give an account of M. Bernard's last two courses in the College of France on the Physiology and Pathology of the Nervous System; one delivered in the winter term of 1856–57, the succeeding one in the spring term of 1857. The subject is so divided that the first volume treats of the general relations of the nerves and the nervous centres, their influence upon nutrition, calorification, and the like, and the manner in which they are affected by galvanism and other external agents; while the second volume is devoted to the study of individual nerves and the special functions of their different branches.

The author commences, as is almost habitual with him, by defining his position in the opening scientific campaign, and by reminding his hearers that "there are, in the development of the sciences, two distinct departments to be cultivated, viz: the discovery of new truths, and the criticism or co-ordination of those which have already been acquired. Among the cultivators of natural science, the one class strive to extend its limits by the introduction of new ideas; while those belonging to the other are more particularly employed in criticizing the discoveries of the first, and bringing them into relation with established opinions."

Bernard has no hesitation in making the avowal that his own position, as conductor of the physiological course at the College of France, places him in the former category. There is no doubt, beside, that his own preference and intellectual tendencies lie in the same direction. It is curious to observe, indeed, in the physiological discussions which have arisen in France within the last ten or fifteen years, that it is almost always his name which appears at the head of new discoveries and innovations ; while his statements and doctrines immediately call up a host of critics and investigators to assail or corroborate them, and furnish abundant material for discussion, examination and counter-experiment, almost without end. Striking examples of this are to be seen in the history of the discovery of the formation of liver-sugar ; of the influence of the sympathetic nerve on calorification ; of the artificial production of diabetes by wounding the fourth ventricle ; and of the digestive properties of the pancreatic juice.

The discovery of the formation of liver-sugar alone, ever since it was first announced, in 1848, to the present time, has given rise to almost incessant discussions, which even now are hardly to be considered as terminated ; the most important part of the whole doctrine, viz., the exclusively internal origin of the sugar found in the liver, having been formally assailed no longer ago than May, 1857, in a very able and elaborate communication addressed to the French Academy by M. Sanson, of Toulouse. During this period, the objections raised against Bernard's doctrine have been twice made the subject of special investigation by commissions of the French Academy, and although in each instance the objectors (including M. Sanson) failed to make good their statements before the Commissioners of the Academy, and though the reports accordingly sustained Bernard's doctrine so far as the special points under discussion were concerned, still the question is not yet regarded as altogether settled, nor is there any complete unanimity of opinion in respect to its details. So wide is the field of research which has been opened by one discovery, and so abundant the material for investigation which it has afforded.

The great advantage resulting from this system of encroachment and innovation is that, in whatever way the special questions raised by discoverers may be finally answered, the acquisitions of science cannot fail to be largely increased, and its domain enriched by their agitation. Even if it should hereafter be shown, for example, that Bernard was mistaken in supposing his liver-sugar to be actually and exclusively produced in the substance of the hepatic tissue, how many new facts, brought out in connection with this doctrine and in consequence of its discussion, would remain as permanent scientific acquisitions. The constant presence of sugar in the liver during health, and its passage thence into the hepatic blood ; the existence of a "glycogenic matter," similar or analogous to starch and dextrine, and its ready conversion into sugar by animal ferments ; the occasional passage of sugar into the circulating fluids throughout the body, and its normal existence in the blood and urine during a part of fœtal life ; these are all facts of the greatest interest for the physiologist, and which certainly would not have been discovered except for the attention attracted to the entire subject by Bernard's doctrine. Such acquisitions are necessarily evolved from the intellectual fermentation, which is excited in the scientific world by the contact of new ideas and the stimulus of unexpected discovery.

Bernard is quite right, therefore, when he maintains that the path of experimental inquiry, if followed in the right spirit, *must* conduct us to more advanced posts of knowledge, and result necessarily in enlarging our scien-

tific acquirements. So long as we interrogate nature for the information we wish to acquire, and rely solely upon her answers to guide our studies, our labour cannot be lost. For, though we may afterwards be compelled to modify the conclusions drawn from an experiment, the actual results of the experiment itself cannot be modified, but remain good forever. It will always be noticed, in fact, even when we seem to have been deceived in our investigations, that it is only our *interpretation* of the experimental phenomena which was erroneous. The phenomena themselves, if faithfully observed, are never erroneous; and though our interpretation of them may afterwards be altered, the results of the experiment itself must always remain the same, and forever bear an undiminished value.

The principal danger of misinterpreting the results of experiment lies, as every one knows, in a too great devotion to preconceived ideas or favourite theories. This devotion is often an unconscious one on the part of the experimenter; and it is often to be remarked that an author will disclaim for himself any preference whatever as to which side of the question under investigation may prove the correct one, and will profess the most impartial desire to arrive at the truth, while his expressions and his mode of treating the subject indicate plainly how much he is interested in establishing a favourite conclusion. The cause of this mental bias is generally to be found in the fact that the experimenter fears lest an adverse conclusion should destroy the value of previous investigations; and he is naturally unwilling that science should lose the advantage of so much well meant labour, and that its acquisitions, once regarded as fully established, should afterward turn out to be fruitless.

Such fears, however, as we have remarked above, are always unfounded. The acquisitions of science can never be fruitless, whatever be the result of subsequent investigations. Every one who pursues for a series of years any department of investigation, finds that his ideas and theories undergo successive changes with the lapse of time. He views the subject in a different light, and arranges the facts presented to him by nature in a different order and relation. But in substituting new theories for the old, he never loses anything previously acquired; on the contrary, the new view which he is led to adopt, concerning any set of phenomena, is always more comprehensive and satisfactory than the previous one; for it includes the new facts discovered by experiment, as well as those which had been established before.

It is possible, however, for this truth to be so entirely ignored, and the object and purpose of experimentation so thoroughly misconceived, that investigation may be resorted to only as a secondary matter, for the purpose of confirming, or "illustrating," as it is called, scientific doctrines. This was probably the case with the French *savant*, mentioned by Bernard as a "prominent member" of the Academy of Sciences, though "not particularly eminent," he adds, "in experimental physiology." The academician had been engaged in preparing a memoir upon an interesting physiological subject, which he intended to present to his colleagues at their next session. In conversing upon the subject with Magendie, he remarked that he "should very soon be ready to present his paper; for the memoir itself was already finished, and he had *nothing more to do but to perform the experiments*."

Bernard gives, in his first chapter, some very judicious directions with regard to what he calls the "art of experimentation;" that is, the manner in which experiments should be arranged and their results compared, the

principles upon which they should be contrived, and the care which is requisite in fixing and regulating their conditions. We shall leave this part of the subject, however, and pass immediately to the consideration of a few of the most important topics treated of in the body of the work.

A very prominent place is given, in the early part of the memoir, to the study of the *recurrent sensibility* of the spinal and cranial nerves. As the history of this subject is somewhat remarkable in many respects, and as the author regards the property of recurrent sensibility as affording very important indications in regard to the classification of nerves, it may be considered as one of the most interesting topics treated in the present volumes.

It is well known, that of the two roots by which each spinal nerve takes its origin from the spinal cord, one is motor and the other sensitive in its nature and properties. The anterior root is especially devoted to the function of exciting contraction in the parts to which the nerve is distributed, and has no relation with the sensibility of these parts; while the posterior root is incapable of exciting motion, by any direct influence, and has, instead, the power of conveying sensation from the parts in relation with its terminal branches.

If these roots, accordingly, be separately divided, the function of sensation or of motion will be abolished, according as the injury has been inflicted on the fibres of the first or the second set. If the anterior root be divided, the corresponding part of the body is paralyzed of motion, but retains its sensibility; if the section be made of the posterior root alone, the sensibility is lost, but the power of motion remains unaffected.

The properties exhibited by the nervous roots themselves, when directly experimented on, correspond with the above functions intrusted to them. If the anterior root alone be irritated, by pricking or by the galvanic current, a convulsive movement ensues; if the posterior root be similarly irritated, a painful sensation is produced, but no movements follow, excepting those of a reflex nature.

Furthermore, if the anterior root be divided, an irritation applied to its *detached* extremity (or that which is separated from the spinal cord) produces instant convulsions, but if applied to its *attached* extremity (or that remaining in connection with the spinal cord), no result follows; since the nerve conveys the motor influence by a direct course to the muscle, and must, therefore, in order to produce any effect, be still connected with it by continuity of fibre. If the posterior root be divided, however, an irritation applied to its *detached* extremity is without effect; but, applied to its *attached* portion, is instantly followed by a painful sensation.

These are the properties of the anterior and posterior nervous roots, demonstrable by direct experiment, which first attracted the attention of investigators, and led them to discover the characteristic functions of the two roots, now so well established. Another property, however, was early found to reside in the nervous system, which interfered somewhat with the completeness of these results, at least so far as regards the posterior roots, viz: the *reflex action* of the spinal cord. For, though irritation of the posterior roots has no direct influence on the muscles, yet, in point of fact, if these roots be irritated, in the living or recently-killed animal, muscular contraction is frequently excited in consequence. This muscular contraction, however, following irritation of the posterior roots, is shown to be indirect or "reflex" in its nature, by the two following facts. First, if the posterior root be previously divided, no muscular contraction is ever pro-

duced by irritation of its *detached* portion, or that still connected with the muscle, but only when the irritation is applied to its *attached* extremity. The motor influence, accordingly, such as it is, is not conveyed in this case directly from the nerve to the muscle, but reaches the latter only by first passing through the spinal cord. Secondly, if, after performing the above experiment, the anterior root be divided, the posterior will then be found to have lost all power to excite contraction, which ever part of it be subjected to irritation. It is only, therefore, through the medium of the anterior roots that the posterior may sometimes give rise, when irritated, to movements of a reflex nature.

A corresponding class of phenomena, connected with the anterior roots, constitutes the property known as the *recurrent sensibility of the spinal nerves.* It has been found that the anterior roots, when irritated in the living animal, sometimes give evident indications of sensibility. This sensibility is much less acute than that of the posterior roots, but it is still asserted, by various observers, to be occasionally so well marked as to leave no doubt of its existence. Like the reflex motor action of the posterior roots, however, this sensibility is not direct, but indirect, in the manner of its production. For, if the anterior root be first divided, it is the *detached* extremity which remains sensitive, the attached portion being then completely insensible. Furthermore, if the posterior root be divided, the anterior is found to have lost the sensibility which it previously manifested.

The sensibility of the anterior root, therefore, depends upon the existence of sensitive fibres, coming from the posterior root, which join the nerve at the junction of the two roots, and then retrace their course, along the anterior root, toward the spinal cord, in a reversed or "recurrent," direction. These fibres have never been traced by the scalpel, and probably can never be followed out from their commencement to their termination, owing to the mechanical difficulties in the way of such a demonstration ; but the above phenomena would seem to leave no doubt as to their actual existence.

The occasional sensibility of the anterior roots of the spinal nerves was noticed by Magendie as early as 1822 ; though it does not appear that the indirect or recurrent nature of this sensibility was recognized until the year 1839. At that time, both Magendie and Longet found that the sensibility of the anterior roots, when it existed, could be made to disappear by dividing the posterior roots, and they therefore regarded it as an indirect sensibility, derived from the fibres of the latter. Longet, indeed, claimed this discovery as his own, and maintained that it was he who, after observing the phenomenon, communicated it to Magendie.

Both observers, however, subsequently failed to reproduce the experiments with their previous results. The anterior roots, when exposed in the living animal, always, subsequently to the experiments of 1839, appeared completely insensible; and this was the case in so many repetitions, that the existence of a recurrent sensibility was practically given up by nearly all physiologists, and particularly by Longet himself. In his *Treatise on Physiology*, published in 1850,[1] Longet repeatedly, and in the most explicit manner, declares the anterior roots of the spinal nerves to be "*completely insensible* to mechanical irritations of every kind ;" and speaks of his earlier experiments, performed in 1839, and which gave a different result, as wanting in confirmation, and insufficient in their conclusions.

Bernard, however, who was also present at the experiments of 1839,

[1] Traité de Physiologie, par F. A. Longet, Paris. 1850.

could not persuade himself to abandon the facts observed, whatever difficulties might exist in the way of adopting the conclusions which had been derived from them. As he himself expresses it, he "had been a witness of these facts; he had seen them and touched them; and, notwithstanding that, in the latter experiments of Magendie, the recurrent sensibility of the anterior roots was no longer to be found, he could not on that account ignore the existence of what he had seen before."

The manner in which Bernard was again led to discover the recurrent sensibility, illustrates very forcibly the care which it is necessary to take in regulating the conditions of a physiological experiment, if we wish to obtain from it uniform results.

"I recollected," he says, "that in 1839, at which time I attended the course of Magendie, that all the preparations for the experiment were made in the mornings, and that it was only afterward, during the demonstrations in the amphitheatre, and when the animal had been allowed to repose, that the experiment was tried, and the anterior roots gave signs of sensibility. In the subsequent trials, however, the anterior roots were subjected to irritation immediately after having been exposed to view, and they were then found to be insensible. I had even supposed this promptitude in conducting the experiment to be a condition favourable to success. But after further reflection upon the condition of the animals in which I had before seen the recurrent sensibility to exist, the opposite condition appeared to be the reason of the phenomena which were observed; for, in those instances, the animal had been left for several hours in repose after the preparations for the experiment were made, and this, by allowing him to recover from the fatigue of the operation, must have brought back the nervous phenomena, more or less completely, to their normal condition."

From 1841 to 1846, accordingly, all attempts to demonstrate anew the existence of recurrent sensibility were without success. But afterward, by modifying and varying the conditions of the experiment, Bernard obtained a different result. He found, as above intimated, that a certain period of repose was necessary to enable the animal to recover from the immediate effects of the severe operation of opening the spinal canal, the loss of blood, the cooling of the spinal cord from contact with the atmosphere, &c. &c. He found, in certain instances, that where the operation had been long and difficult, and had been accompanied by a considerable hemorrhage, not only were the anterior spinal roots immediately afterward insensible to irritation, but the facial nerve, which every one knows to possess a certain degree of sensibility, was also insensible at the same time, owing to the partially exhausted and inexcitable condition of the entire nervous system. It was also found necessary to make only a small opening in the spinal canal, just sufficient to expose the roots of the nerves, and not large enough to uncover the entire thickness of the cord itself. All these conditions, however, being fulfilled, a vigorous and well-fed animal being selected for the experiment, a small opening being made in the spinal canal, by a rapid operation, without much hemorrhage, and the animal then allowed to remain at rest for an hour or two, the anterior roots nearly always showed themselves sensitive to mechanical irritation. In this way, Bernard has again demonstrated, since 1846, the recurrent sensibility of the anterior roots, which, for a certain interval, had been a lost fact to science.

It is worthy of remark, however, that Longet still adheres to his belief in the non-existence of what he calls the "pretended recurrent sensibility" of the anterior spinal roots. In the second volume of his Treatise on Physiology, above alluded to (Part II. p. 274), he speaks of this subject as follows:—

"If the recurrent sensibility of the anterior spinal roots should be regarded as real, I might still insist, as in 1839, upon my right to its discovery; since I was the first who, at that period, directed the attention of physiologists to the extinction of this sensibility which follows division of the corresponding posterior root. But since then, in many hundreds of experiments, *whether I removed one, two, or several of the vertebral arches*, or *whether I allowed my animals to repose or not, after the preliminary operation,* I never observed, in any instance, the pretended recurrent sensibility of the anterior roots; and I have, therefore, been led to abandon my old opinion, which Magendie has recently (1847) again brought forward as the expression of the truth. I nevertheless continue to place full confidence in the later experiments by which I demonstrated the constant and absolute insensibility of the anterior spinal roots and the anterior columns of the spinal cord."

Longet even intimates (page 10 of same volume) that the experimenters who differ with him on this point may have been misled by a faulty method of operating.

"I said," he remarks, "that I had constantly found the anterior roots, when in a condition of integrity, to be *completely insensible* to mechanical irritations of every kind; and that, on the other hand, the posterior roots had always shown themselves extremely sensitive. But I have sometimes found in the dog, as well as in the human subject, three distinct branches of origin for one lumbar or sacral nerve, running parallel with each other in the spinal canal; two of them belonging to the posterior root, and the third being the anterior root. This seems an important fact to remember, since, when meaning to seize with the forceps the anterior root, one might lay hold of that division of the posterior root which lies in front of the other, in which case the signs of a very acute sensibility would not fail to be manifested. It is essential, therefore, to guard against this source of error, which has not heretofore been noticed, and which is undoubtedly the reason why the anterior roots have been thought to exhibit sensibility."

Bernard, nevertheless, actually exhibited with success the presence of recurrent sensibility in the anterior spinal root, if we are to judge from the context (Vol. I., Lecture 4th, p. 73); and he gives also the following rules, which should be observed in order to insure success in the experiment.

I. The animals selected for operation should be young, vigorous, and well fed.

II. When the animal has not been exhausted or enfeebled by the operation, recurrent sensibility may be exhibited by the anterior roots immediately afterward.

III. But if the animal have become exhausted during the operation, which is most frequently the case, the anterior roots, examined immediately afterward, may appear completely insensible. Then, if the wound be closed by sutures, and the spinal cord covered with integument, by waiting a certain time for the animal to recover his strength, the recurrent sensibility becomes re-established.

IV. The preferable mode of operating consists in opening only one lateral half of the spinal column, in such a manner as to expose one or two nervous roots as far as the ganglion. In this way, the spinal cord is less liable to be affected by cooling; and

V. The anterior roots selected for experiment should be those of the largest size.

Such is the history of opinion on this singular property of the anterior spinal roots. In the opinion of Bernard, it is of great importance, as completing the physiological union between the two roots, motor and sensitive, of the spinal nerves. These roots have opposite properties and functions,

but they are still intimately associated, and are necessary to each other's perfect functional activity. The sensitive parts cannot give us complete information with regard to any foreign body, unless they are properly applied and adapted to it by the aid of the muscles; and the muscles cannot contract with certainty and efficiency, unless sensibility is also present, to enable us to appreciate the direction and force of their movements. As the posterior roots, furthermore, are connected with the anterior by the property of reflex action, so the anterior are connected with the posterior by their recurrent sensibility.

Bernard even assumes—somewhat arbitrarily, as we think—that the existence and source of recurrent sensibility in a motor nerve indicate the sensitive roots with which that nerve is associated to form a complete nervous pair; that is, if there be any doubt to which pair of nerves a motor branch belongs, he ascertains from which of the posterior roots it derives its recurrent sensibility, and then regards it as anatomically a branch of that nerve alone. This is undoubtedly allowable in the case of the spinal nerves, where the anterior and posterior roots are all arranged in regular pairs; but its application as a constant rule to the cranial nerves, where there are great variations in the anatomical arrangement, seems of doubtful propriety.

The author treats at considerable length (Chapters VIII., IX., and X.) of the effects produced on the nerves by the stimulus of galvano-electricity; the special influence of the direct and inverse currents, and of the continuous and interrupted currents; and of the differences in reaction, under the electrical stimulus, of motor, sensitive, and mixed nervous filaments. All these details, however, though useful for the experimenter who devotes himself to that particular field, possess but little general interest, and really throw little or no light on the true physiological properties of the nervous fibre. They provide us with the means of more accurate investigation, but are not in themselves particularly valuable or interesting, as a separate class of phenomena.

In the chapter devoted to the properties and function of the spinal cord, Bernard sustains the doctrines promulgated of late years by Brown-Séquard, as to the transmission of sensitive impressions in a crossed direction, the production of hyperæsthesia by section of a lateral half of the cord, and the part taken by the gray matter as a conductor of sensibility. The anterior as well as the posterior columns are sensitive to mechanical irritation, provided the same precautions be observed which are necessary in demonstrating the recurrent sensibility of the anterior roots. The sensibility of the anterior columns is, in fact, a recurrent sensibility, and can be made to disappear, over a limited space, by dividing either the anterior root originating from the cord at that part, or the corresponding posterior root. Section of a lateral half of the cord is followed by an exaggeration of sensibility in the corresponding side of the body below the point of section, and a diminution of sensibility on the opposite side. Finally, after section of the right lateral half of the spinal cord, if the anterior, lateral, and posterior columns of white substance on the left side be also divided, leaving only the gray substance entire, sensibility still persists in the right side of the body below the point of section.

Sensitive impressions are therefore transmitted, in the spinal cord, through the medium of the gray matter, and in a crossed direction; those coming from the right side of the body passing by the left half of the cord, and those coming from the left side of the body by the right half of the cord.

The exaggeration of sensibility after division of a lateral half of the cord,

though a phenomenon very difficult of explanation, seems at the present day to be perfectly well established. It undoubtedly accounts for another singular fact, frequently noticed by experimenters, viz., that the phenomena of reflex action of the spinal cord are usually much more energetic after the decapitation of the animal than any movements of a similar nature which we know to take place during life. This increased activity is probably due to an exaggeration of sensibility consequent upon section of the cerebro-spinal axis; only in this case the sensibility is an unconscious one, and produces no other movements than those of a reflex character.

In the second volume the author takes up in succession the different cranial nerves, and examines their properties, their functions, the effect of their division in various parts of their course, the nature and destination of their various branches, &c. &c. The peculiar alteration and destruction of the eyeball, after section of the fifth pair within the cranium, is dwelt upon at length, and various instances are related of an alteration of the sense of taste, dependent on paralysis of the facial nerve in the human subject. Bernard has also demonstrated a similar alteration of the taste in the dog, after division of the facial nerve within the skull. The effects of division of the pneumogastric, upon the pharynx, larynx, œsophagus, lungs, heart, stomach, &c., are also described in detail.

The chapter devoted to the discussion of the *spinal accessory* is principally a reproduction of the elaborate monograph upon this nerve published by the author in 1851. Some years before, Bischoff had published a memoir, in which he maintained the opinion that the spinal accessory was to be regarded as bearing the same relation to the pneumogastric, that the anterior or motor root of a spinal nerve bears to its posterior or sensitive root; in other words, that the spinal accessory and pneumogastric are associated with each other as motor and sensitive roots, forming by their inosculation a complete physiological pair, like each one of the spinal nerves.

This was soon after the discovery of the distinctive properties of the anterior and posterior spinal roots had been followed by the brilliant generalizations of the transcendental anatomists, by which the skull was shown to be in reality a continuation of the spinal column, and to be composed of a series of cranial vertebræ, entirely analogous in their connection and relations with the vertebræ of the spinal column.

It was easy to see, also, that the nervous centres, and the nerves originating from them, presented, to a great extent, similar analogies in their different parts; and the cranial nerves were therefore examined, to ascertain how far they could be classified according to the principle which regulated the arrangement of the rest of the cerebro-spinal system. The three nerves of special sense—the olfactory, optic, and auditory—evidently presented no analogy with the spinal nerves proper, either in their functions or their anatomical arrangement. With regard to the others, however, a very striking analogy could easily be seen to exist. The Fifth pair, more particularly—having a small and a large root, shown to be respectively motor and sensitive in their nature, the large root provided with a ganglion (Casserian), and the two afterwards mingling their fibres to form a mixed nerve (inferior maxillary branch)—was evidently to be regarded as constructed upon the same plan as a spinal nerve, presenting only those modifications of detail necessarily connected with the peculiarities in the form of the skull.

It required only a further development of the same ideas, to classify most of the other motor and sensitive cranial nerves—such as the oculo-motorius, facial, glosso-pharyngeal, and sublingual—in a similar manner; and it was

the object of Bischoff to complete the above systematic arrangement by including the spinal accessory with the pneumogastric, as forming together the last pair of the true cranial nerves. He sustained this view by declaring the pneumogastric to be exclusively sensitive at its origin, and the spinal accessory to be purely motor. The ganglion of the pneumogastric, situated in the jugular fossa, he compared with the ganglia of the posterior roots of the spinal nerves; while the internal branch of the spinal accessory, joining the pneumogastric below its ganglion, completed, according to his view, the anatomical union of the two roots, making the trunk of the pneumogastric a mixed nerve, containing both motor and sensitive fibres, like the spinal nerves outside the vertebral canal.

The view thus brought forward by Bischoff was received with great favour by anatomists and physiologists. The analogies upon which it was based were, indeed, in many respects so striking, that they could be regarded in no other light than as the expression of actual anatomical relations existing between the two nerves. By following out this idea all the cranial nerves, with the exception of the three nerves of special sense, may be arranged in pairs, like those of the spinal nerves, according to the distribution of their motor and sensitive filaments. All the branches of the fifth pair emanating from the Casserian ganglion supply the region of the face with general sensibility; while the smaller root of the same nerve, together with the facial and the motor nerves of the eyeball, supply the same parts with the power of motion. The sublingual and the glosso-pharyngeal supply respectively the muscles and mucous membrane of the tongue; while the pneumogastric and spinal accessory perform the same office for the various parts to which these nerves are distributed, viz., the muscular and mucous coats of the respiratory passages and upper part of the alimentary canal. The fact that the oculo-motorius, patheticus, external motor, facial and masticatory nerves, pass out from the skull in separate bundles, cannot of itself prevent our regarding them as physiologically the same nerve; since the physiological identity of any motor branches does not depend so much upon their following the same course, as upon their being finally distributed to the same regions. Even the anterior roots of the spinal nerves originate from the spinal cord by many distinct filaments, and are only collected into a single trunk at a certain distance from their point of origin.

The cranial nerves, then, which are similar in their nature to the spinal nerves, may be arranged, if we adopt this view, in three distinct pairs, each consisting of a motor and a sensitive element, as follows:—

	Motor Portion.	Sensitive Portion.	Distributed to
1st Pair	Oculo-motorius. Patheticus. Motor Externus. Facial. Masticatory.	Sensitive branches of "5th Pair."	Face.
2d Pair	Sublingual.	Glosso-pharyngeal.	Tongue.
3d Pair	Spinal Accessory.	Pneumogastric.	Pharynx, larynx, trachea, œsophagus, lungs, and stomach.

The doctrine which regards the spinal accessory as the motor root of the pneumogastric nerve receives a striking confirmation from the fact that if the spinal accessory be divided at its origin, the laryngeal muscles, supplied by a branch of the pneumogastric, are thereby paralyzed to such an extent that the animal loses the power of producing a vocal sound. This singular fact, viz., the loss of voice after section of the two spinal accessories

before their junction with the pneumogastrics, was verified by Bischoff in one successful operation, after several failures; and shows conclusively that some, at least, of the motor filaments of the pneumogastric are derived exclusively from the spinal accessory.

Bernard confirms entirely the above fact from the result of his own experiments. Indeed, his success in performing the operation without inflicting serious injury upon the animal, has been so much greater than Bischoff's, that he seems to have at least an equal claim with the German experimenter, to the establishment of the discovery. The earlier operations consisted in cutting down upon the occipito-atlantoidean membrane, dividing it by a transverse incision, and then cutting off the spinal accessory nerves upon each side through this opening. The external wound, however, in this operation was very large and deep, and in order to reach the superior fibres of the spinal accessory it was usually necessary to cut away a part of the occipital bone; a proceeding almost necessarily followed by troublesome and exhausting hemorrhage from the occipital sinuses. The introduction of air into the veins, owing to the opening of the osseous sinuses, was also found to be a frequent cause of death, during the operation, and consequent failure of the experiment.

Bernard, however, contrived an extremely ingenious operation, by which all these difficulties were remedied. It consists in cutting down upon the spinal accessory just as it emerges from the jugular foramen, seizing at the same time both the external and internal branches between the blades of a forceps, and by a steady, continuous traction tearing away the nerve from its roots. The only difficulty in performing this operation consists in the depth of the wound, at the bottom of which the nerve is to be seized and extracted; owing to which the operator may sometimes fail to grasp the internal or anastomotic branch of the spinal accessory, and lay hold only of the external division of the nerve, which is distributed to the muscles of the neck. When both branches, however, are grasped by the forceps and the traction properly made, the roots of the nerve give way at their origins, even down to the lower part of the cervical region of the cord, and the whole nerve comes away as a slender white filament, an inch and a half to two inches long, terminating in a fine, threadlike extremity. The operation is more readily performed on cats than on dogs, owing to the greater density of the fibrous tissues in the latter, in consequence of which the nerve is apt to be broken, in the effort to bring it away at its roots.

This operation, when successful, leaves the animal with a simple and not very extensive wound of the integument and muscles, which involves no injury to any important organ, and which readily heals, leaving the animal in a favourable condition for exhibiting the effects of division of the nerve, uncomplicated by any other affection or mutilation.

We have already mentioned that the principal effect of this operation is a complete and permanent loss of voice, and that it shows conclusively that the internal or anastomotic branch of the spinal accessory supplies to the pneumogastric, and through it to the larynx, all the motor fibres concerned in the production of vocal sounds.

Bernard, however, is nevertheless unwilling to regard the spinal accessory as representing the motor root of a mixed nerve, of which the sensitive root would be the fibres of origin of the pneumogastric. His objections to this view are both anatomical and physiological in their nature. The peculiarities of the anterior roots of the spinal nerves, with which Bischoff would compare the spinal accessory, are the following:—

1. They originate from the antero-lateral columns of the spinal cord.
2. They join the posterior roots beyond the situation of the ganglion belonging to the latter.
3. They form, together with the posterior roots, a mixed nerve, to every part of which they supply motor fibres.

The objections of Bernard to the view advanced by Bischoff begin with the very origin of the nervous filaments.

"In the first place," he says, "the mode of origin of the spinal accessory is not the same with that of an anterior root. The source of this nerve extends over a very considerable portion of the cord, while each anterior spinal root originates from a very limited point. Furthermore, instead of commencing, like the true anterior roots, from the line of separation between the anterior and lateral columns of the cord, the fibres of origin of the spinal accessory emerge from a part of the cord much further back, and very near, as we shall see, to the posterior column."

"Towards the upper part of the neck, the roots of the spinal accessory are comparatively long, and the trunk of the nerve, running along the side of the spinal cord, rests upon the superior surface of the ligamentum denticulatum. As we go further down the neck, however, the roots of the nerve become shorter and shorter, and its trunk is consequently brought nearer the posterior columns, so that in the lower part of the neck it is placed very far back and in the immediate vicinity of the posterior spinal roots."

The mode of union of the spinal accessory with the pneumogastric is also regarded as differing, in an important respect, from that of the anterior and posterior spinal roots.

"A weighty objection is to be made against the view taken of the anastomosis of the spinal accessory, in its relations with the ganglion of the pneumogastric. It is well known that each anterior spinal root unites with the posterior root a little beyond the intervertebral ganglion belonging to the latter. Most authors, considering the jugular ganglion of the pneumogastric, or that which is seated upon the nerve at the point where it enters the posterior foramen lacerum, as the analogue of the intervertebral ganglion of a posterior spinal root, have thought to sustain their doctrine by relying upon the fact that the spinal accessory unites with the pneumogastric below this ganglion. But in the first place it ought to be shown this ganglion of the pneumogastric is really analogous to the intervertebral ganglion of a posterior spinal root. Now it is easy to demonstrate that the only ganglion which could be compared with those of the posterior roots is that situated on the trunk of the pneumogastric, below its anastomosis with the spinal accessory. This ganglion in certain animals, as the cat and the rabbit, is very visible and distinctly marked, while in the human subject it is represented by a kind of diffused gangliform swelling, to which is given the name of the *plexus gangliformis*, and which was very correctly described by Scarpa. The anastomosis of the spinal accessory differs accordingly from that of the anterior spinal roots, since it really joins the pneumogastric above the ganglion which is analogous to that of a posterior root."

M. Bernard does not intimate what are the anatomical grounds upon which he considers it "easy to demonstrate" that it is the cervical and not the jugular ganglion of the pneumogastric which represents an intervertebral ganglion of the posterior roots. We should have been glad if he had stated the reasons for this opinion more fully; for we cannot say that, in the absence of further explanation, they are very clear. On the contrary, it seems to us that the jugular ganglion of the pneumogastric, occupying the jugular fossa, is much more properly compared with the intervertebral ganglia, situated in the intervertebral foramina, than the cervical ganglion which is altogether outside the skull, particularly as the pneumogastric gives off one of its branches (pharyngeal) above this ganglion, though be-

low the other. The jugular foramen is certainly altogether analogous, in its anatomical relations, with the intervertebral foramina; and it would seem natural, therefore, to regard the ganglia which it contains as analogous to those situated in the latter.

There are other reasons, however, which lead Bernard to decide against the view entertained by Bischoff, as to the analogies of the spinal accessory. He lays a great deal of stress, for example, on the existence and source of the *recurrent sensibility* of a motor nerve, in determining its physiological relation with sensitive roots.

"We must now examine," he says, "by this new criterion the question of the association of the pneumogastric and the spinal accessory. We wish to know, in a word, whether the spinal accessory be the anterior root of the pneumogastric. For that purpose, we must ascertain whether the recurrent sensibility of the spinal accessory be derived from the pneumogastric, in the same manner as the recurrent sensibility of an anterior spinal root comes from the corresponding posterior root. If the pneumogastric furnish sensibility to the spinal accessory, we shall in that case be justified in saying that it plays the part, toward this nerve, of a posterior root. If the contrary should prove true, then the question must be decided differently; since the essential property which characterizes the association of the two roots of a pair of spinal nerves would not be found to exist between the pneumogastric and the spinal accessory."

"Now I have convinced myself that the recurrent sensibility of the spinal accessory, a property which I have found very distinct and well marked in this nerve, in the dog, the rabbit, and the goat, is not at all diminished by section of the pneumogastric; proof positive that it is not this nerve which furnishes the former with recurrent sensibility. I shall show subsequently that this recurrent sensibility of the spinal accessory is derived from the posterior root of the four upper cervical nerves; so that, according to this view, the spinal accessory should be regarded as a motor root superadded to the anterior roots of the four upper cervical nerves, since its recurrent sensibility is derived from the same source with theirs."

Finally, the physiological properties of the spinal accessory, as a motor nerve, are not such, in the opinion of the author, as to make it, properly speaking, the anterior root of the pneumogastric. It is true that it furnishes some motor fibres to the latter nerve, but not by any means all that it contains, nor even the most important of them. In the inferior laryngeal nerve, for example, a branch of the pneumogastric, the only motor filaments paralyzed by section of the spinal accessory are those concerned in the production of vocal sounds. The respiratory movements of the larynx go on perfectly well afterward, for the motor powers of the pneumogastric concerned in this function are derived from other sources.

The function of the spinal accessory, indeed, as connected with respiration, is a very curious one, if M. Bernard's views be correct; and we see no reason why they should not be admitted, in this particular, to be in accordance with all the known facts.

Sir Charles Bell considered the spinal accessory as an assistant in the movements of respiration, as complementary to the intercostals and diaphragm, and as calling the sterno-mastoid and trapezius into play when unusual force is required to expand the chest, as in laboured respiration. It seems, however, according to Bernard, that though this nerve be really connected with the respiratory function, the relation between the two is somewhat different from that stated above. Its influence is, indeed, antagonistic rather than favourable to the movements of inspiration. In the larynx, for example, we know that the spinal accessory presides over the formation of vocal sounds. Now these sounds cannot be made during

inspiration. They require the close approximation of the vocal chords, and a prolonged, sustained, and unusually forcible expiration, in order to produce the vibration in the larynx necessary to their formation. While the voice is in operation, therefore, breathing is temporarily suspended; and it is suspended by the action of the same nerve which produces, through its motor influence, the vocal sounds.

Such is the function of the internal or anastomotic branch of the spinal accessory.

Again, respiration is suspended in all those prolonged and violent muscular efforts which require the spine and thorax to be firmly fixed, as a point of support to the limbs and muscles of the trunk. A free inspiration cannot be made, as we may very easily convince ourselves, unless these parts be in a certain state of relaxation; and the instant any strong muscular effort is to be made, respiration is instinctively suspended. Now the sterno-mastoid and trapezius muscles, instead of acting, when under the direction of the spinal accessory, to increase the respiratory effort, are stimulated on the contrary to fix the chest and shoulder in an immovable position, and put a stop to respiration while the unusual muscular exertion continues. This is the function, according to M. Bernard, of the external or muscular branch of this nerve.

With regard to the analogical relations of the two nerves under discussion, it is a matter of less importance to classify them in a regular order, either together or separately, than that we should know all their peculiarities, and in what particulars they resemble or differ from the other nerves originating from the cerebro-spinal axis. All the cranial nerves present certain modifications in their course and distribution, by which they vary more or less from the regular type of the spinal nerves. These modifications they present in common with the nervous centres of this part and the cranial bones which inclose them. If it is not necessary, therefore, to insist upon absolute coincidence in every anatomical detail, in order to justify a general analogy between these nerves and the regular pairs of anterior and posterior spinal roots.

Still, the peculiarities of origin and function belonging to the spinal accessory are so marked as to separate that nerve, more widely than most others, from the regular type of anterior spinal roots. It is doubtful, indeed, whether that portion which originates from the spinal cord in the cervical region should not be separated physiologically, as it is separated anatomically (by the posterior cerebellar artery), from that portion originating from the medulla oblongata; particularly as experiment shows that the latter portion alone furnishes the internal or anastomotic branch, while the cervical portion is probably destined altogether for the external or muscular branch. The function of the spinal accessory, at least so far as it is distributed to the larynx, is undoubtedly, also, one of a peculiar and special nature. Its action in exciting the movements of the laryngeal muscles, and the modulation of the voice, is beyond question a peculiar one, and quite different from the ordinary motor action of a spinal nerve.

One of the strongest reasons, however, against considering the spinal accessory and pneumogastric as representing the motor and sensitive roots of a spinal pair, lies in the extremely peculiar nature of the sensibility belonging to the pneumogastric itself. It is not a nerve endowed with any acute general sensibility. It may often be divided, as Bernard testifies, in the living animal, without eliciting any sign of pain; while in the mixed and sensitive spinal nerves, and even more especially in the fifth pair of

cranial nerves, a mere touch will produce the most acute suffering. It is a special sensibility, connected with the functions of the lungs and the stomach, which is confided to the pneumogastric, and not the ordinary sensibility characteristic of the spinal nerves. We believe, therefore, that the real analogies of the case would be best satisfied by withdrawing both the pneumogastric and the spinal accessory from the general category of the cranial and spinal nerves, as we have already withdrawn the nerves of special sense, and by considering them as a motor and sensitive pair, differing from the others in the essential nature of the function intrusted to them, as well as in certain details of anatomical distribution.

If we still wish to classify the remaining cranial nerves in a regular series, we can do so by slightly modifying the original plan, and by making, at the same time, four pairs of cranial nerves instead of three; an arrangement more in harmony with the requirements of transcendental anatomy, as they are at present generally understood. By this plan, the pneumogastric and spinal accessory are left out of the list altogether, as being nerves of a special nature and destination. The three sensitive branches of the trigeminus are regarded as distinct nerves, distributed to separate regions of the face; and the glosso-pharyngeal constitutes the sensitive portion of the fourth and last pair. The list of cranial nerves would then be arranged as follows :—

	Motor portion.	Sensitive portion.	Distributed to
1st Pair	Oculo-motorius. Patheticus. Motor externus.	Ophthalmic branch of "5th pair."	Upper facial region.
2d Pair	Facial.	Superior maxillary branch of "5th pair."	Middle facial region.
3d Pair	Masticatory.	Inferior maxillary branch of "5th pair."	Lower facial region.
4th Pair	Sublingual.	Glosso-pharyngeal.	Tongue and fauces.

In the chapter devoted to the study of the pneumogastric, the author insists upon the peculiar character of the sensibility of this nerve, and upon its want of general sensibility. This insensibility of the nerve to ordinary painful sensations is sometimes complete; and when the pneumogastric shows signs of sensibility they are "nearly always of an obtuse character." The probable cause of the difference in this respect, shown by the nerve in different instances, is thus given by the author :—

"I have often," he says, "experimented upon dogs, in order to ascertain the conditions regulating the sensibility or insensibility evinced by the pneumogastric. I have not yet succeeded in arriving at a perfectly satisfactory explanation of all the phenomena observed; yet it has seemed to me, judging from the greater number of instances, that the pneumogastric is insensible in dogs while fasting, but sensitive during digestion. At the same time, I am far from offering this statement as the expression of an established fact. At all events, there is some modification of the sensibility in these cases—dependent, no doubt, on the special nature of the nerve—which demonstrates plainly that, in this respect, the comparison which has been drawn between the pneumogastric and a posterior spinal root, is an incorrect one."

In the remaining portions of the work, devoted to the history of the pneumogastric and the great sympathetic, there are many interesting details of experiments, though we do not find here anything particularly novel in the statements presented. The effects of dividing the different branches of the pneumogastric, as exerted upon the larynx, the lungs, the heart, and

the stomach, are fully discussed, and the different modes of death in these experiments explained, so far as they have yet been successfully investigated.

The author describes, also, the increase of animal temperature produced in the head and face after dividing the sympathetic nerve in the neck; and maintains, in opposition to Brown-Séquard and some others, that this effect is not due to a mere accumulation of blood by paralysis and distension of the vessels. He regards the increase of temperature, in a word, as an active and not a passive phenomenon; similar to the engorgement which takes place in a glandular organ, when, from a quiescent state, it passes suddenly into a condition of active secretion. This opinion has been maintained before, by relying upon the fact that an artificial stasis of the blood induces rather a diminution than an increase of temperature. If the veins coming from the ear of a rabbit, for example, be tied so as to arrest the return of blood, and confine it in the vessels of the part, the organ grows cooler as stasis becomes fully established. It may be very properly objected to this experiment, however, that though the quantity of blood *contained in* the vessels of the parts is thereby increased, the quantity actually *flowing through them* is diminished, and we cannot expect the phenomena of nutrition and calorification to be intensified when the nutritive fluid is prevented from arriving at the organ by the same means which prevents its departure.

The following experiment related by Bernard, however, is not, as he thinks, open to any such objection as the above, and leads to the same conclusion in a more satisfactory manner. He first ties the veins coming from *both ears* of a rabbit; and after the stasis of the blood, and consequent cooling of the parts on both sides, have become fully established, he divides the great sympathetic, on one side, in the middle of the neck. The immediate effect of this operation is a rise in temperature on the corresponding side, and, as both sides are in the same condition as to accumulation of blood, the rise of temperature in this case is thought to be necessarily due to some other cause, operating as an active stimulus to the tissues or the capillary bloodvessels.

It must be acknowledged that the nature and functions of the sympathetic are still shrouded in the greatest obscurity; and the difficulty of isolating it from surrounding parts, without inflicting extensive mutilations upon the animal, will undoubtedly be, for a long time, a serious obstacle in the way of experiments on this part of the nervous system. Its numerous ganglia, scattered everywhere at short intervals throughout its course, make it difficult, also, to determine the true direction of any particular set of its nervous filaments; while it reacts so sluggishly to all artificial stimulus, that direct experiment has so far produced very unsatisfactory results. Bernard, however, gives a very complete account of all that has been ascertained in regard to it up to the present time. He finishes the subject with some considerations as to the effect of dividing the sympathetic, and of wounding certain parts of the medulla oblongata and spinal cord, in stimulating and modifying certain of the secretions. J. C. D.

ART. XIV.—*The Diagnosis of Surgical Cancer.* By JOHN ZACHARIAH LAURENCE, F. R. C. S., M. B. Lond. Second edition. London: John Churchill, 1858. 8vo. pp. 126.

No subjects command greater interest at the present time than cancer and tubercle. Upon none is professional opinion more divided. The most opposite views are entertained as to the essential nature of these new formations, as to their heterologous or homologous character, the causes determining their occurrence, the legitimate means of diagnosis, and above all, as to the possibility of successful treatment. The subject of the little work before us, therefore, commands for it at once ready attention. It does not profess to be a systematic treatise upon cancer, but is limited to the question of diagnosis, and especially as applied to those cases in which the presence of an external tumour brings the disease under the charge of the surgeon.

The term cancer is employed by the author in its broadest sense, to signify the whole class of carcinomatous diseases. Applying this term to the group, or, as he designates it, "the genus," he discriminates the following "species:" 1. Scirrhus. 2. Encephaloid. 3. Melanotic cancer. 4. Villous cancer. 5. Osteoid cancer. 6. Colloid cancer. After a number of illustrative cases, referring especially to melanotic, villous, and osteoid cancer, the second chapter proceeds to present the data on which a diagnosis is to be based, considering the first two species as typical.

The data which will justify an accurate diagnosis and a wise prognosis, are to be sought: 1. In the presence or absence of the so-called predisposing and exciting causes; and 2, in the *symptoms of the case;* which are classed under the following heads: A. *Special previous history.* B. *Condition of patient when first brought under observation.* C. *Anatomy of the tumour.*

With regard to the predisposing causes, the first discussed is the question of *hereditary influence.* The doctrines entertained by Mr. Laurence on this subject can readily be gathered from a few quotations.

"The popular idea is, as medical practitioners must be well aware, that hereditary predisposition exercises an unusually baneful influence in the production of cancer;" but, "on the whole, I am inclined to regard this particular in the ætiology of cancer as of little or no value in a diagnostic point of view."

The following summary is presented, subsequently to a very interesting tabular statement of the "comparative mortality of cancer and phthisis in London in the years 1845 to 1850:" "Hence the average annual mortality in London from cancer in both sexes, from the age of 25 to 80, and from phthisis from the age of 5 to 65, is respectively 796.5 and 6040; and the ratio of the average mortality of cancer to phthisis is 1—7.5. Now I contend that, if cancer is less than one-seventh as fatal as phthisis, this fact alone is sufficient to render it highly probable that *cancer, as a rule, is not an hereditary disease.*"

Nevertheless, "that an hereditary taint *does* sometimes exist cannot possibly be denied." "M. Lebert states that 'he sought with much care for hereditary predisposition in 102 cases, and found it in 14 of these patients.' In 49 cases, which I have observed myself, and in which I have specially noted the point, I find 7 cases of an apparent hereditary predispo-

sition to cancer." "This proportion is too small to establish the hereditary nature of cancer." Yet, small as it is, it must be received with caution; for "the bare statement of a patient that one of his relatives died of cancer, is not sufficient evidence to the pathologist that such was really the case."

Having thus decided to reject the question of hereditary influence as an element in forming a diagnosis, the question of the influence of SEX is next discussed, and while admitting that the large majority of cases occur in females, yet regarding this to be due to the fact that the female breast and the uterus are favourite seats of cancer, the doctrine is propounded that, "however significant the influence of sex may be in the natural history of cancer, its diagnostic value becomes entirely lost."

Age, on the other hand, is regarded as an important element in the diagnosis, provided it be considered in connection with the *locality* of the tumour. "Supposing we are told that a female has a *tumour in the breast*, and nothing more; from this we can infer nothing. Supposing, again, we are told a female with a tumour *somewhere* is aged fourteen: from this we can infer nothing. But let us be told that that 'somewhere' is in the *breast*, and we at once, *exteris puribus*, derive a most important conclusion, viz: that the chances of the tumour being of a cancerous nature are infinitely small."

"The *previous health* of the patient gives us but little information. As a rule, it will be found that cancerous patients have been otherwise remarkably free from disease." In this connection the vexed issue of the possible coexistence of cancer and tubercle is discussed, and while the opinion is expressed that this complication is rare, the details of three cases are given in which the coexistence is stated to have been observed by Mr. Laurence.

The *exciting causes*, so called, are considered to be too equivocally established to be of any value in forming a diagnosis. With regard to the "*special previous history*," attention is first drawn to the *progress of the growth*. Malignant tumours, as a rule, grow more rapidly than benignant. But if too much stress be laid upon this fact, it will mislead; for while some innocent tumours run a very rapid course, some cancers progress very slowly.

Loss of flesh, which, "with exhausting sweats, derangement of the digestive organs, and a peculiar waxy tint of the countenance, form together the principal elements of the so-called 'cancerous cachexia,'" is often wanting; "in the early stages of their disease, patients often preserve their embonpoint in all its integrity."

As to the condition of the patient when first brought under observation, the "*local phenomena*" naturally demand the first consideration. Scirrhus and encephaloid receive separate consideration in this connection. The stony hardness, smaller size, and nodulated surface of scirrhus, its comparative fixedness, the dimpling and discoloration of the skin, &c. &c., are here discussed. The greater bulk and "elastic resilient feel" of encephaloid are insisted upon; and some curious cases are given illustrating the possibility of confounding a softened encephaloid with an abscess, and *vice versa*. "This is a less dangerous error than its reverse, that of taking an abscess for a tumour." "Roux, Sir Astley Cooper, Liston, have all done it, and I have myself seen a breast amputated for a supposed tumour, which turned out after the operation to be only a small chronic abscess." We fully agree with Mr. Laurence in the practical deduction, that the use of the grooved needle should always precede the knife, except in the most palpable cases.

As the disease progresses the tumour presents at some point the cancerous ulcer or fungus, the characters of which are next discussed, with some remarks on the tendency of malignant growths to slough. A few paragraphs referring to hemorrhage, pain of an intermittent character, and the enlargement of neighbouring lymphatic glands, complete the chapter, and with it the survey of the clinical facts which bear on the diagnosis of external cancer.

The third chapter is devoted to *the anatomy of cancer*, in which, after a short survey of the appearances presented to the naked eye, the question of the microscopic characters or minute anatomy of the growths is discussed. And here, indeed, is the vexed question in the diagnosis of cancer; some rejecting minute structure altogether as of no value in a diagnosis, others admitting the importance of a study of the minute anatomy of the growth, but variously divided as to the characters on which an opinion is to be based. Mr. Laurence appears fully aware of the real value of the microscope in these cases: "That it IS a material aid in the solution of the nature of doubtful cases," he says, "cannot well be denied."

The value of minute structure admitted, the question at once presents itself, What anatomical details shall be relied upon as indicating the cancerous nature of the growth?

We are disposed to think, after a careful perusal of this work, that the author has hitherto chiefly directed his attention to the elementary forms observable in these growths, neglecting almost totally the question of the secondary grouping of the form elements. He gives, in his neat plates, numerous drawings of cells and nuclei, but no *sections* of either scirrhus or encephaloid. In view of this, his opinions on the question of microscopical diagnosis appear to us the inevitable sequence of the facts in his possession. But we believe that he would perhaps modify them much, should he turn his attention to the study of thin sections.

Without intending to insist upon it as specific, he still retains the faulty term "cancer-cell." A term which, bad as it is, we quite agree with him is far better than such technical expressions as "Thnetoblast," proposed by M. Robin, or "Macrocyte," suggested by M. Léopold Ollier.

The characters assigned to the cancer-cell are those so often reiterated. The variable size, shape, and granular appearance of the cell, with the large, sharply-outlined single, double, or plural nucleus, and the bright, large, single or plural nucleolus, upon the *vesicular* character of which M. Ollier has laid such stress.

The cell is occasionally filled with fat-globules, and transformed into a granular mass of variable size or shape. Bodies of this kind are found more or less abundantly in most cancers. Free nuclei frequently exist, and may compose the whole of a growth. Bundles of connective tissue are often observed, and the above elements, with fat-globules, the debris of disintegrated tissue, and *fibro-plastic* cells (by which the author implies the cells of developing white fibrous tissue), constitute the elements of the so-called "cancer-field."

He considers that it is upon the presence of these elements, but especially upon the presence of the so-called "cancer-cell," that a diagnosis is to be founded. "In the greater number of cases of cancerous tumours the so-called cancer-cell will be found." At the same time, he is perplexed with the further fact, which indeed is inexplicable from his stand-point, "that this form of cell is occasionally seen in growths manifestly innocent."

The grand question of the homology or heterology of cancer, commanding rapidly so much attention, is, of course, beyond the province of a work

devoted exclusively to diagnosis. We are, however, pleased to observe in these pages passages which indicate a vast progress beyond the old idea of heterologous structure. Especially do we refer to the statement of Mr. Laurence, that he has observed a full series of transition forms between the "typical cancerous and typical fibro-plastic cells." A series of such forms are figured in the plates. The significance of this fact is not lost upon our author, who propounds in connection with it the following proposition :—

"*That there do exist cell-forms, which it is difficult or impossible to refer either to the cancerous or fibro-plastic type exclusively;* and, as a corollary, *That the existence of such forms brings us to the conclusion, that the two forms of cell cannot but be regarded as the extreme links of a chain of forms connected by intermediate stages.*"

It would appear to us that the facts proclaimed in this proposition are in themselves alone sufficient to overturn the doctrine of heterology so long taught in connection with cancerous growths, and although Mr. Laurence does not enter into this discussion, yet his views can be inferred when we find him state that the facts above propounded are "in conformity with the unity of natural products;" and especially when we find him quoting approvingly the broad proposition of Carl Wedl : "The fundamental character of cancer is that of a malformed (aborted) and degenerating new formation of connective tissue."

Entertaining such views, and studying merely the elementary forms of the growth, and not their secondary arrangement, Mr. Laurence's conclusion appears the necessary result of the facts in his possession when he says : "That the results afforded by the microscope must take *a* position, but not an exclusive and overbalancing one, in the series of data which are to serve us as the premises for our conclusion," as to the cancerous nature of a given growth.

Chapter 4th is devoted to colloid cancer, which has lately been so profoundly investigated by Rokitansky. The question of the cancerous nature of these growths is yet *sub judice.* "In our present state of knowledge, it will be best to consider it as a form of cancer. Subsequent research may one day definitely settle the question."

Chapter 5th considers the subject epithelioma, concerning which the opinion is expressed, that "*Epithelioma is a local disease* (*quite distinct from cancer*), *and if it is completely removed before any affection of the lymphatic glands, the patient becomes permanently cured.*"

We forbear any lengthy comments upon this opinion, with which we cannot possibly agree, for although the existence of innocent epitheliomata is undoubted, epitheliomata resulting from the influence of the law of analogous formation upon a cancerous growth, appear to be no less well established. Our object in this article has rather been to present our readers with a succinct account of the author's views than to enter into a controversy with any of them with which we do not agree. Such a discussion would occupy more space than we can at present devote to it. On the whole, we commend Mr. Laurence's work to the favourable consideration of American surgeons. However we may differ from him upon certain points, we feel convinced that he is upon the right path. If our knowledge of cancer is to be progressive, careful anatomical investigation, among other means of study, must not be neglected. For ourselves, we feel convinced that the establishment of the doctrine of the homology of cancer which Carl Wedl proclaims, and at which Mr. Laurence hints, will open a new era in the study of pathology. J. J. W.

ART. XV.—*The Physiology and Treatment of Placenta Prævia.* Being the Lettsomian Lectures on Midwifery for 1857. By ROBERT BARNES, M.D., F.S.S., &c. &c. London: John Churchill, 1858. 12mo. pp. 208.

THE difficulties attendant upon the management of cases in which the placenta is attached to the cervix uteri, the diversity of views held by practitioners of acknowledged experience as to its proper conduct, the high mortality which has everywhere been found to attend it where the records of a large number of deliveries can be consulted, and the importance of the interests at stake, invest with unusual interest every suggestion calculated to throw light upon the physiology of placenta prævia, or its successful treatment.

Dr. BARNES has been for some time favourably known by his contributions to the pathology of the placenta. The present volume is devoted to the exposition of certain original views in relation to the physiology and treatment of placenta prævia, which are introduced by a sketch of the doctrines generally taught.

Nearly all the older writers recognized the attachment of the placenta to the cervix, though some considered it as having been accidentally detached from the fundus and fallen down; and they taught, with more or less distinctness, the necessity of delivery by turning. Puzos, having more confidence than his contemporaries in the powers of nature, advised the excitement of uterine contractions by irritating the os by the finger, followed by a rupture of the membranes as a means of terminating the hemorrhage, and left the case to nature. RIGBY, unaware that others had entertained and even published similar views, announced as the result of his own observation that the attachment of the placenta is to the internal surface of the os uteri, and that, when thus placed, hemorrhage is unavoidable; that these cases differ essentially from those of accidental detachment from the fundus, and hence he drew the distinction of unavoidable and accidental hemorrhages, the one demanding early delivery, the other seldom requiring it.

Our author proceeds to cite the opinions of more recent authors, in which the *unavoidable* character of the hemorrhage and the necessity of *artificial delivery* is distinctly avowed. Thus, Dr. COLLINS says: "We may conclude that where the placenta is fixed over or near the os uteri, nothing but delivery will put a stop to the loss of blood." Also, INGLEBY says: "Pain, efficacious as it is in the accidental form of hemorrhage, unless adequate to the expulsion of the child, is *neither* to be *expected* nor to be *desired* to any material extent in the unavoidable form, as it only renders the effusion more abundant."

"By all these authors it is assumed as law," says Dr. Barnes, "that the hemorrhage is in direct ratio to the activity of the labour. It is assumed that the cervix cannot expand without causing hemorrhage. This assumption, partly true, involves a fundamental error or oversight."

These are unquestionably the doctrines and practice which have prevailed during the past hundred years. Since the publication of Puzos' memoir in 1743 we know of no author who has taught that simple rupture of the membranes and the encouragement of the expulsive efforts of the womb afford the best chance in cases of complete cervical attachment, though in cases of partial attachment this has become the established rule of practice.

A few years since, Prof. SIMPSON proposed an expedient, which, though designed as a resort only in exceptional cases, has been by many designated as the "new treatment." It is true that Mr. Kinder Wood and Mr. Radford had both successfully adopted this plan; but, whether this fact was known to Prof. Simpson or not, to him is due the merit of first offering it to the profession, accompanied by facts of sufficient number and importance to challenge a thorough examination of its merits. Finding that in numerous recorded cases of placenta prævia, in which the placenta had been spontaneously expelled before the birth of the child, the flow of blood had at once ceased, Dr. Simpson proposed artificial detachment as a remedy for those cases of alarming hemorrhage in which delivery is impossible or unadvisable. Incidentally connected with this proposed practice was the theoretical dogma that the hemorrhage in cases of this accident proceeds chiefly from the mouths of the placental vessels, and not from the exposed surface of the uterus. The plan, thus fortified by the doctrine of the placental source of hemorrhage, from which, if true, it seemed quite clear that artificial separation must necessarily put an end to the loss of blood, enlisted the attention of the entire profession.

The practice has been adopted in not a few instances, and the results reported in the medical journals. In his prize essay[1] on placenta prævia, Dr. TRASK has presented the collective experience of the profession on this subject, so far as it could be learned from reported cases; and the result of the inquiry confirms the assertions of Dr. Simpson, and renders it very clear that artificial detachment of the placenta may be relied upon as a generally effectual means of terminating the hemorrhage, subject to the disadvantage that the life of the child is almost necessarily sacrificed by the destruction of the placental connection with the mother. The design of Dr. Simpson's recommendation has been misapprehended by those who have delivered by turning immediately after separating the placenta, inasmuch as this operation was proposed only for cases in which turning could not be safely performed. The plan of artificial detachment may be regarded, therefore, as one of the expedients at present resorted to for the management of the most hazardous cases of this accident.

The views presented by Dr. Barnes as new, and as furnishing a more correct explanation of the physiology of the accident in question, and a more rational treatment, may be briefly stated as follows: Uterine contractions, so far from being deprecated in labours complicated with placental presentations, are to be encouraged as the agency upon which the patient's safety depends. In every case there is a point up to which the separation of the placenta will take place, and beyond which it will not proceed; which being attained, hemorrhage will spontaneously cease. In cases of extreme urgency, a *partial separation* by the hand, to the extent to which it would take place could labour proceed without extraneous interference, will secure all the benefit claimed for entire separation of the organ, with the great advantage that it allows a continued connection between the mother and the fœtus through the undetached portion of the placenta.

We propose briefly to examine the grounds upon which these doctrines are based, and the practice recommended. "What is the course of a labour complicated with cervical or central attachment?" The prevailing doctrine is that there is an essential difference between the circumstances attending the separation of the placenta from the fundus and its detachment from the

[1] See Transactions of the American Medical Association for 1855.

cervix; that the separation from the fundus occurs without hemorrhage, and that from the cervix with hemorrhage, which is unavoidable; that contractions of the womb, which in fundal attachment secure against flooding, in cervical attachment necessarily cause flooding. This doctrine Dr. Barnes regards as physiologically erroneous.

If we watch a case of placenta prævia which does not require the interference of art, we observe that every contraction of the longitudinal fibres of the womb, by which the lower segment of the womb is drawn upward and the os uteri opened, is accompanied by a gush of blood, which ceases as the contractions subside, and that the successive contractions are accompanied by successive discharges of blood, with intervals of freedom from hemorrhage, until at last, if the child has not been first expelled, "a stage arrives when the recurrent contractions of the womb do not entail further flooding," and hemorrhage ceases. Other authors have remarked this circumstance. Our author quotes, by way of illustration, several cases from Dr. LEE and others, in which hemorrhage spontaneously ceased before the birth of the child. The question arises, why does hemorrhage in these cases cease? The prevailing opinion is that the presenting part of the child, under uterine contractions, acts as an internal plug for the bleeding vessels; to which our author objects that "bleeding has often ceased before the membranes were ruptured, and consequently before the child was brought into contact, by pressure, with the neck of the womb."

Our author cites at length a case reported by himself, in which report, as he believes, the true physiology of the accident was for the first time unfolded. In this case of partial presentation the hemorrhage ceased after the employment of the tampon, under a presentation of the feet, the membranes being intact. "The detached portion of the placenta had become plugged up by coagula, and the remainder, being attached to the *body* of the uterus, was not liable to become separated during the contractions of that organ," and the case was resolved into one of natural labour so far as the placental attachment is concerned. Dr. Barnes maintains that the placenta is cast off from the cervix, in concentric zones or rings if the case be one of placenta centralis, and in segments of rings if it be a partial presentation.

> "Zone after zone is bared by recurring contractions, and necessarily sealed up until that physiological limit, that line of demarcation between normal and abnormal placental implantation, the boundary-line of placental detachment which I claim to have discovered, has been reached. This zone attained, the labour is a natural labour!"

It is not, says Dr. Barnes, the "separation of the placenta which secures immunity from flooding, but the contractions of the womb." Dr. Barnes quotes the very pertinent views of Sir CHARLES BELL relating to the muscularity of the womb; they are in fact, says our author, the anatomical foundation of the physiological views which he seeks to enforce. Our author divides the superficies of the uterus into three zones: the fundal zone, the seat of usual and safe placental attachment; the middle zone, the seat of lateral attachment; and the cervical zone, or the seat of dangerous placental attachment; the boundary-line between this last and the middle zone he terms "the lower polar circle," or the "limit of spontaneous placental detachment." Below this boundary-line we have spontaneous placental detachment, with hemorrhage; above it, spontaneous separation, and exemption, for the most part, from hemorrhage.

The position of the lower polar circle, or boundary-line between hemorrhage and safety, our author considers to correspond with the line occupied

by the os uteri when sufficiently expanded to allow the passage of the full-grown fœtal head, which, by measurement, he finds to be about three inches; so that "if we describe a circle within the womb, at three inches distant from the os, * * * we shall have the boundary-line between hemorrhagic and non-hemorrhagic placental attachment." Confirmatory of this, we find, on examining the placenta after its expulsion, that the portion which had been adherent within the cervical zone is infiltrated with coagulated blood, presenting a different appearance from that which had remained attached above the line, and which had been detached after expulsion of the child; and this part, according to our author, usually measures from three to four inches in depth. Our author expresses the belief that this boundary-line of safety is often practically reached, and the hemorrhage stopped, when the os has not opened wider than a wineglass, from the distension by the bag of membranes under expulsive pains. He next considers the two classes of cases in which partial detachment of the placenta may be artificially resorted to, viz., those of undilated os uteri, and those of extreme prostration, the classes in which total detachment has been recommended. Our author's reprobation of forced delivery in cases of undilated cervix meets our hearty concurrence. Though sanctioned by some eminent authorities, it was characterized by DEWEES as an "outrageous practice;" and he declared that during his practice of thirty years the indications, so far as he had witnessed, were "readily met by the use of the tampon and other auxiliary remedies."

The value of total detachment is next considered. He expresses the belief that in the reported cases of this operation, hemorrhage ceased not because the whole placenta was separated, but because the separation had reached that physiological limit which he has pointed out. Of this he offers no proof, except that in some of the cases hemorrhage had ceased *before* the placenta was wholly detached. He finally says: "We are amply justified in concluding that the artificial detachment of the placenta cannot be relied upon to arrest the hemorrhage." Now, without stopping to discuss the point which we consider satisfactorily settled by the researches of Dr. Trask, in which it is shown that, of *sixty-six* cases, in *thirty-five* hemorrhage ceased "immediately and entirely," and in a large proportion of cases hemorrhage ceases "either at once and entirely * * * or within a short time, and that if it continues at all, it is but to a trifling degree"—we cannot but express our surprise that our author should seek to throw discredit upon a procedure which certainly embraces his own plan; for if total separation is inadequate to suppress hemorrhage, it seems difficult to understand how a partial detachment should accomplish this end.

In the partial detachment of the placenta from within the cervical zone, we have, according to our author, a remedy adapted to cases in which "ordinary means are impossible or dangerous."

It cannot be denied that the plan of Dr. Barnes, should its promises be realized, presents great advantages over the entire separation of the placenta both in ease of performance and in the superior chance afforded the child.

Dr. COHEN, of Hamburg, has proposed a somewhat similar course, without recognizing the physiological doctrine upon which that of Dr. Barnes is based. Dr. Cohen advises that in those cases where the symptoms are too urgent to allow us to trust the case to nature, we are to convert the case from a central to a lateral placenta. To accomplish this, we are directed to find out on which side of the uterus the greatest bulk of the uterus is situated, and to feel for the edge of the placenta upon the side

opposite to this, then tear the membranes freely from the border of the placenta and sweep the finger round half the circumference of the os uteri, so as to detach the placenta completely from that side of the uterus occupied by the smaller portion of the placenta. The uterus expands, the detached portion of placenta drops down by the side of the presenting part and is compressed by it, and hemorrhage is suppressed. Dr. Cohen affirms that he has often practised this with success, having never lost a mother, and seldom a child. This is precisely what occurs in partial presentations, when the membranes are ruptured after more or less spontaneous separation. In the very instructive series of cases of placenta prævia published by Dr. Radford, and republished in this *Journal*, April, 1856, a case is reported which occurred in 1826, in which Dr. Radford performed the very operation recommended by Dr. Cohen. Under increase of pains the detached portion "took a lateral position, allowing the child to pass by, and which was born alive in about three hours." Dr. Radford remarks: "I detached the placenta as freely as I thought necessary for the passage of the child, as it is better systematically to do this, rather than risk the tearing of the placenta," &c. This operation was performed in several other cases reported by Dr. Radford. In cases of central placenta, Cohen's plan must often be adopted to afford an opportunity for the passage of the child. Whether the plan of Cohen or that of our author is followed, in either case active contractions of the womb are indispensable to the safety of the patient. To this end, while the plug is used to favour coagulation, friction of the abdomen, a firmly applied bandage, ergot, &c., must be resorted to, and in the failure of these, galvanism may be employed, which our author affirms rarely or never fails.

Whatever may be thought of the reasonableness of the views here presented, they are susceptible of being tested by experiment; and had the publication of this book been delayed until it could have been fortified by a series of cases in which the operation had been performed, a more intelligent judgment could be formed. Our author has chosen to depend upon the arguments drawn from the sources indicated, rather than to wait for an accumulation of facts in support of his views. Total separation of the placenta has been put to the practical test, and has proved successful in accomplishing the great object for which it is employed, with the very serious disadvantage, however, that it almost necessarily entails the destruction of the child. If partial separation, as recommended by our author, will accomplish the same good, and yet secure to the child the means of sustained existence, it will prove an invaluable boon. We have sought in vain in the medical journals for the record of any trials of this method; it remains to be seen if it will accomplish this good. Our author provides for the contingency of its failure by suggesting total separation as a last resort, though he has previously expressed a belief that it cannot be relied upon.

In the appendix are a number of cases, original and selected, in which hemorrhage had existed, but ceased spontaneously before delivery of the child, from which circumstance our author argues that the doctrine that hemorrhage in cases of placental presentation is unavoidable, is untrue, and leads to serious errors in practice.

"Under the prevailing dogmas the arrest of hemorrhage inspires no hope in the heart of the accoucheur. He is taught to believe that it will *unavoidably* return; he hastens to deliver, and the poor woman, who had reached the haven of safety, is destroyed by the operation, a victim of the '*nimia diligentia medici.*'"

There follow two cases in which the placenta was partially detached by the hand. One is reported by Dr. Simpson, as of probable entire separation, which our author claims, and which very probably was but partial detachment; the other a case occurring to our author himself at the sixth month.

The alleged discovery of our author that hemorrhage ceases in the cases above referred to because a certain degree of dilatation is reached beyond which separation is no longer induced, is not, we think, clearly established, though his views must be regarded as very ingenious, and calculated to give precision to our knowledge and practice. The character of the evidence upon which he relies is, to a certain degree, the same that he has justly criticized in the paper of Dr. Simpson on total detachment. He relies upon the fact that hemorrhage sometimes ceases spontaneously before the birth of the child. Dr. Trask has shown a very essential difference between labours in which the placenta was spontaneously expelled before the birth of the child, and those in which it became subsequently detached. In the first class the child also was born by the unaided efforts in 57 per cent. of the cases, while in the latter class spontaneous delivery occurred in only 17 per cent. "The only explanation that can be given is, that cases in which the placenta is expelled before the birth of the child, as a class, are characterized by a tonicity of the womb and a vigour of uterine contraction which we do not find in ordinary cases of this accident." In the cases of spontaneous arrest of hemorrhage quoted by our author, it is highly probable the labours were of a similar character, and we see the risk of assuming upon such a basis that a partial artificial separation will arrest the flow.

Although our author attaches but little importance to the amount of hemorrhage proceeding from the separated portion of placenta, but believes that the principal source of bleeding is the exposed mouths of the uterine arteries upon the uterine walls, it is by no means clear that the hemorrhage in these cases does not depend in a very important degree upon the placenta. Great stress is laid upon the consolidated, indurated condition of the detached portion of placenta. The experiments of Dr. DALTON (see *Amer. Med. Monthly*, July, 1858), seem to demonstrate that there is a free intercommunication of all parts of the maternal portion of the placenta. The uterus of a woman who had died undelivered was removed and placed under water. Air introduced through a blowpipe into the mouths of the divided vessels of the uterus passed freely through every part of the placenta, and escaped in bubbles on making punctures in any portion of the transparent chorion. Now, although the detached portion of placenta may, after the lapse of time, on its delivery, be found infiltrated with coagulated blood, and refuse, as in the experiments of Mackenzie and Sharpy, to allow the escape of blood injected through the hypogastric arteries, it by no means follows that the vessels were thus impervious immediately upon the separation of the detached portion of placenta. Indeed, the more reasonable view seems to be that during a pain, when hemorrhage is most active, the bleeding is to a large extent through these vessels, the womb at that time being acknowledged to be in a state of active contraction; hemorrhage is kept in check between the pains by tonic contraction of the womb, and by coagulation in the separated portion of placenta which sooner or later takes place. The peculiar cellular cavernous or reticular structure of the placenta is well adapted to favour coagulation.

Whether the proposal of partial detachment prove in practice adequate to suppress alarming hemorrhages or not, the work of Dr. Barnes may be

read by all practitioners with great profit. With a confidence in the resources of nature, the result of a careful study of the processes by which he believes the various steps of labour accomplished, he thus speaks in her behalf:—

"Nature declares and pronounces emphatically that the hemorrhage is not in all cases unavoidable and progressive in proportion to the dilatation of the mouth of the womb. She protests against the assumption that, in this great emergency, she is altogether at fault, and powerless to arrest the flooding. Let not those who have never had the courage to trust her, the patience to observe her, or the skill to interpret her, too confidently deny her power"—p. 40.

In cases of partial presentation we know that nature most generally proves equal to the emergency, and effects spontaneous delivery; and this, we must believe, would always be the case could the bleeding be kept in check until the os uteri is dilated. Delivery by turning is regarded by every well-informed practitioner as an evil to be deprecated. How much encouragement is offered in the assurance, should it prove well founded, that after a certain degree of dilatation hemorrhage spontaneously ceases, and a spontaneous delivery may be expected. How to accomplish this temporary arrest is the great problem. Tnat it may, in many instances, be accomplished by the tampon, experience amply shows; repeated examples of its successful employment will be found in the cases of Dr. Radford, already referred to; these form a most instructive series which, in connection with this subject, we advise our readers to consult afresh.

The diversity of opinion in regard to the value of this remedy must depend upon the difference of manner of its employment. Dr. Lee, in speaking of cases of rigidity of the os, says:—

"The tampon or plug has no power to restrain hemorrhage in such cases, nor do I know of any other means—either cold, quietness, or opium—which effectually have, and it is sometimes absolutely necessary to deliver by turning before the hand can be possibly introduced into the uterus without producing fatal contusion and laceration of the soft parts."

In this we think Dr. Lee differs from most practical accoucheurs. We are satisfied from our own experience, that success in the use of the tampon depends upon its thorough and efficient application. The pieces of sponge or strips of linen should be successively introduced until the vagina is completely filled, a compress placed over the vulva, and the whole confined by a T bandage, and a dry folded sheet placed beneath the hips. The least return of bleeding can thus be readily detected, and the dangerous oozing which sometimes undermines the patient without the knowledge of the attendant, may be guarded against. We have completely succeeded by first introducing a full-sized tubular speculum, packing it with the plugging material, after the ordinary mode had failed to arrest the bleeding. Dr. Radford, in addition to the plug, attaches much importance to compression of the abdomen, by a broad bandage which he calls a "retaining bandage," passed around the body, one end being attached to the bedstead, and the other tightly drawn by an assistant. It has been objected to the use of the plug in the early months, that its presence tends to induce premature labour, whereas we ought to try to conduct the case to full time. This is a matter of no little importance, but we are of the opinion that this result is not to be regarded as necessary or general.

Dewees did not so regard it, and the cases of Radford exhibit proof positive to the contrary.

Prof. Miller, in his *System of Obstetrics*, advocates with great earnest-

ness and ability the substitution of Puzos' method slightly modified, in place of artificial delivery; this method consisting in "*originating expulsive contractions* of the uterus by the tampon or plug, and then puncturing the membranes, relying on the tampon to control the flooding until the liquor amnii is evacuated."

If the plug prove inefficient, and the hemorrhage continues, our author's plan of partial separation may be resorted to, and if it fail, we have total separation as a resource which will rarely fail to arrest the flow of blood. But in every class of cases, if our author's views be sound, we have the encouragement that the hemorrhage is not strictly *unavoidable*, as generally understood—that it will not *necessarily* continue until delivery, but that if we can keep it in check until a certain degree of dilatation is reached, and sustain and stimulate the powers of nature, as by ergot and galvanism if need be, it will eventually cease, not to return. J. D. T.

ART. XVI.—*Sur une Fonction peu connue du Pancréas la Digestion des Aliments azotes.* Par LUCIEN CORVISART. Paris, 1857–58.
On a Function of the Pancreas but little known. BY LUCIEN CORVISART.

IF we except the liver and spleen, there is no organ of the body whose exact function has been so long veiled in obscurity as that of the pancreas. There are several reasons for the uncertainty which has existed, and under which we, in some measure, still labour in regard to the office performed by the pancreas in the economy. Not the least of these is the fact (with the exception of one or two anomalous instances in the cat, in the seal, and in certain fish of the genus salmo) of there being no reservoir for containing the fluid which it secretes, and, consequently, there has always been a difficulty in obtaining a sufficiency of the pancreatic juice for a thorough analysis.

Bernard, whose keen sense of the true nature of experimental research never deserts him, has pointed out another cause for the various opinions which have existed relative to its composition, viz: that the pancreatic fluid varies greatly in its characters according as it is collected during the inflammatory process which follows the wound necessarily made in the abdomen of the animal submitted to experiment, or after the parts have regained their healthy condition. In the former case, the liquid is in an abnormal state, and not suitable for an analysis on which to form an opinion in regard to its physiological properties; in the latter, it is normal, and, consequently, capable of performing its *rôle* in the system.

It is not strange, therefore, that from the time of François de la Boe, and Degraaf and Schuyl, his disciples—who contended for the acid character of this secretion—to Tiedemann and Gmelin—who sometimes found it acid and sometimes alkaline—and Bernard—who invariably obtained it with the latter reaction—there should have existed the most discordant testimony relative to its physical and chemical properties.

But if the earlier physiologists differed as to the chemical nature of the pancreatic juice, they were still more at variance in regard to its use. Some considered it as serving to separate the chyle from the feces; others, as contributing to modify the acridity of the bile; others, as dissolving those sub-

stances which had not been digested in the stomach; and Magendie, in the absence of reliable evidence, with more honesty than his predecessors, admits that he is unable to specify what purpose it fulfils in the organism. Even at the present day, when baseless theories do not go for facts, and when the spirit of experimental research has been directed to the task of enlightening us, physiologists are not altogether agreed relative to the functions of the gland under consideration.

Three functions are now assigned to the pancreatic fluid:—

1st. The conversion of starch into glucose.

2d. The faculty of forming an emulsion with fat.

3d. The digestion of the albuminous matters not acted upon by the stomach.

The action of the pancreatic juice upon amylaceous food, whereby it is converted into sugar, was first pointed out by Valentin, and may be considered as definitely established by many recent experimenters. The faculty of forming an emulsion with fat, and thus promoting its absorption by the lacteals, though surmised by Eberle, was never distinctly and prominently brought forward, till Bernard commenced his investigations. Bernard's doctrine, though opposed by Lehmann, Frerichs, Bidder and Schmidt, Lenz, and others, has been too frequently verified by the experiments of Jackson, Jones, and numerous other physiologists, to be discarded, and it may, therefore, be considered as admitting of but little doubt, that one of the most important actions of the pancreatic liquid is that sustained by the industry and genius of Bernard.

Purkinje and Pappenheim were the first to suggest the theory that the pancreatic fluid served the purpose of digesting the nitrogenous matters which have escaped the action of the stomach.

Bernard and Barreswil also hold the opinion that the *acidified* pancreatic juice is capable of dissolving the proteinaceous substances taken as food. According to the former, the bile precipitates these matters from their solution in the gastric juice, and they subsequently, coming into contact with the pancreatic fluid, are redissolved. These actions of the bile and pancreatic liquid also take place with the amylaceous substances, but in this case the latter secretion remains alkaline, whilst, with the albuminous matters, it becomes acid.

Within the last two years, this latter theory has become intimately associated with the name of M. Corvisart, in consequence of its advocacy by this physiologist, through the columns of one of the medical journals of Paris and before the French Academy of Sciences. The memoir now published is, however, the first time that his views have been presented in a complete form; and, though they may not be supported by what we regard as conclusive evidence, yet coming as they do from one of the most eminent of the French physiologists, they are entitled to a respectful and attentive consideration. We propose, therefore, briefly to review some of the main points of the theory advocated by M. Corvisart, to notice the experiments upon which it chiefly rests for support, and to call attention to the facts which militate against it.

The theory advocated by M. Corvisart, as we have already stated, is, that the pancreatic juice possesses the faculty of digesting those nitrogenous substances which have not been acted upon by the stomach, and that it exercises this function not only exteriorly to the body, but also within the animal organism.

The first section of the memoir is devoted to the consideration of the

action of the gastric juice on the albumen of the egg. It is here stated that this secretion dissolves a third of its weight of egg albumen, renders it incapable of coagulation, and transform it into albumen-peptone. We shall not stop, at present, to question the entire correctness of this proposition, but proceed to the next paragraph, which relates to the action of the pancreatic juice on albumen.

A single experiment on the living subject is adduced under this head.

"In a young and large dog, weighing fifteen kilogrammes [about thirty-three pounds], which had fasted twenty-four hours, I opened, without injuring the pancreas, both ends of the duodenum, and carefully washed out the juices, &c., from the intestine with a stream of water $+38^\circ$ C. I then intercepted all communication with the stomach by a ligature, and, by the inferior opening, I introduced into the duodenum 78 grammes [about 1200 grains] of boiled white of egg. I then closed the opening by a ligature. There was still some food in the stomach.

"Eighteen hours afterwards, the animal was killed by strangulation.

"The pancreas was perfectly white, and of normal appearance. It had not been injured by the operation, nor obstructed in its circulation.

"The duodenum between the ligatures was very red, and distended by a liquid that strong pressure could not force out. The organ was opened and completely emptied of its contents; that is, of several pieces of softened albumen floating in 325 cubic centimetres [nearly 10 fluidounces] of an alkaline liquid, from which a light flaky precipitate was slowly thrown down on standing.

"The insoluble substance was collected and filtered, completely dried, and weighed. It amounted to 3 grammes 55 centigrammes [about 55 grains], representing about a third part of the dry albumen ingested.

"The duodenum had, therefore, dissolved nearly 50 grammes [775 grains] of the moist white of egg which had been placed therein."

It is then stated that, making allowance for the water taken into the duodenum with the albumen, at least 250 cubic centimetres ($7\frac{1}{2}$ ounces) of secreted fluid had entered the intestine. In a note, it is further asserted that bile was tested for by nitric acid, and not discovered to be present, and that the quantity of intestinal fluid must have been exceedingly small. The inference accordingly follows that, in eighteen hours, 250 cubic centimetres of pure pancreatic juice had dissolved 50 grammes of moist white of egg, equivalent to 3.55 grammes of the dry substance.

There are several objections to be urged against the deductions drawn from this experiment. M. Corvisart contends that the pancreatic juice, unmixed with bile or any other substance, possesses the faculty of dissolving albumen. We do not think this experiment establishes his view. In the first place, we think he has undervalued the quantity of intestinal liquid present. Now Bidder and Schmidt and others have established, beyond doubt, that the intestinal juice possesses, in an eminent degree, the property of dissolving albuminous substances. As we have no reliable data relative to the quantity of intestinal juice secreted in a given time from a known surface of intestine, we are unable to say how much of the 250 cubic centimetres of liquid found by M. Corvisart consisted of this fluid; but, if we may judge from the experiments of Bidder and Schmidt—who from a large dog, weighing 20 kilogrammes, obtained, in 8 hours and 15 minutes, but 7.860 grammes of pancreatic juice, and from a strong man, weighing 64 kilogrammes, but 150 grammes in 24 hours—we should be still less disposed to attach much value to M. Corvisart's conclusion.

In the second place, there is no certainty that bile was not an important agent in effecting the solution of the albumen, and, if absent at the time the *post-mortem* examination was made, it may have been present and un-

dergone resorption, as M. Corvisart himself admits. Besides, we are far from being sure that it was not present when the examination was made. M. Corvisart informs us that he tested for bile with nitric acid, and obtained negative results. Now, the applicability of nitric acid as a reagent for the discovery of this secretion depends entirely upon its reaction on the colouring matter. As this is not a fixed quantitative constituent of the bile, the test is objectionable, and we therefore find that, when the colouring matter is present in but small quantity, nitric acid fails to indicate bile, even though the latter may exist in considerable proportion. The bile, also, may be so materially altered within the body, that the nitric acid entirely fails to discover it, no matter in how great quantity it may be present. We are unable to conceive why M. Corvisart omitted to make use of Pettenkofer's test, which is altogether more delicate and reliable than the one he employed.

The above remarks apply, of course, only to the experiment we have cited. In a subsequent experiment, M. Corvisart tied the common duct, and thus prevented the entrance of bile into the intestine. The intestinal juice, however, could not be prevented access to the albumen, and doubtless exercised its ordinary influence.

These are the only experiments performed with reference to the action of the pancreatic fluid by M. Corvisart, on living animals.

M. Corvisart objects to the method of experimenting with the pancreatic juice by obtaining it direct from the pancreas by a fistula, as practised by Tiedemann and Gmelin, Bernard, Bidder and Schmidt, and others. He employs, instead, an infusion of the pancreas in water. For this purpose, he takes the fresh gland, cuts it into small pieces, and exhausts it with water. The liquid thus obtained he finds possesses the property of dissolving the nitrogenous articles of food. This fact he has certainly established. Whether, however, the infusion of the pancreas is to be regarded as equivalent to its secretion, is, we think, exceedingly doubtful. M. Bernard found that the infusion of the pancreas was capable of forming an emulsion with fat, but he had previously determined that the secretion possessed the same faculty. M. Corvisart, on the other hand, has not demonstrated that the pure pancreatic juice dissolves albumen, and, in fact, Frerichs has completely refuted any such idea.

We are constrained, therefore, in view of what we have advanced, to refuse our assent, at present, to the theory of M. Corvisart. We do not pretend to say that he may not be correct in his views, but we do say that he has not demonstrated them so satisfactorily as to cause them to be received as physiological truths.

We have neither time nor space to follow M. Corvisart through the other divisions of his subject. The one we have commented upon is a type of the others, which relate to the action of the pancreatic juice on blood-albumen, fibrin, gelatin, casein, &c. There are a number of points of interest connected with his investigations for which we must refer the reader to the memoir. W. A. H.

BIBLIOGRAPHICAL NOTICES.

Art. XVII.—*A Practical Treatise on the Diseases of Children.* By D. Francis Condie, M. D., Fellow of the College of Physicians, Member of the American Medical Association, Member of the American Philosophical Society, etc. Fifth edition, revised and enlarged. Philadelphia: Blanchard & Lea, 1858. 8vo. pp. 762.

Fifteen years since the first edition of Dr. Condie's now well-known treatise, then just issued from the press, was reviewed in this journal. The able author of the review stated it to be his persuasion that the American medical profession would soon regard it not only as a very good book, but as *the very best* practical treatise on the diseases of children. The long and extensive experience, accurate observation, and diligent study, not only of English writers, but of the continental and especially of the German authors, had, it was said, fitted the author in a remarkable manner for worthily accomplishing his difficult task. For the practical physician, who should turn to its pages to learn all the phenomena which may be presented by the disease he is treating, and all the means to which he may resort for the cure of that disease, it was promised to offer many and strong attractions, amongst which were mentioned completeness, clearness, judgment, and good sense. Since then, every three years a new edition has been announced in the bibliographical department of this journal, showing the correctness of the previsions of the reviewer, and the appreciation by the profession at large of the very best practical treatise on diseases of children. Each successive edition has been carefully revised, and such additions made thereto as the increased experience or newly-acquired information of the author allowed. The volume, which at first contained 651 pages, is now extended to 762, and every sentence in the book has been again and again carefully weighed. The constant attention bestowed by Dr. Condie upon this treatise, its revision, and the improvements constantly being made therein, must have cost him great labour; but, at the same time, what a continual source of pleasure it must prove to an author to watch the growth in the favour of the profession of what is destined to make his usefulness live after him, and to transmit to other generations his learning and his name.

In the preparation of this fifth edition of his work, Dr. Condie tells us in his advertisement that—

"The entire work has been subjected to a careful and thorough revision—a considerable portion of it has been entirely rewritten, and several new chapters have been added.

"In the different sections will be found incorporated every important observation in reference to the diseases of which they treat that has been recorded since the appearance of the last edition; and in the several new chapters an account of some affections omitted in former editions, and for the accurate description and satisfactory management of which we are indebted mainly to the labours of recent observers."

We notice in this present edition that Dr. Condie regards the evil effects of enlarged tonsils as more serious than he was formerly disposed to do. It was taught by him that he had seldom seen much injury or very serious inconvenience to result from them in children; that they usually gradually dispersed, and seldom continued beyond the period of puberty. It is now stated by him that "in young children the tonsils are subject to a chronic enlargement, the effects of which are far more serious than the entire neglect which the affection has met with from medical writers would lead us to suppose;" and that "when enlargement is

once established, the hypertrophied tonsils never diminish in size; their excision, consequently, is the only means by which the inconvenience and injury resulting from their presence are to be remedied." It should be added, moreover, that any attempt to remove the hypertrophied tonsils during childhood is strongly opposed; "the operation being by no means generally successful, and it may be productive of consequences more troublesome than the disease"—p. 171.

It is particularly in the section treating of congenital affections, and accidents occurring most generally within the mouth, that the most important additions have been made. Three new chapters have here been added, treating respectively of cephalæmatoma, intestinal hemorrhage, and cysts of the neck.

The head of the new-born child, from the pressure to which it is subjected during a difficult and protracted labour, often presents a tumefaction, varying greatly in size and extent. This tumefaction demands scarcely any treatment; absorption of the contents takes place rapidly, and the swelling quickly disappears. Occasionally, however, the same part is the seat of another much more important and dangerous tumefaction, called by practical writers *cephalœmatoma*. Here the effusion of blood, instead of being beneath the cranial aponeurosis, and above the pericranium, is beneath the pericranium, separating that membrane from the skull. In some cases, besides the effusion of blood beneath the pericranium, on the outer surface of the skull, there exists at the same time a corresponding extravasation beneath the skull, between it and the dura mater. A hard, sharp, abrupt ring encircling the base of the swelling in cases of cephalæmatoma, enables the physician to distinguish it from the tumefaction usually met with. The account given by Dr. Condie of this affection is exceedingly full and satisfactory. In regard to the cause of its formation, upon which there are various opinions, he believes "that the pressure which the fœtal head experiences in even the easiest labours, in children labouring under a peculiar hemorrhagic predisposition, is sufficient to produce cephalæmatoma; and without this predisposition the utmost pressure of the head during labour will fail to produce it"—p. 683.

Of the treatment he says:—

"In the majority of cases, cephalæmatoma will do very well under a very simple treatment—confined pretty much to the application of cold evaporating washes, to hasten the resorption of the extravasated blood. The ordinary spirit-wash, or a solution of the hydrochlorate of ammonia in camphor-water, are among the best. Halmagrand recommends the hydrochlorate of ammonia in red wine; others, a solution of the acetate of lead, or the acetate of zinc in rose-water. A slight, graduated, and uniform compression of the tumour will in all cases be proper.

"The production of suppuration in the tumour, by caustic applications or a seton, as practised by Moscati, Goelis, and Pallettá, is a plan of treatment uncalled for in any case, and calculated to produce greater suffering and more unpleasant results than, in the great majority of cases, would be liable to occur, even were the tumour left entirely to itself.

"By Michaelis, Naegelè, and P. Dubois, an early incision, for the purpose of evacuating the effused blood and allowing the detached pericranium to be brought and retained in contact with the bone, is recommended. They direct a simple incision to be made, extending the whole length and depth of the tumour, down to the bone. After the removal of the effused blood, the edges of the incision are to be drawn together and secured by strips of adhesive plaster; the head being enveloped in a suitable bandage or cap, so adjusted as to make moderate pressure at the seat of the tumour.

"But, as remarked by Bouchut, incision should be resorted to only when a tumour of large extent has remained stationary in size for ten or twelve days after its formation.

"Levy directs the hair to be shaved from the entire surface of the tumour, and for some distance around its base, and a puncture then to be made with a lancet at a depending point, and equable pressure exerted by the fingers, so as to expel as much of the effused blood as possible; afterwards a compress is to be applied, secured by proper straps and bandages, and kept on for about six days.

"When suppuration occurs, and an abscess forms, this is to be treated on

general principles; an early opening for the discharge of the pus should, however, always be made. If the bone becomes necrosed, the dead portions should be removed as they become detached, and the appropriate dressings applied"—pp. 683, 684.

Occasionally a copious discharge of blood by stool, and in a few cases by vomiting also, is met with in the new-born infant. It is usually within the first ten days following birth, though it may take place at any time within the first two or three months, that this intestinal hemorrhage occurs. Of the cause of this hemorrhage Dr. Condie does not express any decided opinion. He says:—

"Intestinal hemorrhage has been supposed to result in the young infant from the compression its body has experienced during a tedious labour; that this cannot be so, however, is evident from the fact that the hemorrhage has occurred as frequently, or nearly so, after easy and rapid labours as after such as were difficult and protracted. Rilliet refers the disease, 1st, to the congestion of the intestinal tube, a condition which is normal in the new-born infant; and 2d, to some impediment to the speedy and complete establishment of respiration, in consequence of which the blood, unable to pass freely through the lungs, engorges the other organs, especially the intestines, the vessels of which, already loaded with blood, are unable to support this new tax. A somewhat similar explanation of the mode of production of the intestinal hemorrhage of the infant is given by Billard"—p. 692.

The treatment recommended is "to place the patient in a cool and frequently changed atmosphere, to apply cold compresses to the abdomen, and to keep the extremities comfortably warm." Diluted alum-whey in rose-water the author has found to act beneficially.

As this disease is often described and referred to under the name of *melœna*, it would have been well, perhaps, to have mentioned this fact.

Another affection occasionally met with in children, treated of in this present edition for the first time, is cysts of the neck. Of the nature of these cystic tumours, which often increase in size with great rapidity, and by their pressure impede most important functions, it is said:—

"The researches of MM. Fleury and Marchesseaux show that two distinct species of these cysts occur. The one being developed in the actual tissue or substance of the thyroid gland, and is in some instances superficial, in others deep-seated. This form of cystoid tumours corresponds with the *cellular* and *thyroidean serous goitre* of Beck and Heidenreich, the *hydrocele of the neck* of Manoir, the *hydro-bronchocele* of Percy, and the *encysted goitre* of other writers. The second species of cervical cyst is developed in the common cellular tissue of the neck, at a greater or less distance from the thyroid gland. It is the *hydrocele of the neck* of O'Beirne, the *cystic tumour* of Boyer and Dupuytren, the *fibro-serous cyst* of Delpech, and the *hygroma cellularis* of Von Ammon and other of the German writers. It is the latter species which appears to be most frequently met with in young children"—p. 724.

The treatment preferred by Dr. Condie is the puncture of the tumour, followed by iodine frictions and compression, whenever this latter can be put in practice, or the passage of a slender seton through the cyst. We should prefer, ourselves, to follow the puncture by an injection of a strong solution of iodine into the cavity of the cyst.

A fact, the knowledge of which is of considerable importance to the surgeon, is not mentioned by Dr. Condie. When these cysts of the neck are opened by the trocar, both those in the thyroid glands and the other cysts of the cervical region, which have been shown by M. Richard to have their origin in a peculiar alteration of the lymphatic glands, the contents, as they escape, soon become tinged with blood; little by little the proportion of blood becomes more considerable, until finally it is pure blood that flows from the canula, and this flow of blood will continue as long as the canula is left in the interior of the cyst. The removal of the eccentric pressure which the contained liquid had exercised upon the walls of the cyst appears to be the cause of this phenomenon, which is one calculated to worry very much the mind of a surgeon not aware of its constant occurrence.

W. F. A.

ART. XVIII.—*Diseases of the Urinary Organs. A Compendium of their Diagnosis, Pathology, and Treatment.* By WILLIAM WALLACE MORLAND, M. D., Fellow of the Massachusetts Medical Society, &c. &c. With illustrations. Philadelphia: Blanchard & Lea, 1858. 8vo. pp. 579.

EVERY one who adds a fact to our stock of knowledge does a service to his fellow-man, and he who by research collects and orderly arranges valuable scattered materials deserves his share of praise.

Lord Bacon exhibits in his New Atlantis nine different classes of men by whose co-operation knowledge is to be disseminated throughout the world: 1. There are the merchants of light; 2. The depredators; 3. The mystery men; 4. The pioneers and miners; 5. The compilers; 6. The dowry men, or benefactors; 7. The lamps; 8. The inoculators; 9. The interpreters of nature. If, then, a man fill well either of the above named offices, according to the great Bacon, he will advance the knowledge of society. We will not presume to designate where in the above classification our author stands. That he does not, in his work before us, hold a position among the pioneers and miners, he has told us in his preface; for he has aimed simply to present a "digest" of the entire subject of urinary pathology, to serve as a convenient hand-book for the large class of practitioners whose leisure does not allow them an extended examination of authors.

To enable him to do this, he has consulted and woven into a uniform treatise the observations of the great mass of authorities on the above mentioned subject.

The book before us is a large octavo volume, got up in uncommonly elegant style, and beautifully and copiously illustrated. It is divided into two parts, one devoted to the consideration of the diagnosis, the other to the pathology and treatment of the diseases of the urinary organs. There is also an appendix, containing reports of interesting cases and investigations bearing upon the subject of urinary pathology, together with a brief but excellent description of the method of clinically examining the urine.

In this part of the book the author has introduced some observations on the subject of diabetes, which he did not think proper to consider in the body of the work, as it is not a disease of the urinary organs, but an affection of assimilation. He quotes the observations of Drs. Hodgkin, David Nelson, Graves, and Goolden. The last named writer mentions the infrequency of true saccharine diabetes. Dr. Goolden is also quoted in reference to the frequency of saccharine urine without diuresis, which condition of urine he mentions as a very common affection in cerebral and other nervous affections, and states that few cases of chorea and epilepsy occur in young people without betraying a trace of sugar in the urine, the sugar disappearing with the passing off of the nervous attack.

We would add, also, another form of saccharine urine (differing from that of diabetes with diuresis, and which is of a pale colour), where there are not simply traces of sugar, but a large quantity is passed, giving rise to a specific gravity of 1040. The quantity of the urine in this form is not generally much, if any, above the normal amount; while, on the other hand, the colour is not unfrequently deeper than that of health, nor is there, as a general thing, marked thirst and emaciation. Again, this form of disease occurs in persons from fifty years and upwards, and is generally tractable to treatment.

One remark of the author, in connection with this subject, we must be allowed to question, viz., that "when the specific gravity of the urine is above 1035," sugar undoubtedly exists. We have more than once had urine of a specific gravity of 1038, and in one case it was as high as 1040, and associated with diuresis, and yet no sugar was present. The high specific gravity in these cases was owing to urea and extractive matters.

The work, as the author remarks, is confined to the consideration of the diseases arising in or especially manifested by the organs in which the urine is elaborated, and temporarily retained, and the passages through which it flows, so that we do not find any notice of the specific affections of these organs, nor of

diabetes, the diseases of the prostate gland, or of Cowper's glands. A full account, however, is given by the author of the anatomy, physiology, and pathology of the supra-renal capsules, which, perhaps, have as little to do with the physiology of the urinary organs as the prostate gland, and certainly their relations are much less important in a pathological point of view.

After some general considerations of the difficulties in the diagnosis of the diseases of the urinary organs, and having made mention of the means which science now offers to our aid, the author passes to the enumeration of these organs, their "anatomical relations," their malposition, and its effects upon diagnosis.

In the same chapter he draws attention to the importance of distinguishing between mere functional disturbance and more serious disease of the urinary organs, and, that mistakes in diagnosis may not occur, he forewarns the practitioner to bear in mind, in his examination of a case, "the normal anatomy of the organs involved; their unusual distribution; their physiological relations; the atmosphere, and its changes; the food and drink, medicinal agents, mental emotions, restrained functions, &c."

Having arrived in "*medias res*," the author commences with the consideration of the supra-renal capsules, from which we gather that the diagnosis of their diseases is very unsatisfactory, and their physiology unsettled. Brown-Séquard, the author says, "advances the hypothesis that the function of the supra-renal capsules is to prevent the deposition of pigment in the blood."

In the account of the pathological conditions of these organs no mention is made of oily degeneration of their tissue, which condition we have seen in one case.

The most interesting part of the work, perhaps, is that relative to diseases of the kidneys. Here we find nothing very new, but quite a full exposition of the knowledge of the day on this subject. The classification of Dr. George Johnson has been adopted, into acute and chronic desquamative nephritis, waxy degeneration, non-desquamative disease, and fatty degeneration of the kidneys.

It is unnecessary for us to enter into an analysis of the subjects here treated, as the ground has already been so thoroughly gone over by Dr. Johnson, in his work on the diseases of the kidneys.

Bright's disease, according to the author, is synonymous with fatty degeneration of the kidneys.

The fatty condition is, again, subdivided into two conditions—the "mottled fatty kidney" and the "granular fatty kidney." These two conditions are distinct forms of disease, and not different stages of the same affection, and, according to Dr. Johnson, have each a distinct history. "There is," says the doctor, "yet another and important distinction between the granular and the mottled form of fatty degeneration. In the first mentioned form of disease the formation of the fatty granulations is preceded by a non-desquamative disease, this being indicated by the condition of the urine, which is highly albuminous, clear, and without sediment; whereas the second form of fatty degeneration comes on very gradually and insidiously, and frequently makes great progress before the urine affords any indications of the existence of renal disease. I have occasionally found a large proportion of fatty matter in the renal epithelium after death, in cases which have certainly not been attended by the secretion of albuminous urine."

These two conditions of the fatty kidney are synonymous, according to our author, with "Bright's disease," the *néphrite albumineuse* of Rayer, the *hydropisie rénale, albuminurie* of Martin Solon, and the granular degeneration of the kidney of Christison.

In the treatment of these different affections of the kidney, the author agrees with most of the writers of the day on this subject, in not advising the administration of diuretics, and speaks highly of the usefulness of active purgation with elaterium, especially where there are urgent brain symptoms, and refers to the prompt relief which follows this treatment under such circumstances.

We can also add our mite of testimony in favour of the above statement, and can never forget the prompt manner in which extreme dyspnœa, caused by effusion into the chest in a case of severe albuminuria, was relieved by obtaining free purgation by the use of elaterium, notwithstanding the patient seemed to be in a condition to contraindicate its use.

The various other diseases of the kidneys, of the ureters, of the bladder, including cystic calculus, and the affections of the urethra, are all fully and judiciously treated of in the remainder of the work.

In the article on vesical calculus, Dr. G. Owen Rees's views in relation to the singleness of diatheses are quoted, the uric being the only true diathesis. The phosphatic and oxalic are but modifications or effects of it. The oxalates of lime, according to the doctor, are produced outside of the body, and the deposit of phosphates by a diseased condition of the mucous membrane of the urinary passages.

The author does not seem to believe that hereditary predisposition has any influence in the production of calculi; and while we do not either believe that hereditary predisposition has much to do in moulding the material into a stone, yet it must have considerable influence in originating that condition of system which gives rise to deposits of the matter out of which they are formed. J. D.

ART. XIX.—*Illustrations of Typhus Fever in Great Britain, the Result of Personal Observations made in the Summer of* 1853, *with some Remarks as to its Origin, Habits, Symptoms, and Pathology.* By J. B. UPHAM, M. D. Formerly Assistant Physician to the Hospitals connected with the House of Industry at South Boston, and at Deer Island. From the Boston *Medical and Surgical Journal.* 8vo. pp. 46. David Klapp: Boston, 1858.

IN 1852, Dr. Upham published a series of observations on "Maculated typhus or ship-fever, collected during the prevalence of the disease at South Boston and Deer Island Hospitals, in the years 1847–48, among, chiefly, the immigrants from on board the crowded, and often badly provided passenger ships arriving from different ports in Europe."

This publication was noticed in the April number for 1853 of this Journal. It presented a very excellent history of typhus fever, in its different degrees of intensity, with the varying phases, complications, and sequelæ, manifested by it during the epidemic in question; accompanied with such views of the pathology, and treatment of the disease, as had been acquired from careful observation at the bedside and in the dead-house.

During the summer of 1853, Dr. Upham was presented with a very favourable opportunity of prosecuting his researches in respect to typhus fever, in the city of Dublin, as well as in the wards of the London Fever Hospital, and of comparing the disease, as it presents itself in its indigenous haunts,with its manifestations and habits as an exotic, in this country. In the work before us, Dr. Upham gives from these European researches a description of the fever, intended as a portrait of it as it presents itself in the wards and dead-house of a fever hospital of Great Britain.

As models of the disease, in its various forms of severity, as it is there met with, Dr. U. furnishes the minutely detailed histories of seven cases, selected from the practice of Drs. Tweedie and Southwood Smith. The broad and general statement of the facts developed by these cases, as examples of the ordinary forms of typhus fever, "in its mild, moderate, severe, and fatal" varieties, may, according to our author, be thus stated: "It is an affection sudden and severe in its accession, originating mostly in the densely populated and poverty-stricken portions of the larger cities and towns of England, Scotland, and Ireland; traceable, in a majority of cases, on the part of the patient, to a more or less immediate intercourse with the sick; common to all ages, and both sexes; ushered in by lassitude, depression, rigors, anorexia, headache, pains in back, limbs, and joints, accompanied, or soon followed, by loss of strength, dulness of the intellect and special senses; perversion of memory; stupor; hot and pungent skin, dusky, moist, or dry; flushed face; suffused eyes; furred and loaded tongue; accelerated, but moderately full, soft, compressible pulse; without any considerable deviation —in its simple, uncomplicated form—from a normal condition of the chest and

abdomen; general sensitiveness of surface; a strong, peculiar, nauseous odour of the body; exhibiting, on or about the fifth day, an abundant, characteristic rash, first seen upon the arms, upper part of chest, and legs, later on abdomen and back, never on the face—the approach of which is previously heralded by an indistinct, mottled, and roseate appearance of the surface, seemingly subcuticular —which rash is at first light-pinkish, florid, isolated or clustered, simulating not unfrequently the eruption of measles—then darker, more or less persistent, spreading, increasing in abundance and intensity for several days, sometimes livid, petechial, fading on or about the tenth day, and disappearing in the order in which it came, from about the twelfth to the sixteenth day; which symptoms may vary in severity and relative importance, may vacillate from better to worse, from worse to better, or remain stationary, or diminish in intensity, till they are merged in convalescence; or may be aggravated and receive accessions—the tongue become dry, swollen, fissured, black, with accumulations of sordes on the teeth and lips; injected eyes; fuliginous face; burning skin; livid and petechial spots; hurried, interrupted, imperfect respiration, accompanied by sighs and moans; dulness at lower posterior part of chest on percussion; an exceedingly rapid, feeble pulse; extreme muscular prostration, but with momentary exhibitions of unnatural strength; coma vigil, or great nervous agitation, simulating at times the busy excitement of delirium tremens; with sometimes coolness of surface and profuse sweating; terminating at a variable period, between the tenth and twentieth day, often earlier, rarely later, in death; the *post-mortem* examination disclosing, externally, much discoloration of depending and posterior parts—internally, the absence rather of any considerable organic lesion, but commonly evincing more or less abnormal vascularity of the brain and its membranes, its substance being firm and natural, the bloody points on its cut surface numerous, distinct, and dark—with oftentimes slight increase of serum beneath the arachnoid, and in the ventricles, clear or turbid; lungs externally normal—internally normal anteriorly —the posterior and depending parts more dense and engorged; lining membrane of the bronchia reddened, stained, not usually injected; heart soft, flabby—its contained blood dark, fluid, dissolved, sizy—with loose, non-coherent clots in the meshes of its valves; viscera of abdomen normal, with the exception of discoloration, and sometimes simple congestion of the mucous lining of the small intestines—occasional softening of the spleen, and general fluid, sizy, disorganized condition of the blood throughout the body—the sum and substance of which symptoms, facts, and circumstances is represented under the conventional term of *typhus*."

Interesting as are all the details embraced in the publication before us, truthful as are the author's delineations of the disease of which he treats, and deserving as they are of an attentive study by all who would make themselves familiar with the etiology, pathology, morbid anatomy, and treatment of a form of fever, the prevalence of which, we regret to say, has of late years, increased in frequency and extent, in the dark, filthy, ill-ventilated, badly drained, and over-crowded courts, lanes, and alleys, where, in all of our larger cities, dwell the improvident, the destitute, and the vicious portions of the community, amid want and wretchedness, and secluded, in a great measure, from the cheerful sunlight, and wholesome air; still, we discover in the illustrations of Dr. Upham nothing particularly novel, either in relation to the causes, nature, characteristics, progress, or treatment of typhus fever.

Dr. Upham assigns as the originating causes of the disease, foul and stagnant air, and the damp, dark, filthy, and crowded habitations of poverty, deprivation, and misery, he, nevertheless, assigns to it, an "eminently infectious and contagious character." He admits, however, that the disease should not be held as contagious in the same sense that smallpox is contagious—that is, invariably and virulently so.

"Certainly the sphere of action is more limited—the communication of the poison more dependent on circumstances—and the morbific influence more within the control of sanitary laws and regulations, than in the usual zymotic or so-called contagious maladies. It may be stated as a general rule, that the contagion, to be effectual, must be concentrated by the crowding together of patients—or accumulated and aggravated in ill-ventilated and pent-up rooms—or stimulated by

the conjunction of other unfavourable hygienic conditions, ill-drainage, filth, effluvia, &c. &c.—or the recipient have been previously subjected to the predisposing causes by deprivation, hardships, and want, excesses, anxiety, fear, despondency, mental and physical exhaustion or debility from any cause, till his system has been brought to a point below the power of resistance.

"It follows that immunity from the reception of contagion in the exposed, and from an aggravation of horrors on the part of the sick, is to be gained, as far as possible, by a strict observance of the well-known maxims of hygiene, first and foremost among which is the possession of a stout heart and sufficiency of the light and air of heaven. Hence an explanation of the fact that, in the outbreak of the fever in 1847, when sheds and shanties open to the elements were of necessity used, in Dublin and elsewhere, both patients and attendants fared the better.

"As is well known, the disease is often epidemic, prevailing extensively, as already stated, in some districts, towns, and localities, while absent in others; and raging and overspreading the country in certain seasons and years. These last are heralded mostly by some wide-spread calamity, involving misery and suffering and general want. At such times multitudes of the most destitute flock to the metropolis and the other great cities of the realm, in search of food and employment, carrying with them a predisposition to the fever, stopping for shelter in the filthiest and most wretched abodes, sowing therein the seeds of disease, and, then, speedily finding their own way into hospitals to die. In the famine year of 1847, the fever was thus engendered and disseminated to a frightful extent. The baleful influence extended into the following, and, conjoined with the cholera, even the next succeeding year. This may be called the great epidemic triad of modern times. It was then that the flood overflowed its natural bounds and poured its surplus waves of fever for the first time upon the shores of the New World." D. F. C.

ART. XX.—*Catalogue of the Surgical and Pathological Museum* of VALENTINE MOTT, M. D., LL. D., Emeritus Professor of Surgery in the University of the City of New York, etc., and of his son, ALEXANDER B. MOTT, M. D., Surgeon of St. Vincent's and the Jews' Hospital. *Secat Salubritar.* New York: 1858. 8vo. pp. 78.

IN this catalogue, over a thousand pathological specimens, the result chiefly of surgical operations performed by the Nestor of American surgeons, are enumerated and described. It will, naturally, be most prized by students attending the University of the City of New York, who can find therein an explanation of the specimens contained in the museum to which they have access. As, however, remarks are frequently appended to the description of the specimen, and, in many instances, the whole history of the case by which it was furnished, the catalogue possesses greater interest for the profession at large than might at first be supposed. Several of the specimens, also, are from cases whose history has been published at length, in past years, in this journal, and with which its readers are all more or less familiar.

This collection is believed by Dr. M. to be the largest that any American surgeon has had occasion to form, and he further states in the preface, that "more than fifty years of active professional life, in the most populous city in this country, have been spent in its accumulation."

In order that the specimens might be arranged according to the diseases they represent, and thus facilitate reference, the collection is divided into forty compartments. Owing, however, to the progressive manner in which it has been accumulated, their classification is by no means systematic. Interspersed among the specimens in the different compartments, are pieces of bone from Waterloo, Antwerp, Delphi, and the Acropolis—crania from battle-grounds, from Indian women, warriors, patriots, robbers, and pirates, and such little luxuries, or puerilities, of a pathological cabinet. These might have all been placed in one compartment, along with some bones of an intrepid chanticleer, about whose history

some curiosity must naturally be felt on seeing them in this collection; they may be the remains of the "tutelary emblem, of glossy black plumage" once sacrificed by the professor to Esculapius.[1]

The number of the specimens of *spontaneous gangrene* preserved in this private museum is quite remarkable; it is no less than thirteen. One of these is of the upper extremity, where its occurrence is exceedingly rare, so much so that the account furnished in the catalogue will be given here.

"No. 810. Spontaneous mortification of the right arm of a lady in Brooklyn.

"She was about 35 years old, the mother of several children; rather delicate; no known cause for it. The first thing noticed was unusual coldness of the hand. The mortification gradually advanced to about the middle of the arm, where it stopped, and in great measure separated from the living, healthy surface above. At her urgent solicitation, but without my recommendation, I amputated the arm, as the fetor from it was intolerable to her. The brachial artery was solid, and did not require a ligature. Several smaller branches were tied.

"Four or five days after, upon opening the stump, mortification was apparent in it. She died in a few days, from exhaustion"—p. 14.

At page 72, Dr. Mott says: "I have operated successfully in several cases of spontaneous mortification, but have also failed. My experience is in favour of amputation through the thigh, even though the toes only should be involved in the mortification. The greater the distance from the seat of the disease, the more probability is there of the arteries being in a normal state."

From these thirteen cases, it is also seen that Dr. Mott performs amputation while the mortification is advancing up the limb. This proceeding is condemned by most surgeons; many of whom reject it in almost all cases, even when the gangrene has ceased to progress. Some most trustworthy statistics show, that, of eight cases of spontaneous gangrene in which amputation was performed, five died and three recovered; while of eleven cases left to themselves, only one died, and ten recovered.

In some remarks made upon a case of *fungus hæmatodes* of the right eyeball (p. 39), it is advised, in addition to extirpating the eye, to tie the common carotid artery of the affected side, as the plan "best calculated to postpone, and sometimes prevent, the return of the disease." In this case, no return had taken place when the patient was last seen, which was two years after the operation. This patient, it should be added, was a female child about seven years of age.

Specimen No. 426 is a very curious one—*absence of the epiglottis.* It was discovered after death. "The absence of the cartilage is complete, and, as there is no cicatrix visible, it is probable that the defect is congenital. There was no suspicion of its existence before death"—p. 26.

Nos. 810 and 478 are specimens of *scirrhus of the male breast.* "The tumour was characterized by extreme hardness, lancinating pains, and great retraction and puckering of the nipple—the features of true scirrhus!" In neither was there any return of the disease. A case of *encephaloid* disease of the same organ is also in the collection, No. 755. "The mass of disease contained several cysts, which, when cut into, discharged an inky fluid, leaving a dark stain on the linen—melanotic infiltration." In this, likewise, there was no return.

At page 8 we find: "No. 741. Testicle (healthy). Removed from Dr. L. L., at his own request, as a cure for onanism. After recovery from the operation, he resumed the habit, but not to the same extent as before."

We hope, for the credit of American surgery, that this may ever be an unique specimen.

At page 34 is recorded a case of *malignant sarcoma,* which shows strikingly the obstinacy with which such affections return. The patient was a man of excellent constitution and general health. The disease was situated in the side. Most of the operations were very extensive and extremely painful. At no time, in any of them, was there the least morbid portion left, and the wound invariably healed in the most kind and rapid manner. This is the record of the different operations:—

[1] See Travels in Europe and the East, by Valentine Mott, M. D. New York, 1842, p. 297.

In Ireland	June, 1832,	By Dr. Horphlin.	
	May, 1837.	" Dr. Herbert Orphen.	
	August, 1840.	" " "	
	April, 1841.	" " "	
In New York	June, 1843.	" Dr. Mott.	
	June, 1847.	" "	
	May, 1851.	" "	
	Jan. 7, 1853.	" "	
	August, 1853.	" "	
	Jan. 7, 1854.	" Dr. A. B. Mott.	
	May 26, 1854.		
	Oct. 25, 1854.	By Dr. A. B. Mott.	
	Jan. 30, 1855.	" "	
	Mar. 9, 1855.	" "	
	June, 1855.	" "	

The patient died of the disease, in the fall of 1855. W. F. A.

ART. XXI.—*Description of a New Midwifery Forceps, having a Sliding Pivot to prevent Compression of the Fœtal Head; with Cases.* By GEORGE T. ELLIOTT, M. D., Physician to Bellevue Hospital, the Nursery and Child's Hospital, and the Lying-in Hospital, etc. etc. 8vo. pp. 24.

"THERE is no end," said the wise king, "to making of many books," and, with equal propriety, it may be said, there is no end to the invention of new obstetrical forceps. Nearly every obstetrician believes himself competent to improve the instrument, by giving to it a new or additional curvature, by modifying its form, by changing the shape or breadth of its blades, or by adding to it some contrivance having for its object to facilitate its introduction and adjustment, or to prevent any injury being inflicted by it upon the head of the fœtus or the maternal organs. Whether all these are to be considered as improvements, it would be very difficult positively to determine; inasmuch as there is, we suspect, no one who has ascertained, from actual experience, the advantages and disadvantages of the several forceps in use, or which have, from time to time, been recommended as possessing excellencies in which all others are deficient. Each practitioner has a favourite instrument of his own, which, perhaps, is generally condemned by others, and this, perhaps, less from any positive evidence they may possess of its imperfections than from the fact that, being accustomed to the use of a certain form of instrument, they have acquired a facility in its application, and thus become so far satisfied with it, as to render them disinclined to make trial of any other.

That, from the results of increased experience, no very material improvements in the form and general construction of obstetrical forceps have been or can still be devised, we have no right to affirm. We believe they have been and that they may still further be improved.

In the pamphlet before us, we have the description of a new obstetrical forceps, the peculiarity of which consists in the insertion in the handles of "a sliding pivot" (pin?), by which the blades are prevented from being approximated more nearly than is necessary to permit them to simply grasp and retain hold of the fœtal head without compressing it.

According to Dr. Elliott, "the most that can be expected of an instrument is, that it successfully meets very numerous indications." He believes—

"1. That the principal use of the forceps, in the immense majority of cases, is that of a tractor alone; and that compression is always in some degree injurious, and to be avoided if possible.

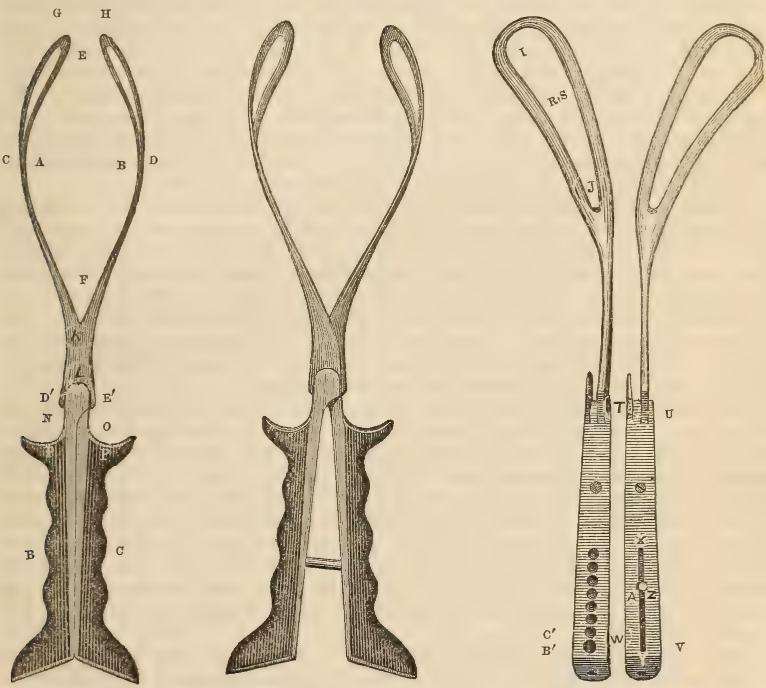
"2. That this traction can be applied, and should be applied, by competent men, in well selected cases, even though the head so float above the brim as to be only capable of being steadied by the hand, introduced above the pelvic brim;

and that thus the class of forceps known as short forceps would not, in these cases, meet the full requirements of the art.

"3. That this traction can be applied, and should be applied, by competent men, in well selected cases, through an os uteri, as yet barely dilated sufficiently to admit the blades separately, and that delivery may subsequently be effected by dilating, or lacerating or incising the os and cervix uteri; and that, in many of these cases, the neglect of this procedure entails loss of fœtal life, demands the perforator, or perils the mother's life by delay, based on ignorance of the full capabilities of the instrument.

"4. I believe in the existence of a large class of cases, in which a light and slender forceps can simplify delivery by altering the position of the head—a procedure inoperative and injurious, when performed with instruments of large pelvic curve—while its neglect frequently demands the perforator, or makes the difference between a safe operation, or one of the greatest risk, to one or both of the lives at stake.

"5. That a forceps capable of fulfilling all these requisitions must, of necessity, be well adapted to those simpler cases, to which some men would limit their use, rendering them, in the words of Dewees, 'scarcely subservient to the art.'



Whole length of forceps . . .	$15\frac{1}{4}$ inches.
A to B	$2\frac{11}{16}$ inches, face of forceps.
C " D	$2\frac{3}{4}$ " back "
E " F	$6\frac{3}{4}$ "
G " H	$\frac{3}{4}$ "
I " J	$4\frac{3}{4}$ "
K " L	$1\frac{3}{4}$ "
L " M	$6\frac{3}{4}$ "
N " O	$\frac{5}{8}$ "
P " Q	$1\frac{1}{8}$ "

R to S	$\frac{13}{16}$ inches, centre of fenestra.
T " U. . . .	$\frac{5}{8}$ "
V " W . . .	$\frac{7}{8}$ "
X " Y . . .	$2\frac{3}{8}$ "
Z " A′ . . .	$\frac{1}{8}$ "
B " C. . . .	$1\frac{1}{2}$ "
Length of pin .	$\frac{7}{8}$ "

Thickness of blade R S $\frac{1}{8}$ of an inch, a little thinner at the end.

N. B.—B′. This hole is sufficiently deep and wide to receive the entire pivot when it is not needed. C′ is a little deeper than those above, so that the distance between the handles may be nicely graduated by the pivot.

D′ to E′. These overhanging projections serve to prevent any risk from slipping of the lock when the handles are widely separated.

"Thus the forceps now presented is made as light as is consistent with strength, with its diameters as small as the indications given above would seem to warrant; while the introduction of the *pivot* allows handles long enough to be grasped by both of the operator's hands, without fearing risk to the child from pressure, either when the fœtal head is large, or in those operations where it is utterly impossible now, and must ever be utterly impossible, to seize the head in any other way than by its oblique diameters." D. F. C.

ART. XXII.—*An Essay on the Pathology and Therapeutics of Scarlet Fever.* By CASPAR MORRIS, M.D., Fellow of the College of Physicians of Philadelphia, etc. etc. etc. 8vo. pp. 192. Philadelphia: Lindsay & Blakiston, 1858.

A MONOGRAPH on scarlet fever, embracing the result of more than thirty years' observation of the disease by the author, in various public institutions as well as in private practice, cannot but be hailed by every practitioner as an acceptable addition to his means of instruction in respect to the nature, phenomena, and treatment of one of the most formidable of the diseases he may be called upon to treat. A disease which in its more severe forms has, heretofore, almost invariably marched on, with greater or less celerity, to a fatal issue, in despite of the most skilfully directed efforts of the physician to stay its destructive course, and which, even in cases where the patient struggles through the attack, and is permitted "to linger into life," leaves behind it chronic lesions of important organs, "equally, perhaps more, to be dreaded than the primary disease."

The essay of Dr. Morris claims to be such a monograph. It is an enlarged and amended edition of an essay published by him some years ago, and which was most favourably spoken of by such as had occasion to consult its pages. The work, in its present form, though somewhat deficient in method, and in many parts unnecessarily diffuse, presents a very fair, accurate, and satisfactory exposition of the actual state of medical opinion in respect to the pathology and therapeutical management of scarlet fever, in the various degrees of intensity, and with the different modifications and complications under which it presents itself, in the same or in different epidemic visitations.

We cannot say that we find in the work of Dr. Morris, any more accurate delineation of the disease; any clearer views of its nature, nor any more judicious and successful plan of treatment, than is contained in any of our leading systematic treatises on the practice of medicine, general or special. Still the publication of the essay is to be approved, inasmuch as it furnishes us with a statement of views in respect to the nature and management of a most important malady, the accuracy of which the author believes to be confirmed by a long and extensive experience. The comparison of views thus tested, with those derived by ourselves from our own field of observation, is an important means of enlarging and perfecting our acquaintance with disease, and perfecting our skill in its successful control.

Our own experience differs from that of the author in some few particulars. Thus, we have seen no facts to convince us of the contagiousness of scarlet fever; nor can we concede to the evidence adduced by the author and by others, so conclusive a character as he claims for it. It would but illy stand the test of a rigid logical scrutiny. We cannot conceive of a disease so eminently contagious as to be contracted, like, we are told, scarlatina may be, by simply looking upon an entirely new garment that had been sent, some distance, from a house in which the disease was prevailing, occurring as an epidemic, within such narrow limits as a single block of houses, while all the neighbouring square remained free from it, or, as we have more than once known to be the case, visiting every house on one side of a narrow street, while on the opposite side, not a single case occurred.

In the management of scarlet fever Dr. Morris advocates a mild and soothing or a sustaining tonic, and stimulating treatment; in this he is borne out by some of the best authorities in the profession, and in the mild, simple form of the dis-

ease, as well as in the low and malignant type, under which, in some of the epidemical visitations it occurs, the treatment recommended by Dr. Morris is confessedly the only one calculated to conduct it to a favourable issue.

That cases of scarlatina do, nevertheless, occur, in the early stage of which a judicious resort to direct depletion by the lancet or leeches, will be found decidedly beneficial, we have been forcibly taught by experience. We are convinced that, in the sthenic, or open inflammatory form under which the disease not unfrequently presents itself, the abstraction of blood, at the proper period, and to a proper extent, is a measure adapted not only to conduct it more certainly to a favourable close, but to prevent the occurrence of most of the serious sequelæ incident to the disease, more especially when it is neglected or mismanaged.

D. F. C.

Art. XXIII.—*An Essay on Inflammation: the tenth of a Series of Articles published in the St. Louis Medical Journal, on "Life."* By J. H. Watters, M. D., Professor of Physiology in the St. Louis Medical College. 8vo. pp. 32. St. Louis, 1858.

That vital phenomena are never exhibited excepting in organized matter, and every vital action, process, or movement is attended with a metamorphosis, disintegration, or decay of the tissue in which it occurs, are propositions the truth of which is admitted we believe by all physiologists. In an essay published in 1851, and noticed in a former number of this journal (for July, 1852), Dr. Watters attempted to show that organization is a form impressed upon matter by the Creator, in order to convert matter into a machine, by means of which advantage is taken of the laws with which he has endowed it, to produce certain results—vital actions—vital phenomena; and that these actions or phenomena result necessarily from the peculiar form or arrangement of the matter constituting the organism, when this is placed under the proper conditions, and that all the processes, actions, or functions of a living organism are the effects of forces which are evolved in its decomposition or decay.

These views, first publicly announced by Dr. Watters in 1851, have since then been further elucidated and enforced by him, in a series of articles that have appeared from time to time in the *St. Louis Medical Journal.*

In his first essay, Dr. Watters endeavoured to deduce confirmatory evidence of the truth of his theory of life by applying it to an elucidation of the phenomena of inflammation; the explanation of the pathology of inflammation there laid down is, in its general outlines, and even in some of its details, proposed by Dr. Hinton, in an article published by him in the number of the *British and Foreign Medico-Chirurgical Review* for July, 1858, as one which is both clear and satisfactory, and by which the facts connected with the phenomena and course of inflammation are better co-ordinated than has been done by any other theory of the disease. The views advanced in the article of Dr. Hinton to which we refer, he assumes as original with himself; not the slightest reference being made to the previous publication of Dr. Watters. We would not be understood as accusing Dr. Hinton of plagiarism. Notwithstanding the very remarkable similarity between the theory of inflammation as set forth by him in the paper referred to and that publicly announced by Dr. Watters seven years previously, it is very possible that it was developed in the mind of Dr. Hinton without any knowledge of the essay of the former gentleman, and by a course of reasoning commencing from a very different and dissimilar starting point. And this fact of the almost entire identity of the general idea of life, or vital action, upon which is based the pathology of inflammation advanced by both these gentlemen, and arrived at by each entirely independent of any knowledge of the other's labours, may be assumed, we think, as a pretty certain evidence of the value of that idea, and of its foundation in truth. But, whatever may be the estimate we may be inclined to place upon the doctrine of vitality here referred to, the claim

to priority in its announcement and elucidation must be decided in favour of Dr. Watters.

"With Dr. Hinton," Dr. W. remarks, "the position that the increased action of inflammation is the *effect* of increased decay, is an induction; with me, the position that the phenomena of inflammation depend upon a disproportion between decay and nutrition, is a deduction from the general proposition that decay is the vital motor, and this from the more general one that Form and Motion are the primary conditions of every mode of motion in physical nature. Hence, the question considered by me is, upon the supposition that decay is the vital motor, Can the phenomena of inflammation be explained? If so, then this deduction is a strong verification of the general proposition. If the phenomena of inflammation justify the induction of Dr. Hinton, how much more do they justify this deduction! For a class of facts which alone might not be sufficient for an induction, might be quite sufficient for a deduction, to confirm a position already arrived at by induction from other facts. But Dr. Hinton thinks the facts of inflammation are sufficient for an induction; in fact, by induction he arrives at the same theory of inflammation which I did by deduction. What greater verification than this could there be? And he presents the same class of arguments, too, as sufficient for the induction, which I presented in my thesis to establish only a deduction. This is really a greater verification than I expected so soon."

We do not intend to enter into an examination of the actual force and validity of the arguments presented by our author in support of the doctrine of life and vital action advanced by him; nor shall we attempt any comparison between that doctrine and the generally received proposition that living organized matter is endowed with a certain property or force, upon the presence of which all its vital acts and phenomena, abnormal as well as normal, are dependent, with the view to test which is the best adapted to explain the functions of the organism in their healthy state and the phenomena of disease.

Our general opinion of the views advanced by Dr. Watters, and the manner in which his exposition and defence of them has been executed, was expressed on a former occasion when his original essay was under consideration. Both hypotheses, that of a specific independent vital principle connected with organized matter, and that which views vital action as the result simply of the disintegration or destruction of organized tissues, are unquestionably attended with serious difficulties as exponents of the vital movements of the human organism in its physiological or pathological conditions. Should, what many of those whose opinions on questions of physiology are received as of great weight assert, be true, that, namely, the disintegration or destruction of tissue is an event always consequent upon and never preceding vital action, we would then be scarcely warranted in setting up the disintegration or decay as the motor of vital action. While, on the one hand, the mere precedence of decay or destruction of tissue to any given vital act would by no means be sufficient of itself to prove that the decay or destruction is the cause or motor of such act, on the other hand, the fact of the change or decay of tissue being invariably sequent to vital action, is almost conclusive evidence that it cannot be received as the cause or motor of the latter. We admit that, in investigating this question, we must be cautious not to be led astray by confounding the visible resulting consequences of disintegration or decay of tissue with the actual process of decay itself.

In the essay before us, Dr. Watters has endeavoured to remove some of the presumptions that may be entertained against the truth of his theory of life, while he presents some additional evidence in proof of the correctness of the views of inflammation deduced by him from it.

These views, arrived at as they have been, by two shrewd minds, without any knowledge of each other's labours, cannot but be worthy of the consideration of every one in the pursuit of physiological and pathological truth. D. F. C.

ART. XXIV.—*The Pathology and Treatment of Stricture of the Urethra, and Urinary Fistulæ.* By HENRY THOMPSON, F. R. C. S., M. B., Lond., &c. Second edition, much enlarged and revised. London: John Churchill, 1858. Octavo, pp. 426.

THE first edition of this work, published in 1854, was reviewed at length in this Journal (July, 1855). It remains for us, therefore, but to point out the changes which have been made in this second edition.

The author has introduced in the body of the work much of what was formerly contained in the appendix; sometimes in the form of foot-notes, and, when it is illustrative cases, at the close of an appropriate chapter. A slight alteration has been made in the order of the chapters, the one treating of urinary abscess and fistulæ now preceding the one upon retention of urine. The historical portion of the subject has been everywhere improved; old works are referred to for the first time, and mention is made of several works of merit—as those of M. Guérin and Mr. Henry Smith—that have recently appeared; enlarged experience has also enabled the author to furnish new cases in illustration of his teaching. An alteration we regretted to encounter is that of indicating the subject of the paragraphs by printing it in large type at the commencement, in place of small type at the side on the margin of the leaf. For reference, the plan adopted in the former edition is decidedly preferable. A few new wood-cuts have also been introduced. These are the changes made in the general arrangement of the work. We shall proceed now to notice those made in its several chapters.

On the subject of anatomy, the presence of the rugæ so numerous in the bulbous and membranous portions of the urethral canal is said to be owing to the existence of numerous long and slender bands of fibrous tissue lying immediately beneath the mucous membrane. The venous circulation in the urethra is described for the first time; and in connection with that much vexed subject, the muscle of Wilson, there is an alteration in the text, which reads "muscular fibres descending from the pubic symphysis and adjacent bone to the membranous part of the urethra," instead of "to the side of the prostate, and towards the urethra, just anterior to it, the latter especially not being constant in quantity in different bodies, in some being little if at all developed." The observations of Professor Ellis (*Med.-Chir. Trans.*, vol. xxxix.), to which reference was made, in a notice made of Mr. Thompson's work on the prostate gland, in the number of this Journal for April last, have also been made use of to more fully complete the anatomical descriptions.

In treating of the classification and pathology of stricture, attention is called to the fact that, in laying open the urethra after death, transverse bands, encircling the urethra and narrowing it, are divided so that the stricture is often less obvious than was anticipated. According to their anatomical characters after death, organic strictures are classified as linear, annular, indurated annular, and irregular or tortuous strictures. These terms explain themselves, the distinction between linear and annular being understood to be the greater breadth of the constricting band in the latter form. Mr. Thompson is less inclined than before to believe in the existence of stricture from a deposit of false membrane upon the urethra, and cites with satisfaction the corroborative opinion of M. Guérin on this point in pathology. The most frequent seat of stricture is no longer believed by him to be at the junction of the spongy and the membranous portions of the urethra, but the portion comprised in the inch anterior to the junction—that is, the posterior or bulbous part of the spongy portion. We also notice (p. 61) that Mr. Thompson has encountered a second impermeable stricture (properly so called).

To the account given of the symptoms and pathological effects of organic stricture not much has been added. Retention of urine is noticed as being in some cases the most prominent symptom throughout; and also, in others, a tendency to rapid recontraction after dilatation, giving rise to the form called by Mr. Syme "the resilient stricture." According to their prominent pathological tend-

ency, organic strictures are classified as simple, sensitive or irritable, and contractile or recurring (resilient of Syme).

Nothing has been changed in the chapters treating of the causes of organic and permanent stricture, and of the pathology of strictures which are only of transient duration.

The chapter upon the diagnosis and treatment of stricture of the urethra, and the employment of dilatation, has been much improved. Mr. Thompson recommends now, in order to explore the urethra with a sound, that the patient be placed in a recumbent position, and gives minute directions for the guidance of the practitioner. He declares, also, that he can speak highly of the utility of bulbous sounds in making this exploration in stricture, as enabling the surgeon to ascertain with accuracy the locality and extent of the contraction. In passing a sound for the purpose of dilating a stricture, attention is called to the unnecessary length of time the instrument is often allowed to remain in the bladder; in most cases Mr. Thompson believes that as much benefit will be obtained by removing it at once as by permitting it to remain. We find that he has changed his mind in regard to using instruments to overdistend the canal chiefly at the contracted part, and now states it to be undoubtedly sometimes advantageous. In regard to instructing patients to pass the catheter, in place of "some few" we now read that "a considerable number" should be thus instructed. The steel conical sound is still a favourite instrument with Mr. Thompson, his armamentarium consisting now of six, in place of three, which formerly constituted for him an efficient set. When a very small instrument must be used, of course very little oil can be made to adhere to it; and just as the instrument diminishes in size, so does the presence of the oil become increasingly necessary. The simple method of applying the oil *to the urethra itself*, and very freely, rather than to the instrument, is recommended; by means of a common glass syringe, about half an ounce of pure olive-oil is injected slowly into the urethra. Mr. Thompson states that he has obtained considerable advantage from the use of oil in this manner. The necessity that exists of using great care in carrying the point of the instrument through that part of the canal which lies *behind* it, on account of the irregular character of the urethral walls, not uncommon in old and tight strictures, is insisted upon. In cases where it is found necessary to tie a catheter in the bladder, it is recommended to adopt the plan of fitting to the orifice of the catheter a bent tube or siphon containing a stopcock. This prevents completely the flow of urine downwards along the outside of the shaft of the instrument when the patient occupies the recumbent position. "Excoriation of the skin is thus prevented, and that wetting of the linen and bedclothes, which is exceedingly disagreeable and troublesome, indeed mischievous, when the catheter has to be retained for a considerable period." The part of this chapter occupied with the constitutional treatment of patients with stricture has been greatly improved, and we should also notice that the *rationale* of the action of dilatation is discussed therein for the first time.

The chapter on the employment of chemical agents in the treatment of stricture has been very little altered. In the old edition he stated that "some of the modern French surgeons use nitrate of silver largely," while in the present one he says, more correctly, that "it is only fair to our French brethren to state that the use of caustics is now almost universally reprobated by them," a form of expression that conveys a more correct idea of the French practice. We have noticed throughout the work, in many places, evidences of further acquaintanceship with French writings and French practice than Mr. Thompson gave evidence of possessing in the first edition of his work. It is this, probably, that has induced him to regard more favourably the treatment of stricture by internal incision. After a good deal of personal attention to the subject, and some experience of the methods adopted, he expresses himself as satisfied that internal urethrotomy offers a very successful means of dealing with certain intractable examples of the complaint. The proceeding adopted by Civiale appears to him to be one of the best. The following conclusions, which form a summary of the chapter upon internal urethrotomy, will show the change in his views:—

"*Internal urethrotomy* is indicated in almost all strictures affecting the external meatus of the urethra; and in many cases of stricture situated about the

middle of the spongy portion, for which dilatation, fairly tried, has proved unsuccessful.

"It is useful, also, in some few cases of stricture situated at the bulbous portion, which are not relievable by dilatation; a single incision, which is not deep, being free from danger, and frequently rendering the stricture perfectly amenable to dilatation afterwards.

"Lastly, it is so in those rare cases in which the urethra is narrowed and indurated at many points, or throughout a great portion of its course, dilatation having been found inefficient."

The historical portion of the chapter upon external incisions has been much enlarged. We notice that to Jean Luis Petit is accorded the credit of having first performed a cutting operation for the cure of the stricture, and not for the mere relief of a complication. This was first shown, if our memory serves us, in a paper on Syme's operation, in the *Archives Générales*, by M. Follin. Mr. Thompson has made it his business to write to every surgeon who has performed the operation, and has collected a list of 219 cases by thirty operators. In this list "the total number of deaths occurring within two or three months after the operation, whether due to it or not, is fifteen. One, certainly—two, I think—fairly were not chargeable to it; leaving say fourteen, or slightly over six per cent. But four others were the subjects of advanced disease of the kidneys, and therefore ought never to have undergone the operation, and with our present experience would not have been submitted to it." The mode of performing the operation is much more carefully described in this new edition.

In the treatment of retention of urine depending on stricture, Mr. Thompson in his first edition recommended the employment of chloroform. In this one he speaks of its benefit with still greater confidence and satisfaction. Sometimes, after the ablest hands have failed to pass an instrument, the urine has been spontaneously expelled when the patient was fully subject to its influence.

The chapter upon urinary abscess and fistulæ has been more improved than any in the work. A very proper division of fistulæ is made into simple, indurated, and fistulæ with loss of substance. The cure of the last form of fistulæ—in other words, the subject of urethroplasty—entirely neglected in the first edition, is now treated of at considerable length.

The chapter upon stricture of the female urethra is the same.

We must therefore look upon the changes made by Mr. Thompson in this work—the best of all that treat of stricture—as enhancing considerably its value.

In his preface to this second edition Mr. Thompson states that the most flattering appreciation of his labours was that which appeared in the appropriation of thirty pages of the second chapter by a foreign surgeon, first pointed out in a review of the *Treatise on Stricture*, by Henry Smith, in the number of this Journal for April last. He adds "that it would be impossible for the author to omit this opportunity of presenting his sincere and cordial thanks to the press of this country, of France, and of America (in the latter the fraud was first discovered and exposed), for the prompt and complete manner in which it hastened to lay bare and publish to the world the fraud in question." It is somewhat remarkable that this should be the first public acknowledgment made in England of the fact that this remarkable plagiarism was first pointed out in this country. British reviewers have tried to make it appear that the discovery of this plagiarism was the result of the very extensive acquaintance of English surgeons with continental medical literature.

W. F. A.

ART. XXV.—*Eutherapeia; or, an Examination of the Principles of Medical Science, with Researches in the Nervous System.* By ROBERT GARNER, Surgeon to the North Staffordshire Infirmary, etc., late President of the North Staffordshire Medical Society, author of Papers in the Linnæan and Zoological Transactions, and of the Natural History of Stafford. London: John Churchill.

IT would seem from the appendages to the author's name on the title page, and from one of the chapters in his book, that he has been devoted to a consider-

able extent to researches in comparative anatomy and natural history; and we infer from what is said in his preface that he, in common with many others in our profession, has had his practice lessened on account of these researches, although they are certainly tributary to a knowledge of practical medicine, and the physician who pursues them in his leisure hours adds to his resources in the investigation and treatment of disease. It seems that in England as well as this country it is almost always injurious to a physician's standing with the public as a practitioner, to be known to be engaged in any investigations which have not the most palpable and direct bearing upon his practice. Those generally have the largest practice who are careful to be known only as practitioners of medicine—the idea of the public being that they are so much engrossed in their practice, that they have neither the time nor the inclination to attend to anything else. In many communities such an impression is so essential to success, that it is the general aim of the young physicians to produce it in regard to themselves, before they really have business enough to occupy more than a very small proportion of their time. And we have no doubt that it is good policy ordinarily for one who wishes to push his researches into studies that have not a palpable bearing on the treatment of disease, to conceal the fact that he does so from the great body of his employers. If he does not, they will get the impression that he has so little practice that he has time to attend to other matters, and will think that he is so much interested in them that he will not take a proper interest in his patients. The truth is, that the investigations referred to really make one a better practitioner, not only by adding to his resources, but also by giving him an agreeable relaxation from the toils of practice, thus adding both to the buoyancy and the vigour of his intellect. The physician who confines himself wholly to the drudgery of practice, going through the same routine day after day, both narrows and belittles his mental powers. And he who pretends to do this as a matter of policy, and going about with bustling air, really spends little of thought upon anything, though the world may be cheated into the opinion that he is very skilful, is really but little above the veriest quack that he affects to look upon with such holy horror.

But to the book that we have in hand. It has a singular title, and is somewhat singular in its character. The chief object of the author, as he states it in his preface, is "to demonstrate that considerable reliance may be placed upon the present theories and practice of medicine, bearing in mind, however, that more light remains to be shed on very many medical subjects, and that all human opinions and doctrines are liable to error." In order to do this, he attempts to give a picture of the state of medicine at the present time.

While there are many good things in the book, regarding the book as a whole it is a failure. All the parts of it do not contribute to the end aimed at. The best chapter in the book has the least practical bearing upon the object for which the author writes. In this chapter he gives us an admirable view of what is known of the comparative anatomy and physiology of the nervous system, and we have no doubt that he could write an extended treatise on this subject which would be of great value. It is a subject on which he is evidently at home. The next chapter also, which is on "Physiological and Pathological Chemistry, or Chemistry in connection with the Functions, Changes, and Diseases of our Bodies," is marked by comprehensive views and an intimate knowledge of all the minutiæ of the subject.

In the next two chapters he undertakes to give a sort of running account of what we know at the present time of the nature and treatment of diseases. He goes over too much ground, in too general a way, to give the reader much definite information. The style in which he does it is dashing and off hand, and it would seem that sometimes he hardly is aware of exactly what he is saying. It is on this supposition only that we can account for some passages that occur. For example, he says, "Asthma appears to be a purely spasmodic disease, attendant on some peculiar conformation of the chest, or lungs, or breast." Now he cannot mean that it is either universally or generally attendant upon such conformation, and yet he says so in effect. In noticing delirium tremens, he speaks of there being an alcoholic poison in the blood, and of ridding the system of it by brisk purgatives as one great object in the treatment of it.

Chapter 6, "On Medicinal Agents and their Classification," is a short one, and contains nothing worthy of special notice.

Chapter 7, is "On the Divine Dispensation in Disease." There is much that is valuable and interesting in it, but we do not see that it is exactly pertinent to the avowed object of the book. Indeed, there is much in the volume that gives it, as we may say, a patchwork character.

We are much surprised at one opinion which the author expresses. He says of cancer, encephaloid tubercle, and melanosis, that "it is questionable whether the plan of treatment pursued by the quack with these latter is not sometimes the most efficacious, destroying their vitality by powerful escharotics, and causing them to drop out, rather than our method of extirpation by the knife."

In the last chapter, which is on pseudo-medical science, some effectual blows are dealt upon phrenology, mesmerism, hydropathy, and homœopathy. We will give one or two specimens of his mode of treating the last named delusion:—

"To show nature's want of curative power, Hahnemann, most unhappily for himself, points out that 'it cannot bring together the gaping lips of a wound, and by their union effect a cure; it knows not how to straighten and adjust the broken ends of a bone; it cannot put a ligature on a wounded artery, but in its energy it causes the patient to bleed to death.' No, nor can it convey our meat and drink to our lips without our own mechanical effort. In the above instances, we see manifested, in a manner that in all ages has obtained the admiration of mankind, the consummate skill of nature. No, Hahnemann, we cannot give up this principle for your dogma; and you above all others ought not to require it, for without the *vis medicatrix naturæ*, what would your treatment be?"

It is thus that he brings the experience of the profession to bear as a full battery upon the exclusiveness of Hahnemannism. "We do not think it 'contrary to nature' to seek to cure disease by an open combat with it, by what Hahnemann terms antagonistic measures; so far holding the ancient maxim, *contraria contrariorum sunt remedia.* We apply cold to the hot head or skin in a frenzy or fever; a warm bath when the perspiration has been suppressed; we bleed in plethora or inflammation, and thus destroy the *pabulum* of disease, or in hemorrhage, to take off, by mechanical means, the *vis à tergo;* we give an alkali by a chemical law to neutralize the acid which may be proved to be present; purgatives in constipations; astringents in hemorrhage or diarrhœa; kousso in tapeworm; all wrong, according to Hahnemann. We have found that we can *citò, tutè et jucundè* relieve a colic by a carminative; a syncope by ammonia; a spasm by an opiate; or a gastralgia by a dose of brandy; and we adopt such methods, though they are deprecated by Hahnemann, as merely palliatives. In fact, we adhere to no dogma, neither the one given above, nor the opposite one of the homœopaths, viz., 'like cures like;' we avail ourselves of various laws and principles—our remedies may be vital, chemical, or mechanical; specific, derivative, or counter-irritant; diverse enough, at any rate, to prove that we are less systemists than the homœopaths themselves; a point on which they attack us."

W. H.

Art. XXVI.—*Etudes sur la Monorchidie et la Cryptorchidie chez l'Homme.* Par M. Ernest Godard, Interne des Hôpitaux de Paris, Membre de la Société de Biologie et de la Société Anatomique. Extrait des Mémoires de la Société de Biologie, année 1856. Paris, 1857. Octavo, pp. 164.

In a recent number of this Journal (that for April, 1858), the last volume of the published minutes and memoirs of the Biological Society of Paris was briefly noticed. Attention was then called particularly to the great variety of subjects, in the study of which the members of that active society were engaged. It is an error, but a very common one, so much so that we feel called upon to refer to it, to suppose that the field allotted to the biologist in the domain of science is one of narrow limits. This error arises from a prevailing notion that when there is no longer life in an object, it is no longer an object for study to the biologist; and again, that any departure from the normal condition of things places a living

organized being beyond his legitimate field of research. This is not so, however; *biology*, meaning *life word*, everything connected with *vital phenomena* must be the object of the biologist's research. Biology does not mean simply the science of live animals, but the science of all organized beings in their two conditions, statical and dynamical, as fitted to act and as acting. Under the one condition, it comprises their organization or their anatomy, both normal and pathological, and the laws of their arrangement in natural groups, or biotaxy. Under the other condition, it comprises the influence of the medium in which the organized being is placed, or, in other words, the influence of exterior agents; and physiology, or the study of the functions of the organs.

The memoir which we are now called upon to notice has been extracted from the memoirs of the Biological Society of Paris for 1856, and published thus in a separate form. It is a work of very considerable importance, and contains information of value to the physician, in the several points of view of anatomy, of physiology, of pathology, and of legal jurisprudence.

The testicles, developed in the abdomen, descend gradually towards the scrotum, into which they fall at the ninth month of intra-uterine life. They may, however, be arrested in their course, and a temporary or a permanent anomaly be constituted, which has been called monorchidia, single-testicle, or cryptorchidia, hidden testicle, according as one or both of these spermatic organs are affected. It is then of these anomalies that the memoir of M. Godard treats. The anomaly that consists in the complete absence of one or of both testicles, of which undoubted examples have been reported, one of them in the twenty-third volume of this Journal (old series),[1] is not considered by him; he merely relates some facts for the purpose of showing that it is an anomaly really existing.

The researches of M. Godard go to show that monorchidia, which is the vice of conformation first treated of, is the result of an anatomical defect; of an error of diagnosis which has led to the application of a bandage for the purpose of retaining a hernia supposed to exist; or of a spasmodic contraction of the cremaster muscle.

The anatomical causes may belong to the testicle, to the gubernaculum testis, and to the pillars of the inguinal canal. Examination after death in the new-born child, has shown the testicle so swollen from inflammation that it could not pass through the abdominal ring; and moreover, the existence of a local peritonitis causing its adhesion to neighbouring organs. Lesions of the gubernaculum testis are, more often than is generally supposed, the primary causes of this vice of conformation. It is, most probably, owing to the contractions of the gubernaculum that the testicle descends from the place where it is first developed in the neighbourhood of the Wolffian bodies downwards to the scrotum. When the gubernaculum is completely wanting, the testicle remains where it was developed. The fibrous rings of the inguinal canal, above all the external, are sometimes so narrow as to prevent the descent of the testicle.

When a child has, in the inguinal region, a movable reducible tumour, the mistake is sometimes made of applying a truss, which will permanently prevent the passage of the testicle out of the abdominal cavity. This mistake is generally made by a bandagist, as M. Godard says; but very capable men may be supposed to make this mistake at times, from want of care in making the examination. In the October number of this Journal, for 1848, page 348, a case is reported by a fleet surgeon in the American navy, in which he had treated for some time an inflamed testicle situated in the inguinal canal as a strangulated hernia.

Spasmodic action of the cremaster muscle is very rarely the cause of monorchidia. M. Godard has been able to collect but two examples of it. In these cases the testicle, after its descent into the scrotum, was raised up again into the inguinal canal by the action of this muscle, and became fixed there.

In the numerous cases observed by M. Godard, 58 in number, in which one testicle had come down into the scrotum and the other not, sometimes both organs were healthy, at others one of them had undergone a change, and at

[1] In an article entitled, "Contributions illustrative of the Functions of the Cerebellum," by John D. Fisher, M. D., of Boston.

others again both were in a pathological condition. All these cases are, therefore, arranged in four great divisions, as follows:—

1st. The descended testicle and that of the opposite side arrested in its evolution are in the normal condition.

2d. The descended testicle is normal, that which has not descended being diseased.

3d. The descended testicle is in a pathological condition, that of the opposite side arrested in its evolution being healthy.

4th. Finally, both organs are diseased.

Each of these divisions offers some varieties according to the side where the anomaly has its seat, and the place in which the testicle has stopped. The diagnosis, the prognosis, the consecutive accidents, the pathological anatomy, the condition of the testicle, and the means to be adopted to facilitate its descent, in all these manifold varieties, are carefully and ably treated of. In the majority of these cases, the descended testicle was healthy; that is, it did not bear marks of disease. The examination made of its structure, however, showed that it was not performing its functions, that it was not acting as an organ of generation. When cut open, the gland had its normal colour, and its consistence was the same with that of the descended testicle. The seminiferous cones were arranged as in the normal condition, and no difference could be observed, by the aid of the microscope, in the canaliculi. The liquid extracted from the canaliculi did not contain any spermatozoa; only nuclear epithelium, globules of blood, and some fatty globules. Upon an examination of the epididymis, not a trace of spermatozoa could be seen in the liquid contained in the canaliculi, nothing but cylindrical epithelium. The seminal vesicle of the same side never contained spermatozoa.

In the absence of any other cause for the non-secretion of spermatozoa in the arrested testicle, and recollecting the fact that it is always fixed and motionless in the place it occupies, M. Godard concludes that the organ does not secrete because it does not possess the mobility it ought to have, and which it enjoys in the scrotum, where every moment it is subject to the contractions of the cremaster muscle.

The consequence of this capital fact is, that in cases where monorchidia exists the testicle in the scrotum is the only one that serves in generation.

It was an idea of the ancients, and it is still held by some persons, that the sex of the child depends upon the testicle that provides the semen by which the ovule is fecundated. Hippocrates, for example, says that, in order to have a girl, the man should tie the right testicle as firmly as he can bear it; to have a boy, he should tie the left.[1] Numerous observations recorded by M. Godard show that persons having but one testicle in the scrotum, consequently but one active organ, have children of either sex.

It being a matter then of the greatest importance that the testicle should descend into the scrotum, nothing should be neglected that could further its descent. The means recommended by M. Godard, as a general rule, are gymnastic exercises, swimming, and violent movements; of course, these means must be carefully watched and employed with discretion.

By cryptorchidia, M. Godard means that vice of conformation consisting in the absence of testicles in that portion of the integument that corresponds to the scrotum. This condition of things, very rare in men, constitutes, as is well known, the normal state of the greater number of animals. Its causes are identically the same as those of monorchidia. It cannot, however, be hereditary, as monorchidia sometimes is, unless it be in a family such as that of the Irishman, where it was hereditary not to have children. From the observations made by M. Godard, it is proven beyond a doubt, that men whose two testicles are arrested in their evolution are not impotent, but sterile.

The text of this memoir is illustrated by a large number of figures, and by three large lithograph plates.

By the manner in which the Biological Society of Paris issues its *Comptes Rendus* and *Memoirs*, it is well seen how well they appreciate the fact that *a*

[1] Littré's translation into French. Vol. viii. p. 501.

society is what it does, if we may so express ourselves. Judging of it in this way, we must look upon it to the medical man as one of the most important societies in the world. W. F. A.

ART. XXVII.—*On Amputation by a Long and a Short Rectangular Flap.* By THOMAS P. TEALE, F. L. S., F. R. C. S., Surgeon to the Leeds General Infirmary. Illustrated by engravings on wood by Mr. Bagg. London: Churchill, 1858. 8vo. pp. 72.

THE question as to the best mode of performing an amputation has ever been a somewhat unsettled one. Even at this day we can recognize in the surgical ranks the existence of two distinct parties—those who advocate the circular operation, and those who prefer to amputate by the method of single or double flaps.

Mr. Teale, the writer of the little volume before us, states, in his general remarks, that he has at different periods of his life practised both methods of operating. Finding, however, that neither of these were entirely free from certain objections, Mr. Teale has devised a plan of proceeding which affords, in his estimation, results of a much more satisfactory character than either of those ordinarily employed. In performing an amputation, the author cuts two flaps, both of these being of a perfectly rectangular shape. The two flaps, however, vary much in size; the external one, as a general rule, being four times the length of the internal and shorter one. The vessels and nerves of the limb are always to be comprised in the lesser flap. When the flaps are cut, the long one—equal in length and breadth to one-half the circumference of the limb, and consequently a perfect square—is allowed to fall easily over the end of the bone; its lower extremity is then brought up, and united by points of suture with the angles and extremity of the short flap. A stitch or two is also inserted, in order to unite the reflected with the unreflected portion of the greater flap. It must be observed, also, that in making the dissections above described, care is taken to separate the soft parts from the bone, close to the periosteum, so as to preserve to the greatest extent the tissues necessary to form the pad or cushion of the stump.

The chief peculiarity of the operation, as thus described, will be seen to be the obtaining of a sufficiently large fleshy mass for the covering of the end of the bone. The stump thus formed will be of large size, soft, movable over the sawn end of bone, and devoid of all large vessels or nerves. It will, consequently, be well fitted to sustain the necessary amount of pressure; and the frequently occurring evils of a painful and irritable cicatrix will be by these means entirely obviated.

In order to substantiate the conclusions at which he has arrived, Mr. Teale presents us with the following summary of cases which have been operated upon by his new method:—

56 AMPUTATIONS (From June 16, 1855, to June 16, 1858.)	Thigh, 18.	Accident, 1.	Death,	0.
			Recovery,	1.
		Disease, 17.	Deaths,	3.
			Recoveries,	14.
	Leg, 28.	Accident, 1.	Death,	0.
			Recovery,	1.
		Disease, 27.	Death,	1.
			Recoveries,	26.
	Arm, 6.	Accident, 3.	Death,	1.
			Recoveries,	2.
		Disease, 3.	Deaths,	2.
			Recovery,	1.
	Forearm, 4.	Accident, 1.	Death,	0.
			Recovery,	1.
		Disease, 3.	Death,	0.
			Recoveries,	3.

By glancing the eye over the above table, it will be seen that of 6 cases of amputation for accident, only one death occurred; and this death, we are informed, was caused by shock. In the annexed tabular statement of the result of 24 amputations, according to the ordinary methods, performed during the same period, at the same hospital, and by the same surgeons, the mortality is stated as being vastly increased; thus, of 17 amputations for accident, 10 died, and of these 10 deaths, 7 were the result of putrid or purulent poisonings.

Table of Amputations by Ordinary Methods.

24 Amputations (From June 16, 1855, to June 16, 1858.)	Thigh, 2.	Accident, 2.	Death,	1.
			Recovery,	1.
		Disease, 0.	Death,	0.
			Recovery,	0.
	Leg, 12.	Accident, 6.	Deaths,	3.
			Recoveries,	3.
		Disease, 6.	Deaths,	0.
			Recoveries,	6.
	Arm, 5.	Accident, 5.	Deaths,	4.
			Recovery,	1.
		Disease, 0.	Death,	0.
			Recovery,	0.
	Forearm, 5.	Accident, 4.	Deaths,	2.
			Recoveries,	2.
		Disease, 1.	Death,	0.
			Recovery,	1.

Why so great a difference as to the rate of mortality should exist between the proceedings of Mr. Teale and those ordinarily employed, we confess we are unable to understand, and we cannot see how it is to be charged simply to the different mode of making the incisions. It would seem to us, also, that after the formation of so large a flap there might perhaps be some tendency to non-union, or to sloughing, a point upon which the author is silent. Be this, however, as it may, the favourable statistics of the modified operation with which we are presented will undoubtedly lead surgeons to give Mr. Teale's method of amputation a fair trial, since the drawings of the stumps, photographed some months after the operation, are such as will compare most favourably with those obtained by any other procedure. J. H. B.

Art. XXVIII.—*Journal de la Physiologie de l'Homme et des Animaux.* Publié sous la Direction du Docteur E. Brown-Séquard. Tome premier, Numero III. Juillet, 1858.

The Physiological Journal of Brown-Séquard, which has now reached its third number, continues to maintain the high character which distinguished the first and second numbers, and which we have already adverted to in former notices of this periodical.

The present number contains twelve original contributions, besides the translations and extracts from other periodical publications. The original memoirs are as follows:—

1. Memoir on Hybridity in general, on the Distinction of Animal Species, and on the Hybrid of the Rabbit and Hare. By M. P. Broca.
2. On some New Facts relative to Epilepsy following Injuries of the Spinal Cord. By M. Brown-Séquard.
3. Researches on the Erectile Organs of the Female, and on the Muscular Tubo-Ovarian Apparatus in their connections with Ovulation and Menstruation. By M. Ch. Rouget. (With a plate.)
4. Researches on some of the Effects of Cold on Man. By MM. Tholozan and Brown-Séquard.

5. Remarks on the preceding paper. By M. Brown-Séquard.

6. On the Suspension of the Radial Pulse by Forced Extension of the Arm. By M. Verneuil.

7. On the Association of Inspiratory Efforts with a diminution or arrest of the Movements of the Heart. By M. Brown-Séquard.

8. On the Occlusion of the Superior Orifice of the Larynx and Pharynx during Inspiratory and Expiratory Efforts. By M. E. Smith. (With two figures.)

9. Researches relative to the Physiology and Pathology of the Annular Protuberance. By M. Brown-Séquard.

10. On the Origin of Sugar in the Chyle. By M. S. Colin.

11. Experimental Researches relative to the possibility of the passage, through the Nervous Centres, of Electro-Magnetic Currents applied to the Skin of Man. By M. F. Bonnefin.

12. On the Formation of Glucose in the Animal Economy. (Report made to the Academy of Medicine.) By M. Poggiale.

All these articles exhibit ability in the authors, and several of them are of a highly important character. We may cite, for instance, the memoir on hybridity, by M. Broca, and that of M. Colin "On the Origin of Sugar in the Chyle," as being of much interest, and as specially deserving of consideration.

Dr. Isaac's valuable memoir on the kidney is translated entire, and illustrated with the original drawings of the author, and the labours of several other American writers are noticed appropriately. W. A. H.

ART. XXIX.—*The Science and Art of Surgery: being a Treatise on Surgical Injuries, Diseases, and Operations.* By JOHN ERICHSEN, Professor of Surgery and of Clinical Surgery in University College, and Surgeon to University College Hospital. Second edition, enlarged and carefully revised. Illustrated by 400 engravings on wood. London: Walton & Maberly, 1857. 8vo. pp. 1040.

As a general text-book on surgery there is perhaps no work which justly occupies so high a position as the treatise of Mr. Erichsen. That this is the case has been most amply proven by the rapidity with which the first edition, both English and American, has been exhausted. In the preface to the new edition we are informed by the author that he has spared no pains in revising his original work, that much of it has been entirely rewritten, and that he has endeavoured to render the additions which he now makes of an essentially practical character. In this effort Mr. Erichsen has most happily succeeded; many of the subjects which were previously somewhat meagre in description, having in the new edition been faithfully described. The general arrangement, however, of the volume is the same, although the great amount of fresh matter interspersed throughout gives it almost the character of a different book.

In the recent issue we observe that the author has devoted several pages to the subject of the administration of anæsthetic agents, a topic which, fraught with such vital interest to the surgeon, is too frequently most summarily dismissed by the various writers on surgery. The present volume is also enriched by the introduction of numerous cuts, so that it may now be considered as being fairly illustrated; in this respect presenting a very great superiority over the work as originally presented. The index also exhibits marks of great improvement, being copious. The first appearance of Mr. Erichsen's publication did much to enhance the reputation, already great, of its distinguished author amongst his brethren on this side of the Atlantic. That this reputation will not now suffer, we are well convinced, from a careful perusal of the pages of the second edition. As a comprehensive treatise on surgery, Mr. Erichsen's is undoubtedly the favourite in this country, and we feel assured that its speedy reissue will be again most heartily welcomed. J. H. B.

Art. XXX.—*Lectures on the Principles and Practice of Physic, delivered at King's College, London.* By Thomas Watson, M. D., Fellow of the Royal College of Physicians, late Physician to the Middlesex Hospital, etc. A new American, from the last revised and enlarged English edition. With additions, by D. Francis Condie, M. D., Fellow of the College of Physicians of Philadelphia, etc. etc. With one hundred and eighty-five illustrations on wood. 8vo. pp. 1224. Blanchard & Lea: Philadelphia, 1858.

It may be confidently asserted that no work on the Practice of Physic has ever received more universal commendation than has that of Dr. Watson. The author possesses the happy faculty of giving to the dry relation of pathological facts, and the uninteresting details of therapeutical appliances, not simply clearness and exactness, but actually attractiveness. His style, which is plain, simple, almost conversational, is, at the same times, graceful, accurate, and forcible, and all his teachings clear, full, and precise. His lectures possess a fascination of manner which is to be met with in the purely practical writings of no other physician of modern times, with an extent and copiousness of scope, and an accuracy and completeness in the handling of the several subjects embraced in them which has heretofore been scarcely attained.

Borrowing without stint from the vast fund of medical knowledge accumulated by the joint labours of the physicians, as well of former years as of the present day; testing the value of every assumed fact—old or new—by the well-established principles of general pathology and therapeutics, and the result of his own clinical observations, the teachings of Dr. Watson have a copiousness, accuracy, and earnestness, which cannot fail to convey to the mind of the student, with the desired truthfulness and vividness, the knowledge they are intended to teach; to inculcate those principles in medicine which have received the sanction of the best minds in the profession, and those plans of treatment which come to us indorsed by the experience of the most successful practitioners of the healing art.

The work is an eminently practical one, in the proper acceptation of the term. The great object of the author is, on the one hand, to teach his readers how to know, distinguish, and judge disease, and, on the other, how best to treat it, in order to abate the suffering and insure the safety, as far as possible, of the patient. To this end, physiology, pathology, histology, and organic chemistry are called in, whenever their aid is available, to explain, correct, and embody the facts derived from simple observation—to enlighten, in other words, the teachings of empiricism. Dr. Watson is, however, no theorist; neither does he allow himself to be led astray by the results of hasty observations, or too rapid conclusions from badly established premises or imperfectly observed facts. He is wary of all novelties, and treats with proper contempt high-sounding terms, which have a semblance of knowledge without the substance; but, at the same time, he is a firm believer in the progress that medicine has already made, and which it must still continue to make.

The lectures of Dr. Watson, as they appear in the edition before us, present a full and faithful exposition of the actual state of the theory and practice of medicine, with all the improvements it has of late years received from the physicians of almost every portion of the civilized world. They are generally conceded to be, at once, the most complete and practical treatise of physic extant; the best adapted to the wants of the student, from their exactness, clearness and simplicity, and to those of the practitioner from their copiousness and accuracy.

In the present edition, the work has undergone a thorough revision; everything of value which has been recently added to our store of knowledge in the various departments of medical science, will be found incorporated in it, whether in reference to the etiology, pathology, and treatment of the different forms of continued fever; the physiology and pathology of the spinal cord; the recent researches on fatty degeneration of the heart and other organs; cardiac coagula, and the occlusion of arteries by travelling cardiac vegetations; affections of the supra-renal capsules; the influence upon the endo and pericardium of injections

of lactic acid into the cavity of the peritoneum; diseases of the stomach, and on the actual value and proper employment of bloodletting as a remedy in inflammation; embracing an exposition of the views, experiments, and observations of Drs. Jenner, Brown-Séquard, Todd and Gull, Quain, Garrod, Kirkes, Brinton, Addison, and Richardson, Mr. Lochart Clarke, and a host of others, who, of late years, have perfected and enlarged almost everything in relation to medicine, both as a science and an art.

So copious have been the additions made by the author in the present edition, that the work is extended over two hundred pages, "notwithstanding a very considerable enlargement in the page."

It might be supposed that to a work, like that of Dr. Watson, of which the accuracy and fulness are so universally confessed, no opening would be found for any additions or annotations on the part of the American editor. Nevertheless Dr. Condie has, by a judicious exercise of his editorial functions, succeeded in increasing the interest and value of these lectures generally, but more especially to the American student and practitioner. His annotations are mostly just, and his additions pertinent and well drawn up. Independently of the proper textual additions, he has also augmented considerably the number of illustrations beyond those of the latest London edition, and thus considerably increased its attractiveness, which, in itself, is a merit of no small importance, as tending to a wider diffusion of its valuable teachings.

In its present form, the work is not only a valuable, but an indispensable, addition to the library of every physician; so that the members of the profession owe many thanks to the American publishers, for offering it to them in so attractive a form, as respects paper, typography, illustrations, and binding, and at a price so low as to place it within the reach of even the poorest among them.

ART. XXXI.—*A Practical Treatise on the Causes, Symptoms, and Treatment of Spermatorrhœa.* By M. LALLEMAND. Translated and edited by HENRY J. MCDOUGALL. Third American edition.

To which is added: *On Diseases of the Vesiculæ Seminales and their Associated Organs; with special reference to the Morbid Secretions of the Prostatic and Urethral Mucous Membrane.* By MARRIS WILSON, M. D. Philadelphia: Blanchard & Lea, 1858. 8vo. pp. 380.

THE treatise of Professor Lallemand is well known in this country. A translation, made by William Hood, M. D., of Portland, Me., was published in this city as long ago as the year 1839, and, as the title page of the volume before us states, this is the third American edition of the translation made in England by Mr. McDougall.

Notwithstanding the very favourable reception this treatise has met with from the profession generally, and the laudations which have been bestowed on it by the medical journals, it has, we believe, been productive of much harm, having taught a most injurious mode of practice—the cauterization of the urethra—and led many to resort to advertising charlatans for relief.

The work of Marris Wilson, which has been appended to this edition of the treatise of Lallemand, seems to us to be a fit companion, teaching what we regard as equally objectionable modes of treatment. We may instance the following, which we give in the author's own words:—

"I am in the habit of applying a strong solution of the nitrate [of silver] to any single part of the canal, by using a curved glass syringe—a catheter, in fact—with an opening on the back of the curve near its extremity, the instrument having a small globe of India rubber attached to its external end. The opening is made to pass over every part to which it is required to apply the caustic solution, and a slight pressure kept upon the India rubber globe always brings a fresh quantity of the solution to the opening" (p. 372).

It is entirely unnecessary for us to make any reflections on such a mode of treatment as the one thus recommended.

A remarkable case is recorded in this publication of Dr. Wilson, in which, for

the relief of spermatorrhœa, the left testicle was removed, and by Sir Astley Cooper; not being relieved, the right was amputated, but the erections and emissions, both diurnal and nocturnal, were as frequent as before. Then, under the supposition that the prostate was the seat of the disease, pieces of potassa fusa were introduced into the gland, for the purpose of destroying it. This treatment is now being pursued. Distinct emissions continue to take place, but at long intervals, the fluid being considerably less in quantity! (pp. 338–40.) Curling relates a case where a gentleman in the upper ranks of life was castrated, on account of most distressing self-pollutions. The patient committed suicide; and the surgeon who had been rash enough to emasculate him, was threatened by the patient's friends with an action at law for performing so unwarrantable an operation. (*Practical Treatise on the Diseases of the Testis, &c.*, Philad. 1856, p. 338.)

In the treatment of spermatorrhœa, the best surgeons confine their interference to general treatment; cauterization, which they were induced to try by what is said in its favour in the romance of Lallemand, they soon abandoned. We believe, ourselves, that the advice given by an eminent author, whose writings, though extensively disseminated of late, are little heeded, is that from which the greatest benefit is sure to come: *ἐι δὲ πνεύματι τὰς πράζεις το̃ν σώματος θανατο̃ντε, ςήσεσθε.* (Eʼπις. προσ Pῶ H΄. ιγ΄.) W. F. A.

Art. XXXII.—*A Treatise on Fractures.* By J. F. Malgaigne, Chirurgien de l'Hôpital Saint Louis. With one hundred and six illustrations. Translated from the French, with notes and additions, by John H. Packard, M. D. Philadelphia: J. B. Lippincott & Co., 1859. 8vo. pp. 683.

M. Malgaigne's most excellent and elaborate work on fractures and dislocations has been so long and so favourably known to the profession, as to render unnecessary any extended notice at the present time. It affords us, however, sincere pleasure to be able to welcome the appearance, in an English dress, of the first volume of this valuable treatise. The translator, Dr. Packard, undoubtedly deserves the thanks of the profession for the zeal and fidelity with which he has discharged the labour of rendering into English this volume on fractures. The annotations which he has appended to it are numerous, and appear to us to be of much practical value, adapting, as they do, the treatment of fractures to the generally received and most approved American methods.

The original illustrations of M. Malgaigne's work were comprised in a large folio atlas of lithographic plates. For the convenience of readers, and facility of reference, these have been faithfully reduced one-half, and have been collected at the end of the volume, with an explanatory text attached. The general appearance of the volume is extremely satisfactory, and reflects much credit on the translator. We trust that its success may be such as to warrant the speedy presentation of the remaining portion, which treats on dislocations.

J. H. B.

Art. XXXIII.—*A Treatise on the Venereal Disease.* By John Hunter, F. R. S. With copious additions, by Dr. Philip Ricord, Surgeon of the Hôpital du Midi, Paris, etc. Translated and edited, with notes, by Freeman J. Bumstead, M. D., Lecturer on Venereal at the College of Physicians and Surgeons, N. Y., Assistant Surgeon to the New York Eye Infirmary. Second edition, revised, containing a *résumé* of Ricord's recent Lectures on Chancre. Philadelphia: Blanchard & Lea, 1859.

Hunter's *Treatise on the Venereal Diseases*, with the additions made to it by M. Ricord, is almost universally acknowledged to be the best work that has ever been published upon the subject of which it treats.

Hunter was resolved, as he said himself, that this work should not be a mere bookseller's job—each subsequent edition rendering the former useless. The

greatest of syphilographers, whether living or dead, although teaching doctrines widely different from those of Hunter, was not able to teach them better than by means of this treatise, the facts contained in which, some additional facts, observed by himself, show to have been wrongfully interpreted.

In this new edition of Ricord and Hunter, the American editor has made a number of very valuable additions, the most important of which are taken from the publication recently made by M. Fournier, of the lectures on chancre recently delivered by M. Ricord, at the Hôpital du Midi; a review of which publication is contained in the April number of this journal for the past year.

The editor states in his preface, "that the present edition of 'Ricord and Hunter' contains the fullest exposition of the views of M. Ricord that has yet been published." In this we fully concur, and would add, that it is the best arranged, most instructive, and most valuable work that has ever appeared on the subject of the venereal disease. W. F. A.

ART. XXXIV.—*The Modern Practice of Midwifery: a Course of Lectures on Obstetrics, delivered at St. Mary's Hospital, London.* By WM. TYLER SMITH, M. D., Member of the Royal College of Physicians. With an Introductory Lecture on the History of the Art of Midwifery, and copious Practical Annotations, by AUGUSTUS K. GARDINER, A. M., M. D., late Instructor on Obstetrics in the New York Preparatory School of Medicine, Author of the "Causes and Curative Treatment of Sterility," etc. Illustrated by 212 engravings. 8vo. pp. 760. R. M. DeWitt: New York, 1858.

THIS work is simply a reprint, in book form, of the lectures of Dr. Tyler Smith, which appeared in the *London Lancet* during the year 1856; such additional matter as is contained in the subsequently published manual, by Dr. Smith, of theoretical and practical obstetrics (London, April, 1858), being inserted, as far as practicable, in its appropriate locality; with an introductory lecture, and sundry annotations and additions, interspersed throughout the text, by the American editor.

Having very recently had occasion to express our opinion in reference to the doctrines and practice of midwifery, as taught by Dr. Tyler Smith, and the manner in which these doctrines and practice are set forth and illustrated by him, in a notice by us of the manual just referred to, which, as the author states, comprises the substance of his previously published lectures, differently arranged, and with such alterations and additions as give the work, to a certain extent, a character of originality, it will not be necessary for us to repeat here the remarks we then made.

While we feel no desire to dispute the statement of Dr. Gardiner, that the volume before us "differs" but "little from the author's revised edition," we must very candidly say that we doubt the propriety—waiving the question of right—of issuing the work of Dr. Smith in its present form, after the author had himself thought fit to recast it into an entirely new and different one; evidently intending that the form of manual should supersede that of lectures. To every author must unquestionably be conceded the right of deciding for himself the particular shape in which he shall appear in print before the public; and even though it may be agreed that his decision, in this respect, has been the result of an evident error of judgment, still, no one who shall undertake to edit his works has a right to rectify—without the author's privity and consent—the error, any more than he would have to suppress or modify any portion of the text.

Excepting this one circumstance of his not having followed, in matter and in plan, in the American edition of Dr. Tyler's work on obstetrics, the latest London edition—remodelled and revised by the author—the editor has fulfilled his duties, upon the whole, in an acceptable manner. The preliminary lecture, on the history of the art of midwifery, though but a sketch, is, to say the least of it, interesting and instructive, while his practical annotations, together with his lecture on the operations upon the os and cervix uteri demanded in cases of rigidity of the latter parts, are, in the main, sound and judicious. D. F. C.

ART. XXXV.—*The Uræmic Convulsions of Pregnancy, Parturition, and Childbed.* By Dr. CARL R. BRAUN, Professor of Midwifery, Vienna. Translated from the German, with Notes. By J. MATTHEWS DUNCAN, F. R. C. P. E., Lecturer on Midwifery, etc. etc. 12mo. pp. 182. S. S. & W. Wood, New York, 1858.

THE work before us consists of a translation into English of one of the chapters of the *Lehrbuch der Geburtshülfe,* the text-book of midwifery of Professor Charles R. Braun, of the University of Vienna, which appeared in the early part of 1857. Dr. Duncan, of Edinburgh, informs us that he undertook the translation of this portion of Professor Braun's treatise from a conviction of its value, as well in respect to the importance of the subject of which it treats as from its completeness and erudition. The translation was first published in the *Edinburgh Medical Journal,* and subsequently in its present separate form, partly, as we are informed, "in consequence of the reception it had already met with, and partly from the utter deficiency of any similar treatise in the English language."

The pathology of eclampsia as it occurs in the pregnant, parturient, and puerperal female, has occupied the attention of some of the most distinguished obstetricians of the past, as well as of the present day. Unfortunately, however, the observations heretofore made in reference to the disease have not been of a character to throw much light upon either its remote or direct causes, or to settle with certainty its true character and most successful treatment.

It is more than probable that under the general denomination, eclampsia puerperalis—puerperal convulsions—there are included convulsive affections dependent on different and very dissimilar causes. In the chapter of his recent work on midwifery, the translation of which is before us, Professor Braun has endeavoured to show that, what he denominates "true puerperal eclampsia," is always intimately connected with albuminous diabetes, or Bright's disease of the kidneys, in an acute form, "which, under certain circumstances, spreads its toxæmic effects on the nutrition of the brain and entire nervous system."

"The toxæmia, or blood poisoning, in the eclampsia of the pregnant, parturient, and puerperal conditions, is," Prof. B. remarks, "commonly produced by uræmia; that is, by a change in the urea which has been retained in the blood, or by retention of excrementitial extractive matter of the urine. Hence, according to the present state of our knowledge, true eclampsia during pregnancy is designated *uræmic;* without, however, implying that it is peculiar to pregnancy or child-bearing; because the same disease, with similar phenomena, may manifest itself, also, in women not pregnant or in childbed, and even in males under certain favourable conditions."

Professor Braun is very far from denying that the most dissimilar causes may, during the period of pregnancy, as well as out of it, produce phenomena resembling those of uræmic eclampsia; but, after a careful examination of descriptive and statistical observations, by himself and others, he thinks that he is permitted to maintain the position, "that acute Bright's disease and uræmic poisoning of the blood are the true causes of puerperal eclampsia."

The coincidence of eclampsia and albuminuria is a fact that has been abundantly verified by the numerous observations of Simpson, Lever, Cormack, and others, of Great Britain; of Develliers, Regnault, Dubois, Danyau, Cazeaux, and Blot, of France; and of Helfft, Frerichs, Litzmann, Wieger, Oppolzer, Braun, and others, of Germany, as well as by the daily observations of medical practitioners everywhere.

An œdematous intumescence of the external parts of the body, often of the face, arms, and hands, but more generally of the ankles, feet, and labia majora, is a common phenomenon preceding and accompanying puerperal convulsions, and such intumescence is, also, one of the most frequent symptoms of Bright's disease. The œdema is not always permanent, but appears when the patients are in a recumbent position, and disappears when they arise. It sometimes de-

creases towards the end of pregnancy, and not unfrequently disappears altogether, even while sometimes the albuminosity of the urine and the morbid process in the kidneys are increasing. It is only when in pregnant women the œdema is connected with the presence of albumen, fibrin cylinders, and fatty degenerated scales of Bellini's epithelium in the urine, that they have any connection with uræmic eclampsia.

"Frerichs," says Prof. B., "has often found the œdema already developed in the third month of pregnancy, although far more frequently it is first observed in the last four months, often, however, without attracting attention, as the other less obvious features of the morbus Brightii are overlooked, although they are present. The more the pregnancy advances, the more the albumen in the urine increases, but disappears quickly after labour, sometimes within a few days, unless the exudative process in the kidneys makes further progress, and assumes the condition of chronic Bright's disease. But the dropsical swelling is not necessarily connected with this albuminuria, just as on the other hand there not unfrequently occurs, during pregnancy, a dropsy in which the urine is found quite normal."

"The symptoms," remarks our author, "of uræmia, which are observed in animals after the extirpation of both kidneys, are the same as those of the acute Bright's disease of pregnant women, in whom the diseased condition of the secretion and excretion of the kidneys causes pollution of the blood with excrementitial elements of the urine, a state which frequently, after headache, giddiness, drowsiness, delirium, coma, and convulsions, leads on to death."

"Among the symptoms of uræmia, are also the diminished power of vision, amblyopia, and complete amaurosis. A pregnant woman, after headache, vomiting, or a condition of stupefaction, may be found altogether blind. We have also to enumerate here the humming in the ears, or the suddenly supervening partial deafness of pregnant women; also a fever very like typhus, with diminution or complete suppression of urinary excretion (*Febris urinosa* of Frerichs); in addition, a painful vomiting of tough or watery substance, in which the addition of an alkali reveals the presence of ammoniacal compounds, but never of undecomposed urea.

"These various phenomena appearing in the different regions of the body during life, leave no changes constantly discoverable after death. The brain and its membranes are then either exsanguine, natural, or engorged with blood; the sac of the arachnoid, and the cerebral ventricles, contain sometimes, but not always, a serous effusion. The globe of the eye is, after uræmic intoxication and amaurosis, sometimes normal; sometimes it exhibits an increased secretion of aqueous humour, sometimes an exudation on the retina.

"The degree of coagulation of the blood, when drawn from the veins, varies; it has a tinge of violet colour, sometimes an ammoniacal smell, distinctly reminding one of putrid urine, and it contains carbonate of ammonia—often in such quantity, that it effervesces on the addition of muriatic acid—with generally some remains of undecomposed urea.

"The uræmic fits do not originate, as was formerly supposed, only from the blood being poisoned by urea. Filtered urine, injected into the veins of animals, has been tolerated without evil consequences.

"In regard to uræmic intoxication, Frerichs has been led, by a series of carefully performed experiments, to conclusions, which we have found confirmed in the cases of several pregnant women who suffered from eclampsia and acute Bright's disease.

"(*a*). The phenomena of uræmic intoxication are produced neither by urea or any other ingredient of the urine, nor by the united excretory matters, as such, of this fluid; but they commonly arise from this circumstance, that the urea accumulated in the blood is transformed into carbonate of ammonia under the influence of some peculiar ferment.

"(*b*). Carbonate of ammonia is the baneful power which produces these disturbances of the functions of the nervous system.

"For the production of uræmic intoxication, it is therefore necessary to have in the blood quantities of urea, and the presence of some ferment, by means of which the urea may be changed into carbonate of ammonia. If the fermenting

material is wanting, then the blood may be for a long time impregnated with urea, without any injurious consequences appearing. In this way the fact is accounted for, that in the bodies of persons dead of Bright's disease, the blood may be found saturated with urea, without any uræ-phenomena having been observed during life. The cause of this fermentation is, as yet, not altogether known."

The theory of the identity of uræmic poisoning, from acute Bright's disease and puerperal eclampsia, has been strenuously opposed by Marchall, Depaul, Legroux, L'Huillier, Siebert, Stoltz, Seyfert, Levy, Scanzoni, and others. Those gentlemen maintain that the diseased condition of the kidneys often met with in the bodies of those who have died of eclampsia is the consequence and not the cause of the convulsions—an accidental secondary phenomena, only, of hyperæmia caused by the eclampsia and the serous plethora—hydræmia—of pregnancy. This opinion they consider to be proved by the following considerations. It is only, they assert, in the minority of such as die of eclampsia that the changes discovered in the kidneys are sufficient to justify the diagnosis of Bright's disease. There is no evidence that the presence of albumen and fibrin cylinders in the urine always precede the convulsive attack. The arguments adduced to prove uræmic poisoning of the blood in cases of eclampsia, are not of themselves sufficient to prove that genuine puerperal convulsions are always the result of such poisoning as a consequence of Brightian degeneration of the kidneys. Puerperal eclampsia, the objectors maintain, consists in general clonic convulsions of the voluntary muscles, proceeding from the spinal marrow, with absence of consciousness; which convulsions have their immediate cause in the irritability of the motor system of nerves, the result of pregnancy and augmented by the act of delivery.

In reply to these objections, it has been attempted to be shown that the existence of Bright's disease is extremely common in those who have died from eclampsia, as is proved by the observations of Professors Braun, Gustav. Braun, Wedl, Lumpe, Hecker, Devilliers, Regnauld, Hesse, Simpson, Sabatier, Hohl, and a host of others. That the indications of the presence of nephritic disease, soon after the appearance of convulsions are of too decided and extensive a character to allow us to suppose that such disease had been developed within the few preceding hours. That acute Bright's disease often precedes the occurrence of eclampsia is very certainly proved by numerous observations on record, and hence it is a probability, bordering on certainty, that a connection exists between the two.

"The circumstance that eclampsia does not occur in every case of Bright's disease during pregnancy, may, as Litzmann has very correctly remarked, be accounted for by convulsions following only when the blood has been very considerably impregnated with the excrementitial elements of the urine, which always implies a very profound, or, at least extensive disease of the renal tissue."

Albuminuria is said to occur more frequently in parturient and puerperal women than among the pregnant. In support of this position cases in which only a trace of albumen, without admixture of exudation clots, is to be detected in the urine, have been estimated as of equal importance with those cases where that copious excretion of albumen and of cylindrical clots takes place which is constantly met with in eclamptic patients. Litzmann was the first correctly to appreciate this occurrence of traces of albumen among lying-in women as a catarrh of the bladder, on account of the simultaneous presence of pus-globules, and the absence of exudation clots. He believes, also, that this cannot be regarded even as a symptom of commencing Bright's disease.

The supposed sudden and abundant appearance of albumen and of fatty degenerated cylindrical clots has not been proved by any convincing microscopical observations that have as yet been made public.

According to the observations of Frerichs, Hasse, and the author, the most violent attacks of epileptic and hysterical convulsions, even when occurring repeatedly during the same day, never have, as a consequence, albuminuria or excretion of cylindrical clots. In women, however, affected with habitual epilepsy, eclampsia may supervene, from Bright's disease, during pregnancy, labour,

or childbed. When eclampsia appears during the puerperal state, it originates in Bright's disease, which had been already developed during pregnancy.

Cases of unquestionable eclampsia from uræmia, dependent upon disease of the kidneys, and terminating fatally, have been recorded by Braun and Bamberger, in a case of retroversion of the uterus, and by Picard, in a man affected with stricture of the urethra.

"The first stage of Brightian kidneys, in which the exudation in the tubuli uriniferi is still fluid, and cannot be demonstrated by the microscope, or when the coagulated exudation appears periodically in the urine in large quantities, as cylindrical clots, and then again disappears for some time, must be regarded as a blood-poisoning disease, equally dangerous as the fatty metamorphosis of the kidneys. For Brücke's ingenious investigations on the casual connection of albuminuria and uræmia, have shown that the occurrence of uræmia depends not so much on the *intensity* of the structural changes as on the *extent* of the morbid exudation of the kidneys."

If any value is to be ascribed to the careful chemical researches of Miahle, they afford sufficient grounds for our believing that copious albuminuria is not the product of hydræmia, but a primary exudation from the kidneys.

According to Dr. Cormack, pregnancy produces an increased necessity for purification of the blood through the congested kidneys, and in this way defective secretion of poisonous excrements from the blood—*i. e.*, toxæmia—is induced. The direct influence of this morbid blood on the brain, spinal cord, and medulla oblongata, causes convulsions.

"If we hold, with Frerichs, Litzmann, the author, Wieger, and others, that the ordinary cause of Bright's disease in pregnancy is the retardation of the stream of venous blood in the kidneys, from the compression of the venous trunks by the gravid uterus, then it is obvious that, in this circumstance, we have the cause of the abnormal process of exudation being more or less uniformly spread over the whole organ, without any immediate necessity for its texture undergoing more profound changes. On the contrary, such will probably appear only gradually, and after a long duration of the diseased action. It must always be kept in mind that it is possible to have one kidney only, or at least principally, affected, from the uterus having a peculiar position, as several cases already cited demonstrate. In this way, also, partly, the circumstance may be accounted for, as Litzmann has already remarked, that sometimes, in spite of the intensity of the local morbid process, no signs of uræmia appear, while in other cases uræmia is observed when the disease is much less intense, but probably affects both kidneys, in which case, the quantity of the urine does not always undergo a marked diminution."

In proof of the uræmic intoxication from Bright's disease producing parturient eclampsia, for the most part by carbonate of ammonia in the blood, and perhaps also by extractive matters of the urine, Professor B. remarks:—

"The investigations of Frerichs, Litzmann, the author, Heller, Kletzinsky, Oppolzer, Gegenbauer, and others, have demonstrated that in the eclamptic, urea and carbonate of ammonia, developed by its decomposition, are generally found in considerable quantity in the fresh blood; that from the presence of these materials in the blood, the occurrence of uræmic eclampsia may be prognosticated, and that these substances are observed also in the blood of children born of uræmic mothers.

"Chemical analysis, however, cannot always, even during the most violent eclampsia, discover the presence of carbonate of ammonia in fresh blood, as is shown in an observation made on the 14th August, 1854, by Gustav Braun and Holler. * * * In this observation, the blood was found not to have a uræmic, but a cholæmic, constitution; as it contained no carbonate of ammonia, but all the elements of bile. This observation, although it does not stand alone, cannot be used to invalidate the theory of the very frequent coincidence of eclampsia and uræmia. It rather points out that the excrementitial elements of bile may, producing cholæmia in the living body, be likewise a cause of eclampsia."

In reference to the theory of the intoxication of the blood by carbonate of ammonia, Dr. Litzmann's conclusions, after a careful consideration of the objections that have been urged against it, are: that the fresh blood of healthy individuals

never contains ammonia; but that the latter is present in the blood in the majority of the cases of uræmia, being formed there by the decomposition of urea formed and retained in the blood, or by the decomposition of urea that has been secreted into the urinary passages, and has returned into the circulation by absorption.

"But," remarks Prof. B., "on the one hand, presence of ammonia in the blood is not, by any means, a sign of uræmia exclusively, for it has been exceptionally observed (*Reuling*) in other diseased conditions (*typhus*, *pyœmia*), where the urinary secretion was not disturbed. On the other hand, cases undoubtedly occur where, notwithstanding obstructed secretion of urea by the kidneys, and the occurrence of all the characteristic symptoms of uræmia, the blood does not contain any ammonia, and the ammoniacal contents of the exhaled air are not increased (*Reuling*); but where, on the contrary, undecomposed urea is found in the transudations from the blood, and in the sweat especially, may be in such quantity as to be left on the skin in the form of a white crystalline dust (*Schottin*, *Fiedler*). The cause of the uræmic phenomena cannot, therefore, be sought for in the decomposition only of the urea retained in the blood into carbonate of ammonia."

Wieger would regard uræmia not so loosely as the ancients, who held it to be a metastasis of urine, nor so exclusively as Frerichs, who considers it an intoxication by carbonate of ammonia, but as a consequence of Brightian exudation into the kidneys, which, in its chemical relations, is characterized by retention of water and excrementitial matters in the blood, which itself, from the loss of blood-corpuscles and albumen, is impoverished in these elements. But it is impossible, according to our author, to say whether the chief part in the combined operation is to be ascribed to the excess of serum or to the diminution of the albumen and blood-corpuscles. Hydræmia, however, he remarks, is never the cause of the nervous symptoms, but has only a predisposing action.

"Eclampsia puerperalis is not," according to Prof. B., "a consequence of hydræmia of pregnancy, or of pains. For the following reasons, Bright's disease cannot be regarded as the consequence of eclampsia:—

"*a.* Blot's observations show that the average quantity of albumen in the urine is, in albuminuric individuals not suffering from eclampsia, 33 per cent.; in the eclamptic, sometimes 74 per cent. But, from the quantity of albumen in the urine, we cannot decide whether eclampsia will occur or not, because the whole quantity of urine secreted in twenty-four hours, and of urea accumulated in the blood, may greatly vary.

"*b.* From the analyses of Becquerel, Rodier, Devilliers, and Regnauld, it appears that the albumen in the blood of pregnant women is diminished 4 per cent., and in albuminuria 16 per cent. But hydræmia and diminished albuminous contents cannot be the cause of the escape of albumen into the urine, because, as Wieger has shown, it would be impossible to explain why hydræmic blood secretes a small quantity of urine; why blood-globules often, and exudation clots always, appear in the urine; why the absolute quantity of urea and of extractive matter secreted within twenty-four hours is diminished; why, in *post-mortem* examinations of eclamptic patients, the kidneys so often exhibit extensive degenerations; and how it can happen that, in cholera with an early stage of nephritis, condensation of the blood coexists with excretion of albumen.

"*c.* Depaul has advanced the opinion that albuminuria is a consequence of eclampsia, inasmuch as, when it exists during pregnancy, it disappears a few hours after delivery, while it is often not till some days afterwards that eclampsia begins, and causes the reappearance of the albumen.

"Wieger, founding his arguments on numerous observations made by the author, Blot, Regnauld, and Devilliers, has controverted this opinion in detailed statistical tables. He shows that, in cases of distinct albuminuria, and of eclampsia, the albumen does not disappear a few hours after delivery; that this does not happen till two days at least, and often not till a week after; that it often persists, and even increases; that in women dying comatose, the albuminuria continues till death; and that the danger of eclampsia not unfrequently increases with the intensity of the albuminuria.

"*d.* The cases adduced by L'Huillier, Depaul, Dubois, Mascarel, and the

author, in which, during puerperal eclampsia with fatal issue, no traces of albuminuria could be discovered, prove only that, in pregnant women, convulsions may, in exceptionable cases, be produced by various causes, as meningitis, burns, typhus, capillary apoplexy, habitual epilepsy, anæmia, cholæmia, etc. They do not force us to take refuge, in a manner that cannot be justified, in a mysterious hypothesis of some peculiar condition of puerperality, in hydræmia, leukæmia (*Bengel*), oligocythæmia, hypernosis (*Oldham*), hypalbuminosis, etc., for the explanation of the ordinary form of the far more frequently occurring uræmic eclampsia."

With Hasse, our author recognizes the truth, therefore, of the following propositions: "1. All cases of albuminuria in pregnancy and childbed, are not referable to Bright's disease. 2. Only a few cases of Bright's disease go so far as to produce uræmia. 3. It is not a necessary result of uræmia, that every one suffering from it will have eclampsia. 4. The appearance of this affection is not necessarily connected with any particular stage or extent of degeneration of the kidneys. 5. The function of labour is not the immediate cause of eclampsia, nor do the spasms during the paroxysms stand in any connection with the coming on of uterine pains."

In the foregoing analysis of the strictly pathological portion of Prof. B.'s remarks on puerperal eclampsia, we have endeavoured to present such an outline of his opinions in respect to the nature of the disease, and of the general arguments adduced in their support, as will bring them very fully to the notice of our readers, in order that they may be enabled to test their validity by the results of their personal observations.

We proceed now to give a brief sketch of the treatment of uræmic eclampsia recommended by our author, based upon his views of its pathology.

When at an early period of pregnancy hydræmia is developed, it may be somewhat ameliorated by a nutritious diet, vegetable tonics, and chalybeates, with the use of tepid and vapour baths. To neutralize the carbonate of ammonia formed in the blood by the decomposition of urea, the use of benzoic acid, lemon-juice, or tartaric acid is recommended by Frerichs. Congestion of the head is to be obviated by vinegar injections, aloes, jalap, etc.

When exudation has taken place into the Malpighian capsules, and the tubuli of Bellini and Ferrien, if there is a copious secretion of urine, the removal of this exudation may sometimes be effected by the copious use of any mild diluents; but when the secretion of urine is scanty, the best means for the removal of the exudation is the use of the acids named above, or the mineral waters of Seltzer or Vichy.

To restore the normal tone, pills of tannin and extract of aloes are recommended by Frerichs.

In cases of Bright's disease occurring during pregnancy, a question arises whether the induction of premature labour be admissible, with the view to prevent the venous congestion and the progress of degeneration in the kidneys. Prof. B. considers that artificial premature delivery is always improper, in the absence of all symptoms of uræmia; but he is in favour of it when the severity of the albuminuria, the quantity of cylindrical clots, a high degree of hydræmia, considerable dropsical swelling, with dangerous disturbance of the functions of the heart, lungs, brain, etc., indicate the imminence of the patient's danger, more especially when we have good reasons for supposing the fœtus to be dead.

When, in patients affected with Bright's disease, without uræmia, labour sets in, to prevent the occurrence of convulsions, Chailly recommends the production of a slight degree of anæsthesia by the use of chloroform; Prof. B. is in favour of this, also, in cases of metralgia and protracted labour, occurring in patients affected with Bright's disease.

When uræmic eclampsia has occurred, our author recommends highly the induction of chloroform narcotism, as soon as any indication of an impending paroxysm is detected, and the continuance of the narcotism until such indication disappears, and a quiet sleep ensues. If, however, the convulsive paroxysm be not arrested, during its presence and the succeeding comatose state, the use of the chloroform he directs to be suspended, in order to allow an abundant supply of fresh atmospheric air to reach the lungs.

To obviate the secondary congestion of the head, and meninges of the brain, the local application of ice is recommended, or sprinkling with cold water, or, as a means still more efficacious, the cold douche. Sponging the skin with tepid vinegar produces, we are told, a most desirable diaphoresis, and is easily accomplished.

Prof. B. condemns general bloodletting, in uræmic eclampsia, as being calculated to increase the hydræmia and aggravate the convulsive paroxysms, as well as from the danger of its giving rise to puerperal thrombus and pyemia, exhaustion, and a protracted convalescence. It is very certain, he however admits, that a cautious selection of single cases may be made, in which a single bleeding may not be inadmissible—cases occurring in strong, full-blooded patients, with violent pulsation of the carotids, with a dark red hue of the face remaining for some time after the paroxysm, and with indications of commencing œdema of the lungs, there being, at the same time, entire absence of any signs of anæmia, chlorosis, or debility.

If the eclampsia continue or first occur after delivery, and the chloroform and acids do not act with sufficient promptness and permanency, Prof. B. recommends large doses of opium or acetate of morphia, from one to six grains of the first, or from a quarter of a grain to a grain of the latter, every six hours, and at the same time an enema of from twenty to thirty drops of anodyne tincture. With respect to the value of opium as a remedy in such cases, the observations of Kiwisch, Scanzoni, Kilian, Wieger, Hohl, Feist, Gedé, and others, correspond with those of the author.

For the removal of the coma remaining after the cessation of the convulsive paroxysm, the remedies recommended are complete rest of body, mind, and external senses—with perfect quietness—the use of benzoic and vegetable acids, cold drinks, and moderate diaphoresis. The author is opposed to all derivatives—as sinapisms, blisters, local hot bathing, etc. Hasse advises an active impression to be made upon the bowels by calomel or castor oil, by the mouth, or enemata of assafœtida and vinegar, infusion of senna, croton oil, and the like.

During the convulsive paroxysm, the patient is to be guarded from injury, and from wounding her tongue, by proper restraint—which should not, however, confine the extremities, or embarrass the freedom of their movements.

When eclampsia occurs in connection with labour, if the expulsion of the child is not speedily accomplished, the author advises the acceleration of delivery by the rupture of the membranes, and the mechanical dilatation of the vagina and os uteri, the free scarification or division of the latter, or, where all things are favourable to their use, by the application of the forceps, or by such other measures as the nature of the presentation and the character of the labour warrant.

When, after the birth of the child, the discharge of urine is delayed, or only a very small amount is voided, the catheter should be introduced, in order to avert the retention and decomposition of the urine in the bladder, and the danger of resorption of carbonate of ammonia.

For the details of treatment we must refer to the work itself. In cases of eclampsia occurring during the pregnant, parturient, or puerperal condition of a female, where the cause of the convulsions can be traced with tolerable certainty to uræmic intoxication—and this can only be done when we ascertain that the urine is rich in albumen and cylindrical clots, and deficient in uric acid and urea—there can be little doubt that the treatment laid down by Prof. B. is, in its general outlines, the one best adapted to conduct the case to a favourable termination. From the induction of asthenia in cases of puerperal convulsions, we have repeatedly experienced the best results, and the opium practice has in its favour the testimony of some of the most authoritative of the practitioners of the continent of Europe. Notwithstanding the unqualified denunciation of general bloodletting in true puerperal eclampsia by the author, we must insist upon its propriety, if not as a curative, which it can seldom be considered, at least as a precautionary means, by which the brain and other organs are placed in a condition better adapted to sustain without injury the violent convulsive paroxysms. We admit that bleeding has heretofore been carried to an unwarrantable extent in all the forms of puerperal convulsions, but this abuse of the remedy is no argument against its judicious use in the cases to which it is appropriate. There

is certainly no remedy that has been proposed in the treatment of eclampsia puerperalis that has in its favour a greater array of imposing authority.

To relieve the uterus of its contents as speedily as possible, when the convulsions occur during labour, is no doubt advisable; we cannot, however, be brought to believe that the necessity for this is so urgent as to warrant, in order to effect it, a resort to manipulations, which can be justified only under circumstances where a neglect of them would very certainly endanger the life of both mother and child, which circumstances certainly do not present themselves in any of the ordinary forms of puerperal eclampsia. D. F. C.

ART. XXXVI.—*A System of Human Anatomy, General and Special.* By ERASMUS WILSON, F. R. S., author of "The Dissector's Manual," "A Treatise on Diseases of the Skin," etc. etc. A new and improved American, from an enlarged London edition. Edited by WILLIAM H. GOBRECHT, M. D., Professor of Anatomy in the Philadelphia College of Medicine, Fellow of the College of Physicians of Philadelphia, etc. With three hundred and ninety-seven illustrations on wood. Philadelphia: Blanchard & Lea, 1858. 8vo. pp. 616.

OF the many treatises on anatomy which we possess there is, perhaps, none more favourably known to the profession than the work with the foregoing title. In this country a copy of Wilson's Anatomy is found on the shelves of nearly every practitioner and in the hands of nearly every student of medicine, a fact which of itself sufficiently proclaims the high estimation in which the volume is held by American medical men. Of the merits of the work generally we need therefore say nothing; but we cannot refrain on the present occasion from remarking that this new edition is by far the most perfect of any which have ever appeared. The volume, in passing for the seventh time through the English press, has undergone a most careful revision at the hands of the author; many portions of it have been entirely rewritten, especially the introductory chapters relating to histological anatomy.

Very many valuable annotations, additions, and illustrations have also been introduced by the American editor, Dr. Gobrecht. In our opinion the merits of the work have been much increased by the evident labour and assiduity expended by Dr. Gobrecht in his endeavours to render the present volume a manual of anatomy of the most sterling character. The duties of the publishers have also been discharged in a most satisfactory manner; and we feel confident that this new edition of so valuable a book cannot but meet with the same degree of favour which has always characterized the appearance of its predecessors. J. H. B.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Results of an extended Inquiry into the Quantity of Carbonic Acid evolved from the Lungs under the Influence of various Agents.*—Dr. Edward Smith, in a communication to the Section of Physiology of the British Association for the Advancement of Science, stated that he had conducted a series of experiments extending over several months, and found, by his new instrument, that the quantity of carbonic acid expired varied most materially under the influence of different kinds of food, different states of the atmosphere, etc. The paper went into an inquiry—first, as to the quantity of carbonic acid expired in twenty-four hours, with the variations hour by hour; second, the influence of season; and third, the influence of nearly all ordinary articles of food and of a few medicines. During the summer, respiration is always feeble, as compared with the colder months of the year; and although the skin exercised most important functions, he found that it was not vicarious for the lungs in the expiration of carbonic acid; for while the lungs expired 600 grains, the skin threw off only six grains. The increase in the quantity of carbonic acid was greater and more enduring after eating oatmeal and rice than after partaking of arrowroot; whilst wheat produced the greatest quantity, though the increase was less enduring than with oatmeal and rice. Tea, coffee, and cocoa were found to be respiratory exciters, and consequently increased the waste of the system; they could not be classed as food; but as tea induced perspiration, it was most valuable as a remedy against the action of heat. Tea caused the evolution of much more carbon than it supplied. Tea would also be useful in cases of drowning and interrupted pulsation. Brandy, sometimes administered in cases of drowning, had the very opposite effect to that desired, being a non-exciter of pulsation; whereas tea increased the action of the lungs and skin. If the object were to prevent the waste of the system, then alcohol might be useful, and tea would be improper; but if they wished to refresh themselves, tea should be taken. The experiments made showed that those who were more susceptible of injurious influence by heat were the least able to bear any change of climate; and if this were borne in mind, it would be found of service to those who might contemplate going abroad—to the East or elsewhere.—*British Medical Journal*, Oct. 23, 1858.

2. *Experiments on Digestion.*—Dr. Geo. Harley read a communication on this subject before the Section of Physiology of the British Association for the Advancement of Science, at its late meeting in Leeds.

The communication was illustrated by numerous experiments showing the properties of the saliva, the gastric juice, the bile, and the pancreatic secretion. The author stated that, contrary to an opinion lately published by Bernard, he had found that the human saliva contains both sulphocyanide of potassium and

iron. The latter substance, however, can only be detected after the organic matters contained in the secretion are destroyed by burning. Dr. Harley had ascertained that a person of nine stone secreted between one and two pounds of saliva in twenty-four hours. The gastric juice, the author said, does not destroy the power possessed by the saliva of transforming starch into sugar; consequently, the digestion of amylaceous food is continued in the stomach. The gastric juice has the property of changing cane into grape sugar. The author made some remarks upon the cause of the gastric juice not digesting the living stomach; and said that his experiments showed that it is not the epithelium lining the organ which prevents its being digested, but the layer of thick mucus which covers its walls. When the latter substance is absent, the gastric juice attacks the walls of the living stomach, and digests them, causing perforation and death. As regards the bile, it seems that this secretion takes an active part in rendering the fatty matters of our food capable of being absorbed into the system. The most curious of all the digestive fluids, however, is the pancreatic secretion, for it unites in itself the properties of all the others. It not only transforms starch and other such substances into sugar, but it emulsionizes fats, and even digests protein compounds. As a remedy in indigestion, pancreatine should be greatly superior to pepsine, which can only digest one kind of food, namely, protein. The author said he had been labouring to obtain pancreatine in a perfectly pure state, and had been to a certain degree successful. With pancreatine, we should be able to digest any kind of food we pleased; and, therefore, the obtaining of it in a state of purity would prove an invaluable boon to suffering humanity.—*British Med. Journ.*, Oct. 16, 1858.

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3. *On the Form of the Eyeball, and the Relative Position of the Entrance of the Optic Nerve in different Animals.*—Mr. THOS. NUNNELEY read an interesting paper on these subjects before the British Association for the Advancement of Science, at its recent meeting at Leeds.

It was well known, he observed, that the orbits are much larger than the eyeballs, and that their axes diverge considerably in an outward direction, while those of the two eyes are perfectly parallel. The eyeballs lie in the fore-part of the orbits, and, according as they are more or less prominent, and more or less covered with the lids, do they appear to be larger or smaller. The eye of the infant is larger, in proportion to the size of the body, than that of the adult; but it is by no means certain that the eye of the male is larger, proportionately to the size of the body, than the eye of the female. By some anatomists, the human eye was described as a spheroid, the diameter of which, from before to behind, is greater than in any other direction. He had measured a great number of eyes, of the human subject as well as of animals, and he found that wherever there was a departure from the spherical figure, it was in a direction contrary to that which had been commonly stated. In some instances, the difference between the two diameters was scarcely perceptible; in all where a distinction was observed, the transverse was the greatest. He had prepared a set of tables, which were printed, containing the result of the measurement of two hundred eyes of various creatures. The measurements, he thought, clearly proved that, whatever part the fibres of the optic nerve play in the phenomena of vision, the greatest number of them are distributed on that part of the eyeball where there is the greatest range of vision, and that the greatest expanse of retina is on that part of the ball opposite to where objects are placed, and, consequently, it is where the visual image of them must fall. That this was a fact, a careful comparison of the position of the eyes in the head, the size of the eyeball, and the exact position of the entrance of the nerve into it, with the mode of life and habits of various creatures would render obvious. Man, from the erect position of his body, the horizontal placing of his eyes, and his habits, has a more panoptic range than any other creatures (of course, in this consideration, all motions of the head, neck, and body of the animal must be excluded, and those of the eyeballs alone admitted). In him, the optic nerve enters the ball not far from the centre, leaving, however, a somewhat shorter space on the inner and lower parts of the retina than on the upper and outer. Now, while man enjoys a free range of vision above the horizontal line, there are far more occasions for him to look

at objects below than above this line; and thus more visual images are projected to the upper and outer sides of the entrance of the optic nerve than to the inner and lower sides of this spot. In the pig, who sees at no great range before him, and who seeks his food with the snout almost always on the ground, whose head and eyes are consequently, for the most part, downwards and near to the ground, the nerve enters the ball more outwardly, and much lower, than it does in man. The pig wants not to see far before him, but he does require, while grubbing, to look behind him from whence danger comes. So with the timid herbivorous animals. In the bullock and sheep, who pass much time with the head in a dependent position, near to the ground, in open plains, the upper and inner sides of the retina are much larger than the lower and outer portions; while in the deer, who live in more wooded places, where danger is from the front, but who, like the bullock, has the head downwards in feeding, though the inner or anterior side of the retina is still larger than the posterior, it is so to a much less extent than it is in the bullock, while the upper portion still continues as proportionally large as it is in sheep and bullocks. On the contrary, in the horse, who is not preyed upon, who carries the head erect, and observes all around, the nerve enters the eye more nearly in the axis. In birds, with few exceptions, the upper portion of the retina is much more considerable than the lower parts; but the anterior and posterior portions vary much in different genera. Those whose locomotion is performed principally by the feet, and whose range of habitation is very small—as the common fowl and turkey—have the inner or anterior portion very considerably greater than the outer or posterior. Those birds whose range is greater, and who use the wings for progression, but do not wander very far—as the grouse and partridge—have a much less difference in the two portions of the retina; while in those birds whose flight is far and prolonged—as the crow, rook, swan, goose, and duck—the entrance of the nerve is very nearly in the centre of the ball. So in reptiles: in the turtle, who only requires to see immediately before and under him, the outer and upper portion of the retina are very much the larger. In the more active alligator, frog, toad, and chameleon, while the upper portion maintains its size, the outer and inner parts are more nearly equal. In those creatures whose habitation is, for the most part, under ground—as the shrew and the mole—the eyes are so small as to have led Magendie to assert that the mole is without the organs altogether; but this is not the fact, for all the essentials of an eye exist, even true retinal elements, optical nerve, and a well-developed choroid; yet the organ is so minute, and concealed by the skin and hair, as probably only to enable the creature to discern the light, which is all that it requires; for, living under ground, where it seeks its prey, it obviously must depend on the acuteness of other senses, rather than of sight, for its living. Though in the individual there is usually some proportion between the size of the eye and the body, taking different classes and genera, the size of the animal is very little guide to that of the eye, the proportions between the two being determined by other considerations than that of the bulk alone of the creature; for though, as a whole, the eye in fish bears a larger proportion to the whole body than it does in other divisions of the animal kingdom, and the eyes of birds are, as a class, much larger than those of mammalia or reptiles, yet amongst the different genera of all these classes, there are very great differences, determined, apparently, by the following considerations, amongst others not so obvious: When the creature lives in feeble light, yet moves actively about, and is guided in its locomotion by the sense of sight—as in nocturnal birds and animals, and fish—the eye is very large, apparently to take in a large quantity of the feeble light; on the contrary, where the creature is guided in its movements by other senses, then the eye is very small—as in the bat, the mole, the shrew, and the eel. Where vision penetrates to a long distance, and where the eye enjoys great power of overcoming the aberration of the parallax, the eye is large—as in rapacious birds. When the brain and intellect are more developed, the size of the eye diminishes, and the two eyes become more parallel—as in man and the higher mammalia. Where animals are feeble, timid, have but little defensive power, and are preyed upon, the eye is usually very large—as in the hare, the conies, the whole deer tribe, and many other of the ruminants. Where the animal is not predaceous, and its size and strength are such as to protect it from

being preyed upon, the eyes are commonly small—as in the whale and the elephant. In the latter, the eye is even smaller than it is in the horse, and scarcely larger than in the eagle.—*British Med. Journal.*

4. *The Structure of the Choroid Coat of the Eye, and more particularly the Character and Arrangement of the Pigmentary Matter.*—Mr. THOS. NUNNELEY read a paper on this subject, before the Physiological Section of the British Association, at its meeting in Leeds. The choroid coat is the dark tissue interposed between the delicate sentient retina and the hard, dense, sclerotic, and co-extensive with the latter. It begins at the entrance of the optic nerve by a round aperture, with a distinct edge, in close apposition with the nerve, but not organically connected with it, and passing forward as far as the junction of the sclerotic and cornea, where, as choroid proper, it terminates. It there comes in connection with the ciliary circle or muscle, the ciliary body and the iris. The choroid is essentially a vascular membrane, being made up of bloodvessels, colouring matter, and a modified white fibrous tissue. The choroid universally provided the pigmentum nigrum, and is of a deep bronze colour, approaching to black. The pigment was described as consisting of two distinct forms of cells—on the inner surface the choroid, of true hexagonal cells, and in the tissue, and on the posterior surface, of stellate cells. The use of these cells was to destroy the light as soon as it had acted on the retina; and they were the most perfect absorbers of light of any substance in nature that Mr. Nunneley knew of. From the account he gave of the arrangement of the pigment, it afforded what he considered a satisfactory anatomical explanation of an abnormal condition of the eye which had hitherto not been understood—*muscæ volitantes.* The figures of those motes he believed to resemble exactly portions of the choroid coat when teazed out; and they might be expected to appear and disappear with the varying condition of the vessels arising from disordered stomach, or the cerebral circulation, and be cured by whatever corrects those conditions; or the muscæ might result from different organic changes in the choroid coat, which are incapable of being removed.—*British Med. Journ.*, Oct. 9, 1858.

MATERIA MEDICA AND PHARMACY.

5. *New Modes of Administering Iodine.*—Efforts have lately been made in France to administer iodine in a more efficacious manner than had hitherto been done. M. LERICHE, of Lyons, has published valuable articles in *L'Union Médicale*, wherein he endeavours to show that iodine, combined with vegetable substances, advantageously replaces cod-liver oil. He proposes a syrup made of the juice of water-cress and iodine, and also an iodine wine. The syrup has the advantage of not fermenting, and contains exactly one grain of iodine per ounce. The wine is composed thus: Bordeaux wine eight ounces; concentrated infusion of red roses about thirteen drachms; tincture of iodine one drachm and a half. Each ounce contains one grain of iodine. From one to six tablespoonfuls may be given daily, according to the indications and the age of patients. In the space of three years M. Leriche treated 38 scrofulous patients with the wine; 21 were perfectly cured after a treatment steadily pursued for some time; 8 did not improve at all; and 9 improved but slightly, either because the treatment was carried on imperfectly, or because it was left off too soon.

M. BOINET, on the other hand, well known by long-continued investigations respecting the use of iodine, read, on the 28th of September last, before the Academy of Medicine of Paris, a paper, in which he proposes to use iodine as an article of food. The author administers iodine as found in nature, viz., combined with those plants which contain the greatest quantity of the alkaloid. The latter being thus given in minute doses, in a continuous and almost imperceptible manner, yields most advantageous results. M. Boinet uses fuci, marine plants, cruciferæ, salts containing iodine, and some mineral waters holding iodine in

solution. His excipients are ordinary bread, ginger-bread, cakes, biscuits, chocolate, wine, beer, syrups, &c., some being especially calculated for children. Trials were begun by M. Boinet as far back as 1849, upon subjects suffering very severely from the various well-known scrofulous symptoms, and most of them were cured after continuing the iodized food for several months. The author has not found that iodine administered for a long time produced a loss of flesh and atrophy of certain organs. Far from having these effects, the iodine, in his hands, has invigorated patients, and favoured the development of organs. Messrs. Chatin and Trousseau are to report upon the paper.

6. *Therapeutic Properties of Sarsaparilla.*—Dr. A. M. Adam, in speaking of Prof. Böcker, of Bonn ("Medical Notes from the Continent," &c., in *Edinburgh Med. Journ.*, Oct. 1858), states his most recent pharmaco-dynamic experiments, "which, I believe, are as yet unpublished, have been made with sarsaparilla. He informed me that, after carefully performing ninety-eight experiments with this drug on healthy people, he found that, contrary to all our usually received opinions on the subject, it possesses neither diuretic nor diaphoretic properties. Another series of twenty-six experiments, on the persons of uncured syphilitic patients, gave exactly the same results. Böcker also satisfied himself that sarza does not increase the efficacy of the agents, such as iod. potass., etc., which are usually given along with it; and that the good results obtained by the administration of this salt, dissolved in decoction of sarza, are in no degree attributable to any virtue in the solvent fluid. I told Dr. Böcker that I remembered hearing Professor Syme, many years ago, express his opinion on the utter uselessness of so expensive a drug as sarza, remarking, in his own quaint, forcible style, that he believed an "infusion of hay" would be just as good, and a vast deal cheaper. He seemed amused, and said that he entirely agreed with Syme; that infusion of sarza had no greater effect on the system than so much common tea; and that we must regard it merely as a pleasant, but very expensive, vehicle for the administration of other medicines."

[Our own clinical observations have led us to the same conclusions as have been arrived at by the learned professor of Bonn, as to the utter absence of any therapeutic properties in the sarsaparilla.]

7. *Formula for Jaser's Itch Ointment.*—Of all the ointments proposed for the treatment of scabies, one of the most efficacious, and at the same time least expensive for charitable institutions, is that of Jaser. Dr. Delaharpe, Physician to the Hospital at Lausanne, has used it for many years, with invariable success. The following is the formula:—

Flowers of sulphur, half an ounce; sulphate of zinc, one and a half drachms; powdered root of white hellebore, one drachm; soft soap, thirteen drachms; hog's lard, two ounces. M. Delaharpe has been for some years in the habit of adding half a drachm of essence of caraway. The latter addition has perceptibly increased the activity of the ointment, without rendering it more irritating. A pound of the preparation costs about two shillings. Each patient requires for cure, on an average, about eight ounces.

Would it be possible, asks Dr. Delaharpe, to diminish the number of substances entering into the composition of the ointment, without impairing its antipsoric properties? I fear not. The fat and the potash soap modify one another, and by increasing the proportion of soap, we should run the risk of having too irritating an ointment. I propose, however, to make some experiments on this point. The sulphate of zinc does not here act a corrosive part, as might be supposed, for it is completely decomposed by the potash of the soap; it thus serves to form a little sulphate of potash, and by degrees some sulphuret of zinc, by contact with the sulphur. The sulphuret of zinc may have an antipsoric action, at the same time that it contributes to diminish the itching and irritation of the skin, as zinc ointments in general do. A little hydro-sulphuric acid, which is disengaged from the sulphuret of zinc, may also favour the action of the ointment.

The powdered hellebore and essence of caraway are the most active antipsoric

elements of the compound; their quantities could not be diminished without disadvantage—I should rather endeavour to augment them; but perhaps we should have to dread a general action, in consequence of the absorption of veratria.

Further, we must not forget that the most essential condition in the treatment consists in the administration of a soap-bath on the admission of the patient. If soft soap of good quality be employed, and if the patient takes care to brush or rub his skin strongly in the bath, the cure is effected in twenty-four hours. The want of these precautions, I have more than once observed, is sufficient to delay the cure by a day at least. This explains a fact which has often surprised me—the greater obstinacy of the itch in females, notwithstanding the usually greater delicacy of their skin. Through modesty or want of management they do not rub themselves sufficiently in the bath, and the action of the ointment is retarded.—*Dub. Hosp. Gaz.*, Nov. 15, 1858, from *Bull. Gén. de Thérap.*, 15th June, 1858.

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8. *Glycerole de Gourdon and Oil of Cade, as a topical application in Eczematous and Impetiginous Eruptions.*—Modern observers have classed resinous and empyreumatic substances, so extensively employed by the ancients, among the most powerful local applications in the treatment of herpetic eruptions. Thus, purified tar, mixed with lard, in the proportion of from fifteen to forty-six grains of the former to an ounce of the latter, is daily used in the wards of the Hôpital St. Louis, as the resolvent, *par excellence*, of scaly eruptions, and as a valuable desiccative in chronic, eczematous, and impetiginous affections. Since the introduction of glycerine into therapeutics, M. GIBERT employs this substance as an excipient, in preference to lard. To facilitate its use, the mixture is thickened by the addition of starch, according to M. Garot's process. This preparation possesses the advantage over ordinary ointments made with a fatty excipient, that it is removable by water.

The following is the formula of the *glycérolé de goudron:* Glycerine, one ounce; purified tar, half a drachm; add, with the aid of heat, powdered starch, half an ounce. With this quantity of starch, which has been determined by M. Lecocq, we obtain an ointment of thin consistence, and easily spread. The mixture should be perfectly homogeneous.

This application allays itching (we have seen it rapidly succeed in cases which had for several years resisted various and numerous remedies); it dries up excoriations, checks exhalation, dissipates slight cutaneous phlegmasiæ; it acts, in a word, as an astringent and resolvent, without producing irritation. Thus, eczema rubrum, impetigo, intertrigo, prurigo of the scrotum and anus, acne rosacea, and sub-inflammatory mentagra, are, under its influence, most advantageously modified.

Another product very well known to the readers of this journal is the *huile de cade* (oil of pitch). M. Gibert uses it very frequently; but as the empyreumatic properties which this resinous oil possesses in a much higher degree than tar, are such, that it can seldom be applied in a state of purity, he usually mixes it with oil of sweet almonds, or with cod-liver oil. The following preparation is used, under the name of *huile cadée*, in the Hôpital Saint Louis: Cod-liver oil, two parts; oil of pitch, one part. This application possesses very efficacious resolvent and drying properties. M. Gibert has seen eczemas, which had continued for months in a stationary, red, excoriated, and oozing condition, notwithstanding the external and internal use of preparations of sulphur, heal under the influence of this combination.

It is particularly in cases of obstinate pruriginous, papulous, and eczematous eruptions of the anus and genitals, which are so often such a source of annoyance to both the patient and the practitioner, that M. Gibert has most frequently derived benefit from the local use of oil of pitch. He adds, in such cases, the diligent use of cold hip-baths, and, in order to alter the diathesis on which the eruption depends, the internal employment of Dr. Boudin's arsenical liquor, modified in the following manner: Distilled water, one pint; arsenious acid, three-quarters of a grain; dissolve with the aid of heat. The mixture is divided into six vials, each of which is marked for two days; half a bottle to be taken for a

dose each morning, fasting, with the addition, at the moment the medicine is swallowed, of a cup of chicory, sweetened with honey. M. Gibert states that he has seen eruptions of several years' standing, and which had resisted the use of thermal waters, and of many other remedies, yield in a few weeks to this mode of treatment.—*Dub. Hosp. Gaz.*, Nov. 15, from *Bull. Gén. de Thérap.*, 15th Aug., 1858.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

9. *On the Nomenclature and Classification of Continued Fevers.*—Dr. Chas. Murchison, in an interesting paper (*Ed. Med. Journ.*, Oct. 1858), of which this is the title, remarks that all the divisions of continued fevers have, for the most part, been founded upon their symptoms, or their supposed anatomical origin, but that he is convinced that the most philosophic classification must be one based on their etiology. Dr. M. maintains that the class of continued fevers comprises three, or, in all probability, four, distinct species, originating from widely different causes.

"First, there is *typhus*, the grand predisposing cause to which is destitution; while the exciting cause, or specific poison, is generated by overcrowding of human beings, with deficient ventilation, and afterwards is propagated by contagion. Hence it is, that epidemics of typhus occur during seasons of famine, and in besieged cities; and hence it is, that we find it limited to the most overcrowded localities of large towns, and seldom meet with it in country districts, or in the upper classes of society.

"Secondly, there is the '*relapsing fever*,' about which there may still be some doubts as to its specific distinction from typhus. There can be no question that it differs widely from that disease, both in its symptoms and mortality; and also, that a previous attack of the one confers no immunity from a subsequent one of the other. On the other hand, relapsing fever is found to prevail, as epidemics, at the same times, and under the same circumstances, as typhus. Researches are still wanting as to the distinctive etiology of these two fevers; but I have grounds for believing that it will yet be shown that relapsive fever is produced by famine alone; typhus by destitution and overcrowding combined; in other words, that destitution and starvation are the predisposing causes of typhus, the exciting causes of relapsing fever.

"Thirdly, there is *typhoid* or *enteric fever*, which is less contagious than either typhus or relapsing fever, and which is quite independent of the causes which give rise to these, being generated by the putrid emanations from decaying organic (animal) matter. The grounds for this opinion may be briefly summed up as follows:—

"1. Previous attacks of either typhus or relapsing fever confer no immunity from subsequent attacks of typhoid fever (and *vice versâ*).

"2. There is no authenticated proof that the poison of typhus has ever generated typhoid fever (nor *vice versâ*).

"3. Typhoid fever does not prevail in wide-spread epidemics. It is essentially an endemic disease; or when it does become epidemic, such epidemics are always of the most limited and circumscribed character.

"4. Typhoid fever is always most prevalent in autumn, or after a long continuance of hot weather. A hot autumn, after a wet summer, appears to afford the most favourable conditions for its development.

"5. Typhoid fever is not, like typhus and relapsing, limited to the poor, but is met with among poor and rich alike.

"6. Typhoid fever is not confined to overcrowded localities, but appears alike in the most dense and in the least populous districts of large towns, and even in isolated houses in the country.

"7. There is evidence, of the most conclusive nature, that typhoid fever may result from the emanations from (animal) organic matter, in a peculiar state of

decomposition.[1] In every instance where 'fever' has been described as originating from such a cause, the fever has been typhoid. The reason why this cause is not more generally recognized, is the want of attention to the distinctions between the different fevers. Those who deny that 'fever' can be the result of putrid emanations, adduce thousands of cases of typhus and relapsing fever as negative evidence, in the same way as there are not wanting a few who bring forward typhoid cases to prove that fever cannot be the result of destitution and overcrowding.

"Fourthly, there is *simple fever*, or *febricula*, which is non-contagious, and arises from such non-specific causes as exposure to the sun's rays, fatigue, surfeit, etc. In its simplest form, this fever may terminate in twenty-four or thirty hours, as in the ephemeral or diary fever of systematic writers, or it may be prolonged to eight or ten days, as in the ardent or sun fever of tropical climates."

All the continued fevers which have been described by authors under so many names, may be referred, according to Dr. M., to one or other of the four species already spoken of. Dr. M. has arranged the following synonyms under each of the fevers to which he believes they belong. He likewise gives briefly the leading distinctive characters of each fever.

I. Typhus Fever.

Characters.—A disease generated by contagion, or by overcrowding of human beings, with deficient ventilation, and prevailing in an epidemic form in periods, or under circumstances of famine and destitution. Its symptoms are: more or less sudden invasion marked by rigors or chilliness; a small, weak, usually frequent pulse; dry, brown tongue; in most cases, constipation; skin warm and dry; a morbilliform rash, appearing between the fifth and eighth days, frequently accompanied by true petechiæ, and lasting until death or recovery; great and early prostration; delirium coming on early, and for the most part low and wandering; contracted pupils; duration of the fever usually about 14 days, seldom or never more than 21. In the dead body, no specific lesion, but great congestion of all the internal organs.

Synonyms.

Τυφος (Hippoc.?);[2] Febris typhodes? (*Prosp. Alpin.*, 1611; *Recalchus*, 1636; *Juncker*, 1718); Typhus (*Cullen*, 1769); Enecia Typhus (*Mason Good*, 1817); Typhus and True Typhus (*modern English writers*).

Febris pestilens (*Galen? Fracastorius*, 1546; *Forestus*, 1591; *Riverius*, 1623; *Willis*, 1659; *Sydenham*, 1668); Febris epidemica (*J. Burserius*, 1625); Pestilential Fever (*Grant*, 1775; *Stoker*, 1826); La constitution epidémique (*Beaulac*, 1810); Epidemic Fever *pro parte* (*recent writers*).

One of the morbi contagiosi (of *Fracastor.*, 1546); Febris contagiosa (*Coytterus*, 1578); Infectious Fever (*Lind*, 1763); Der ansteckende Typhus) *J. V. Hildenbrand*, 1810); Typhus contagieux (*J. C. Gasc*, 1811); Das ansteckende Nervenfieber (*Horn*, 1814); Contagious Fever (*Bateman*, 1818); Tifo contagioso (*Rossi*, 1819); Contagious Typhus (*English writers*).

Febris putrida et maligna, Synochus putris and S. cum putredine (*old authors*);[3] Febris maligna pestilens (*Riverius*, 1623; *Sennertus*, 1641; *Willis*, 1659); Malignant Fever (*Langrish*, 1735; *Fordyce*, 1791); Febris continua prutrida (*Boerhave*, 1738; *Wintringham*, 1752); Putrid Malignant Fever (*Huxham*, 1730); Febris exanthematica, maligna, et venenosa, et perniciosa (*J. F. Bianchini*, 1750); Febris maligna (*Le Roy*, 1771); Putrid Fever (*Macbride*, 1772); Febris continens putrida (*Selle*, 1770); Das Faulfieber (*Hecker*,

[1] In addition to the evidence upon this point which will be found in my essay in the *Medico-Chirurgical Transactions*, I may allude to the circumstance, that Dr. Barker, of Bedford, has recently succeeded in producing in animals symptoms very similar to those of typhoid fever, by making them inhale the noxious principles arising from cesspools.—*The Influence of Sewer Emanations*, by T. Herbert Barker, M. D. London, 1858.

[2] Probably a different disease.

[3] Previous to the time of Huxham and Pringle, the terms putrid and malignant were frequently applied to all fevers, except the simple or febricula.

1809); Febbre putrida (*Ital.*); Fièvres putrides et malignes, *pro parte* (*Fr.*); Typhoid Fever, with putro-adynamic character (*Copland*, 1844).

Febris contagiosa in carceribus genita (*Huxham*, 1742); Jail Fever (*Pringle*, 1750; *Heysham*, 1782; *John Howard*, 1784); Typhus Carcerum (*Sauvages*, 1764); Febris carceraria (*Burserius*, 1785); Jail distemper (*J. C. Smyth*, 1795); Maladie des Prisons (*Fr.*).

Malignant Fever of the Hospital (*Pringle*, 1752); Febris nosocomialis (*Burserius*, 1785); Fièvre des Hôpitaux (*Fr.*).

Pestis bellica and Typhus bellicus (*var.*); Morbus Castrensis vel Morbus Hungaricus, *pro parte* (*many old authors*); Morbus qui ex castris in Bavariam penetravit? (*Rhumelius*, 1624); Febris Castrensis (*Willis*, 1659; *Haller*, 1742); Febris militaris (*Petri*, 1665); Typhus Castrensis (*Sauvages*, 1768); Camp Fever (*Grant*, 1775); Die Kriegspest (*Hufeland*, 1814); Typhus des camps et des armées (*Louis*, 1829).

Febris pestilentialis nautica (*Huxham*, 1752); Ship Fever (*Lind*, 1763; *Grant*, 1775); Febris nautica (*Burserius*, 1785).

Febris pestilens quam Cuticulas vel Puncticula vocant (*Fracast*, 1546; *Forest*, 1591); Tabardillo et Puntos? (*De Torres*, 1574); Febris purpurea epidemica? (*Theræus*, 1578); Febris petechialis (*Salius Diversus*, 1584; *Sennert*, 1641; *Selle*, 1770; *Burserius*, 1785); Febris peticularis (*Roboretus*, 1591); Febris purpurata? (*Riverius*, 1623); Febris pulicaris seu puncticularis (*Pet. A. Castro*, 1650); Morbus puncticularis (*Donkers*, 1686); Febris petechialis ver (*Hoffmann*, 1700); Das Fleckenfieber (*Ettmuller*, 1726; *Reuss*, 1814); Spotted Fever (*Strother*, 1728); Morbus cum petechiis (*Strack*, 1786); Febbre petecchiale (*Rossi*, 1802; *Rasori*, 1812); Morbo petecchiale (*Acerbi*, 1811; *Palloni*, 1817); Typhus exanthematicus and Das exanthematische Nervenfieber (*of German writers*); Petechial Fever (*Peebles*, 1835); Petechial Typhus (*var.*).

Febris asthenica? (*var.*); Febris attacta *pro parte* (*Selle*, 1770); Fièvre ataxique *pro parte* (*Pinel*, 1798); Fièvre adynamique (*Pinel*, 1798; *Roux*, 1813); Adynamic Fever (*Stoker*, 1826; *Burne*, 1828); Brain Fever? (*var.*).

II. Relapsing Fever.

Characters.—A contagious disease, which is apparently generated by destitution, and which is only met with in the epidemic form during seasons of scarcity and famine. Its symptoms are: a very abrupt invasion, marked by rigors or chilliness; quick, full, and often incompressible pulse; white tongue; tenderness at the epigastrium; vomiting, and often jaundice; enlarged liver and spleen; constipation; skin very hot and dry; no characteristic eruption; high-coloured urine; severe headache, and pains in the back and limbs; restlessness, and rarely subacute delirium; an abrupt cessation of all these symptoms about the fifth or seventh day; after a complete apyretic interval (during which the patient may get up and walk about), an abrupt relapse on the fourteenth day from the first commencement, running a similar course to the first attack, and terminating on or about the third day of the relapse; mortality small, but occasionally death from sudden syncope; after death, no specific lesion, but usually enlargement of liver and spleen.

Synonyms.

A five days' Fever with Relapses (*Rutty*, 1770); Short Fever, Five days' Fever, Seven days' Fever (*var.*, 1843); Relapsing Fever (*Paterson*, *Steele*, etc., 1847; *Jenner*, 1849).

Fever of the New Constitution (*O'Brien*, 1828.)

The Epidemic Fever (*auct. var.*); Epidemic Fever of Edinr., 1817 (*Welsh*, 1819); Epid. Fev. of Ireland *pro parte* (*Barker and Cheyne*, 1821); Scotch Epidemic of 1843 (*Wardell*, *R. Cormack*, *Alison*, *Jackson*, *Henderson*, *Craigie*, *etc.*); the Silesian Fever of 1847 (*Brit. and For. Med. Ch. Rev.*, July, 1851).

Epidemic Remittent Fever (*Mackenzie*, 1843), Gastric Fever with Remittent type (*Craigie*, 1843); Gastro-hepatic Fever (*Ritchie*, 1855); has also been designated—Mild Yellow Fever, Bilious Relapsing Fever, Bilious Remittent Fever, Remitting Icteric Fever.

Irish Famine Fever (*Stoker*, 1826; and *Dublin Journal*, 1849); Die Hungerpest (*Graevell's Notizen*, 1848).

Dynamic or Inflammatory Fever (*Stoker*, 1826); Synocha (*Christison*, 1840 *and* 1858).

Relapsing Fever, in all probability, constituted one of the varieties of the Inflammatory Fever, or Synocha, of the writers of last century; in more recent times, on the other hand, it has not unfrequently been considered a variety of Typhus.

III. PYTHOGENIC FEVER.

Characters.—An endemic, slightly contagious disease, generated by putrefying organic (animal) matter. Its symptoms are: an invasion often insidious, or marked by slight rigors, a sensation of chilliness, or profuse diarrhœa; pulse usually frequent and soft, but variable in the same patient; tongue red and fissured, in a few days becoming dry and brownish; in most cases, but not invariably, increased splenic dulness, tympanitis, abdominal tenderness, gurgling in the iliac fossæ, and diarrhœa, with or without melæna; urine copious and pale; skin warm, often with irregular sweats; an eruption of rose-coloured papules, first appearing between the seventh and fourteenth days, and coming out in successive crops, each of which lasts two or three days; very rarely petechiæ; frequently epistaxis; prostration coming on late, and often slight; delirium active or often absent; dilated pupils; the disease often protracted to the thirtieth day, and occasionally, though rarely, followed by a relapse of all the symptoms, including the eruption; after death, ulceration of the solitary and aggregated glands of the ileum, and enlargement of mesenteric glands.

Synonyms.

Typhus mitior and Synochus *pro parte* (*Cullen*, 1769); Abdominal Typhus (*Autenrieth*, 1822, and *German writers generally*); Fever with affection of the abdomen (*Alison*, 1827); Fever with ulceration of the intestines (*Bright*, 1829); Synochus and Typhus with abdominal affection (*Southwood Smith*, 1830); Fièvre Typhoide (*Louis*, 1829; *Chomel*, 1834); Typhoid Fever (*Stewart*, 1840; *Bartlett*, 1847; *Jenner*, 1849); Mild Typhoid Fever (*Copland*, 1844); Enteric Typhus (*Christison*, 1850).[1]

Πυρέτος ἡμιτριταίος? (*Hippoc.*); Hemitritæus? Tritœophyas? and Triphodes? (*auctor. antiq. var.*); Febris semitertiana seu composita (*Galen? Forestus*, 1591; *Spigelius*, 1624); Tritœophya typhodes (*Mangetus*, 1695); Frigerari? (*Sagar*, 1776).

Infantile Remittent Fever (*var.*); Febris hectica verminosa (*Vander Bosch*, 1769); Febris verminosa (*Selle*, 1770); Infantile Gastric Remittent Fever (*Locock*, 1840); Infantile Hectic Fever and Worm Fever (*var.*).

Febris non-pestilens? (*Forestus*, 1591); Morbus Hungaricus and Febris Hungarica *pro parte* (*Auct. var. et Sennertus*, 1641); Endemic Fever (*var.*); Autumnal or Fall Fever (*Austin Flint*, 1852; *and in America generally*).

Febris putrida (*Riverius*, 1623); Febris putrida quæ vulgo *lenta* appellatur (*Willis*, 1659); Febris putrida nervosa? (*Wintringham*, 1752); Febris putrida aut biliosa (*Tissot*, 1758); Febris a putredine orta (*A. Tralliani quoted by Burserius as Sny. for his Feb. gastric. ac.* 1785).

Febris lenta (*Forestus*, 1591; *Willis*, 1659; *Linnæus*, 1763; *Vogel*, 1764); Febris hectica sive lues neurodes (*Willis*, 1667); Slow Fever (*Strother*, 1716; *Langrish*, 1735); Nervous Fever (*Gilchrist*, 1734); Slow Nervous Fever (*Huxham*, 1739); Febris chronica? (*Juncker*, 1736); Febricula, or Little Fever, commonly called the Nervous or Hysteric Fever, the Fever on the Spirits, Vapours, etc. (*Manningham*, 1746); Febris atacta *pro parte* (*Selle*, 1770); Febris lenta nervosa maligna (*Burserius*, 1785); Irregular low nervous Fever (*Fordyce*, 1791); Fièvre ataxique *pro parte*, and F. adéno-méningée (*Pinel*, 1798); Nervenfieber? (*Bischoff*, 1814); Fièvre nerveuse (*Fr.*); Common Continued Fever (*Armstrong*, 1816); Low Fever (*var.*).

Febris petechizans vel Spuria (*Hoffman*, 1699).

Febris biliosa (*Galen? River.* 1623; *Stahl*, 1700; *Juncker*, 1736); Bilious Fe-

[1] Clinical Lectures, not published.

ver (*Pringle*, 1750; *Rutty*, 1770); Febris biliosa putrida (*Selle*, 1770); Febbre biliosa (*Benelli*, 1775); Synochus biliosus (*Sauvages*, 1763); Febris cholerica? (*Vogel*, 1764); Bilio-gastric Fever (*Copland*, 1844); Gastro-bilious, and Bilious continued Fever (*modern writers*).

Febris catarrhalis? (*Bockel*, 1580; *Crause*, 1676); Febris colliquativa? (*J. R. Fortis*, 1668); Febris Stercoralis? (*Quesnay*, 1753); Morbus mucosus (*Rœderer and Wagler*, 1762); Febbre glutinosa gastrica (*Sarcone*, 1765); Febris mucosa (*Selle*, 1770); Febris pituitosa (*Stoll*, 1785); *Strack*, 1789); Febris colliquativa primaria seu essentialis (*Burserius*, 1785); Morbus biliosus mucosus (*Knaus*, 1786); Febris pituitosa nervosa (*Jacobi*, 1793); Schleimfieber? (*Canz*, 1795); Fièvre muqueuse (*Fr.*), Mucous or Pituitous Fever (*Copland*, 1844).

Febris gastrica (*Ballonius*, 1734); Febris acuta stomachica aut intestinalis (*Heister*, 1736); Febris gastrica acuta (*Burser*, 1785); Fièvre méningo-gastrique (*Pinel*, 1798); Gastrisches Fieber (*Richter*, 1813); Fièvre gastrique (*Dict. des Sc. Méd.*, 1816); Epidemic Gastric Fever (*Cheyne*, 1833); Gastric Fever (*Craigie*, 1837).

Febris mesenterica maligna (*Baglivi*, 1696; *Hoffmann*, 1728); Febris intestinalis et mesenterica (*Riedlin*, 1705); Febris mesenterica acuta (*Burchard, quoted by Burserius*, 1785); Fièvre entero-mesenterique (*Petit* and *Serres*, 1813); Enteritic Fever (*Mills*, 1813); Gastro-enterite (*Broussais*, 1816); Entero-mesenteric fever (*Abercrombie*, 1820); Dothinenterite (*Brétonneau*, 1825; *Christison*, 1840); Enterite Folliculeuse (*Forget*, 1841); Muco-enteritis and Gastro-enteric Fever (*var.*); Enteric Fever (*Wood*, 1848; *Aitken*, 1858).

Many of the cases decribed by Cullen and his successors, under the designation Enteritis erysipelatosa, were probably examples of this fever.—(See description of it in Alison's *Path. and Pract. of Med.*, p. 323.)

IV. Simple Fever or Febricula.

Characters.—A sporadic, non-contagious disease, arising from exposure to the sun, fatigue, surfeit, inebrity, etc. Its symptoms are: frequent, full, and firm pulse; white tongue; great thirst; constipation; high-coloured urine; very hot and dry skin; no eruption; severe headache, and sometimes acute delirium; the fever subsiding in from one to ten days, with copious perspirations, herpetic eruptions, etc. In the dead body, congestion of all the internal organs.

Synonyms.

Καῦσος (*Hippoc.*); Causus sive Febris ardens (*Galen, Willis*, 1659; *Boerhave*, 1738); Synochus causonides (*Forestus*, 1591; *Mangetus*, 1695); La Calentura? (*Piquer*, 1751); Causos (*Vogel*, 1764); Endemical Causes (*Mosely*, 1789); Enecia Cauma (*Mason Good*, 1817); Ardent Fever (*Burnett*, 1812; *Ranald Martin*, 1841; *Copland*, 1844); Ardent Continued Fever (*Morehead*, 1856).

Σύνοχος? (*Greeks*); Synocha vel Synochus Simplex (*Riverius*, 1623; *Hoffmann*, 1700; *Juncker*, 1736; *Burserius*, 1785); Synocha (*Linnœus*, 1763); *Sauvages*, 1768; *Cullen*, 1769); La Fièvre Synoque (*Davasse*, 1847); Synoshische (*Germ.*).

Synochus imputris? (*Galen*); Febris continua non putrida (*Lemmius*, 1563; *Boerhave*, 1738); Synocha sine putredine (*Sennertus*, 1641); Synocha non putris (*Bellini*, 1732); Febris non putrida (*Quavin*, 1781).

Συνεχὴς φλεγματῶδες? (*Gr.*); Febris Sanguinea? (*Avicenna*); Synocha sanguinea? (*Sennertus*, 1641); Feb. acuta sanguinea (*Hoffmann*, 1700); Febris venosa (*Ballonius*, 1734); Acute Continual Fever (*Langrish*, 1735); Simple Inflammatory Fever (*Huxham*, 1739; *Fordyce*, 1791); Febris acuta simplex (*Storck*, 1741); Synocha plethorica and Ephemera plethorica (*Sauvages*, 1716); Febris continens inflammatoria simplex (*Selle*, 1770); Febris acuta (*Ploucquet*, 1791); Entzundungsfieber and Entzundliche Fieber (*Reil*, 1794, etc.); La Fièvre angioténique (*Pinel*, 1798); La Fièvre angioténique pure et simple (*Bouillaud*, 1826); Fièvre inflammatoire (*Fr.*); Febbre inflammatoria (*Ital.*).

Febris depuratoria? (*Quesnay*, 1753); Judicatoria? (*Sagar*, 1776).

Febris septenaria (*Platerius*, 1656; *Sprengel*, 1814); Ephemera plurium dierum (*Sennertus*, 1641; *Juncker*, 1736); Synochia septimo die soluta (*Hoffmann*, 1700); Febris continens? (*Stahl*, 1700); Febris continua simplex (*Lieutaud*, 1776); Simple Continued Fever (*modern writers*).

Febris ephemera (*Riverius*, 1623; *Sennertus*, 1641; *Sauvages*, 1768); Diary Fever (*Strother*, 1728); Ephemera simplex (*Boerhave*, 1738); Febris diaria (*Juncker*, 1736; *Linnæus*, 1763); Fièvre ephemère (*Davasse*, 1847); Febricula (*var.* and *Jenner*, 1849, not the Febricula of *Manningham*); Das entägige Fieber (*Germ.*); Effimero (*Ital.*); Efemera (*Span.*).

Ephemera a frigore and E. a calore (*Sauvages*, 1768); Sun Fever (*Scriven*, 1857).

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10. *Symptoms and Treatment of Diphtheria.*—We find in the *Lancet* (Nov. 6th) the following remarks, by Dr. C. D. KINGSFORD, on the symptoms and treatment of diphtheria, a disease which is now prevailing epidemically in some parts of England.

"Diphtheria may be divided into the *mild* and the *severe* forms.

"The mild form, which, for the sake of distinction, may be designated the *diphtheritic sore-throat*, is ushered in by a variable amount of feverishness, loss of appetite, and at first only slight pain in swallowing; the tongue presents a thick, white, creamy coat, through which some of the papillæ are visible; the velum palati, uvula, and pharynx are of a bright-red colour; the tonsil glands are much swollen and of the same livid hue, and upon the inner side of one or both of them distinct white patches are seen, which in some instances resemble an exudation from the sulci of the tumid gland, but more frequently are flat and filmy in appearance, not confined to the tonsils alone, but spread over the uvula and posterior wall of the pharynx: both the exudation and the filmy deposit adhere tenaciously to the submucous surface, and cannot easily be scraped off. Ulcerative stomatitis not rarely precedes and accompanies this mild form of diphtheria; indeed, by some, they are considered to be identical; the parotid and submaxillary glands are not much swollen, although one or two enlarged glandulæ concatenatæ may often be detected.

"The severe form, or *genuine diphtheria*, is always characterized by a high state of fever, a hot pungent skin, flushed countenance, congested lips, a rapid feeble pulse, great difficulty in swallowing, and hurried respiration; the tongue is covered by a thick, dirty, yellowish-brown, or sometimes slaty-coloured coat; the velum palati, uvula, and pharynx are of a deep, dusky *erysipelatous* redness; the tonsils usually enormously swollen and of the same dark-red colour, but instead of the white patches observed in the mild form, a large ash-coloured membrane is spread over the inner side of one or both tonsils, and also upon the uvula and posterior wall of the pharynx. As the disease advances, the above symptoms increase in severity; the breathing becomes stertorous from mechanical obstruction; deglutition so painful that young children will refuse to swallow even liquids; the saliva dribbles from the mouth, and a foul, acrid discharge often flows from the nares; the pulse becomes more rapid and feeble; the glands of the neck are now swollen and tender, and the voice is hoarse and indistinct; the patient, restless, tosses about in the bed, or else lies on his back in a semi-comatose state. These cases, when fatal, terminate either by rapid prostration of the vital powers, or by an affection simulating croup, from extension of the diphtheritic membrane into the air-passages; in both instances, death is usually preceded by obstinate vomiting, probably the result of inflammation or irritation of the par vagum.

"The prognosis must, at all times, be very guarded, but will depend much upon the disease being from the first recognized and energetically treated; for the mild form, if left alone or improperly handled, will quickly pass into genuine diphtheria, when the prognosis becomes more unfavourable, although modified by the duration of the disease, and the age and temperament of the patient.

"The treatment is divided into constitutional and local, and varied according to the severity of the case. Even in the mild form, or diphtheritic sore-throat, it will be found advisable, in the first instance, to confine the patient to bed in a well-ventilated room; if the bowels be sluggish, a brisk calomel purge should be given, but under no circumstance should any other antiphlogistic measure be

resorted to, but a liberal diet at once enjoined, consisting of strong beef-tea, port wine, jellies, and farinaceous food, which ought to be administered at short intervals, and in moderate quantity. The following draught to be taken every three or four hours: Chlorate of potass, from ten to thirty grains; dilute hydrochloric acid, ten to thirty minims; decoction of bark or water, half an ounce to an ounce. The dose of the salt and mineral acid to be increased according to the age of the patient. The topical treatment consists of sponging the fauces, two or three times a day, with the compound solution of alum (L. P.), (by means of a piece of soft sponge attached to the end of a pen-holder, or portion of whalebone); the patient also, if not too young, should gargle frequently with a strong solution of alum. The speedy removal of the white patches, by this local application of alum, renders highly probable the suggestion that the deposit is a fungus.

"In treating the severe form, or *genuine diphtheria*, it is most important to guard against being misled by the feverish excitement, and thereby be induced to adopt antiphlogistic remedies. It should be borne in mind, that the fever is the result of a poison analogous in type to adynamic erysipelas; and as it would be unwise to treat the latter disease by lowering the system, so would any depleting means, for the purpose of reducing the fever attending diphtheria, be fraught with danger. The pharynx should be sponged every eight hours with a solution of lunar caustic (sixteen grains to an ounce of distilled water), and for this purpose the sponge, by being easily compressed between the swollen tonsils, will be found preferable to a brush. A most liberal allowance of wine and nutritious diet must be instituted from the first, and the following draught: Chlorate of potass, from ten to thirty grains; tincture of sesquichloride of iron, ten to thirty minims; syrup, a drachm; water, seven drachms: given every one, two, or three hours, according to the age of the patient and the degree of pyrexia present; the more intense the inflammatory symptoms, the oftener should the draught be exhibited; nourishment also should be given in definite quantities at short intervals. It will happen, not unfrequently, with very young children, that *some time* before the mechanical obstruction precludes deglutition, all voluntary efforts at swallowing will be obstinately resisted, from pain, and disinclination to be aroused. These cases excite the greatest anxiety, as unless a sufficient quantity of support can be taken, the vital powers must quickly succumb to the influence of the poison. Still all attempts to give medicine or food by the mouth should now be discontinued, and an enemata of strong beef-tea and port wine (one ounce of each) be administered, per rectum, *every two hours;* also, for a child above three years old, five grains of quinine should be added to each alternate injection. At bedtime, to procure rest, it may be advisable to add five minims of Battley's sedative. The glysters may be thickened with arrowroot; and, at intervals, milk substituted for the beef-tea and wine. The quantity injected should never exceed two or three ounces at a time (or it will fail to be retained): and hence the necessity for the frequent repetitions. The topical application of the nitrate of silver must be persevered in, and the patient allowed to sip any nutriment he will. By adopting this procedure, time is gained, and life maintained until the virulence of the poison is overcome or exhausted. Mercury, in any form, excepting as a cathartic at the onset of the disease, seems to be especially contra-indicated. Blistering and external stimulants to the neck are worse than useless, by adding to the irritability of the sufferer, without exercising any beneficial or derivative effect upon the fauces.

"Tracheotomy, if entertained, should be adopted immediately the croupy symptoms have become established, and not deferred as a *dernier ressort.* The presence of vomiting I should consider sufficient proof that the disease had already advanced too far to warrant any hopes of success from an operation.

"It will be observed that the plan of treatment above advocated is based upon that of two analogous affections, viz., ulcerative stomatitis, and acute asthenic erysipelas of the head and neck; viewing the mild form of the disease as allied to stomatitis, and therefore prescribing the chlorate of potassa with the mineral acid; but regarding diphtheria as a complication of diphtheritic sore-throat with erysipelas, and hence ordering, in addition to the salt, large and frequently-repeated doses of the sesquichloride of iron.

"The question of infection is very difficult of solution, yet the rapid spread of the malady in schools, and the recorded deaths of several members of a family from this disease, render it imperative that every precaution be used to prevent its dissemination. In some cases, I have most conclusively traced the origin of the disease to emanations from putrid, stagnant ponds and sewers.

"When the affection of the throat assumes the malignant or putrid type, which is recognized by a livid, gangrenous appearance of the tonsils, and by an intolerable fetor of the breath, the treatment recommended for genuine diphtheria, with the addition of a gargle consisting of one drachm of liquor chloride of lime and eight ounces of water, will be found most serviceable.

"A very serious complication occasionally arises as a sequela to the severe form of diphtheria, viz, paralysis of the muscles of the neck, of the pharynx, and of the larynx. Dr. Gull, who has already drawn attention to this subject, informs me that he has met with a case in which the upper extremity was involved; and this morning I was consulted by Mrs. C——, who was recovering, not only from loss of speech and of deglutition, but also from partial blindness, and paralysis of both arms, the result of this formidable complaint. These cases are to be treated upon tonic principles, by change of air, and those remedies which are calculated to improve the general health. The nervine tonics are especially indicated. When the head falls forward upon the chest, from paralysis of the spinal accessory nerve and cervical plexus, great relief and comfort will be afforded by a collar of soap plaster spread upon leather. Also when, from paralysis of the glosso-pharyngeal nerve, the efforts to swallow are attended by violent fits of choking, all medicines and a large proportion of nourishment must be administered per rectum. Even under the most favourable circumstances, recovery will be slow and gradual; but when the phrenic nerve is implicated, the greatest danger to life is threatened.

"In fatal cases, the *post-mortem* examination reveals the ash-coloured membrane spread over the pharynx, extending into the posterior nares and down the œsophagus; but when death is preceded by symptoms of croup, it is found also in the larynx and trachea. Upon detaching this membranous exudation, the submucous surface presents an ecchymosed appearance, but no distinct signs of ulceration.

"In conclusion, I would most strongly urge the importance of injections in the treatment of the severer forms of diphtheria; nor ought they to be delayed until the patient is unable to swallow, but administered as soon as he ceases to take a sufficient quantity of nourishment. I would likewise beg to add my conviction, that if a sthenic plan of treatment were adopted from the very commencement of an attack, the mortality from this now much dreaded affection would be greatly reduced."

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11. *Diphtheria.*—Dr. SEMPLE called the attention of the Medical Society of London (Oct. 18) to a disease which was exciting very great interest at the present moment—namely, diphtheria. He had been called down to Bagshot to see some cases of the affection in question, but he arrived about an hour after one of the patients had died; he found that the last fatal case was the third in the same family, the ages of the patients being respectively eight, twelve, and fifteen years. They died at intervals of about a week from each other; and while the funeral service was being performed on one of the children, the death of the elder sister, aged fifteen years, occurred. Dr. Semple, however, repaired to the house of the patient, in company with Dr. Blount, of Bagshot, and obtained permission to make a *post-mortem* examination of the body, although to a limited extent. It was to be regretted that very few examinations after death had been obtained in this disease, owing to the prejudices existing in the minds of relatives, especially in the rural districts. However, nearly the whole of the diseased parts were examined, consisting of the tonsils, a part of the tongue and of the pharynx, the epiglottis, the larynx, and the trachea. On the whole of these parts the pellicular membrane, which is the characteristic feature of true diphtheria, was developed. It was especially necessary to bear this character in mind, because he (Dr. Semple) had reason to believe that, at the present time, many affections of the throat were confounded together, under the name of diphtheria, which had

very little relation to that disease—at least in the sense in which the term was used by Bretonneau, Trousseau, Guersent, and other French writers. It was especially necessary to exclude the scarlatinal sore-throats from the category of diphtheria, because scarlatina has no real or necessary connection with the diphtheria epidemic. Other cases which were confounded with diphtheria were, ulcerated sore-throats of various kinds, cancrum oris, and even common tonsillitis. The true distinction existing between diphtheria and the malignant ulceration of the tonsils in scarlatina, was to be found in the circumstance that the pellicular exudation in diphtheria may be readily removed by the handle of the scalpel, leaving the mucous membrane below it, congested indeed, but smooth and entire; whereas, in the malignant ulceration of scarlatina, the substance of the tonsils is actually eaten away and destroyed. The fatality of diphtheria was quite appalling. Dr. Blount had attended eight cases of the disease in the vicinity of Bagshot, and every one of them had been fatal. Both the commencement and the termination of the disease were marked by peculiarly treacherous and insidious characters; the symptoms, at first, are so slight, that they are hardly noticed by the patients or their relatives; and when medical advice is at last sought for, the pellicular exudation has generally reached the air-passages, when the hopes of recovery are almost extinguished. The termination of the disease is equally insidious, and is often quite unexpected; for although many patients die with symptoms of well-marked asphyxia, yet others perish suddenly by syncope while they appear to be going on favourably. Dr. Semple was remarkably struck with the circumstance that the locality, where these fatal cases occurred, was not one where one might expect, *à priori*, a malignant and fatal form of disease to prevail; for the surrounding country was open and beautiful, the soil dry, and the patients by no means placed in unfavourable hygienic conditions; nor did it seem that the disease attacked ill-fed or puny children, for the girl, whose body was examined, was a plump, well-formed person, moderately fat, and with good muscular development. The therapeutical management of this disease was a question of the greatest importance; but, hitherto, all kinds of treatment had been attended with very unsatisfactory results; the best treatment was, unquestionably, the application of strong caustics to the throat, at the very earliest possible period; and the hydrochloric acid, used in a concentrated form, was, perhaps, the best. This appears to stop the progress of the false membrane into the air-passages, and acts somewhat in the same manner as concentrated nitric acid in preventing the progress of phagedænic ulceration. When the false membrane has reached the trachea and bronchi, there is very little hope for the patient. The next question is whether, when this extension has taken place, the operation of tracheotomy—so much extolled and so extensively practised by the French physicians—ought to be resorted to. In the case of the girl at Bagshot, Dr. Semple was prepared to recommend the performance of the operation, if he had found the patient alive, and if she were suffering from the symptoms of asphyxia; but the *post-mortem* examination proved that this proceeding must have been useless, for the false membrane was so extensively spread over all the air-passages that it could not have been removed, and it had so little tenacity that it would have broken under the forceps used for extracting it. True diphtheria, therefore, was characterized by the presence of the false membrane at the back of the mouth, extending, in the fatal cases, into the air-passages, and causing death by obstructing the respiration; and it differed, on the one hand, from those sthenic forms of throat-disease which were accompanied by full pulse, and the other ordinary signs of acute inflammation; and, on the other hand, it differed from those low forms of throat-disease which arose from a depressed habit of body, from unclean and unhealthy habitations, and from the complication of scarlatina. The extensive prevalence of diphtheria, at the present day, demanded the attention of the Government; it was sweeping off great numbers of the rural population, both young children and persons verging on puberty; and from the peculiarly insidious nature of the early symptoms, it was generally neglected until remedial means were of no avail.—*Lancet*, Oct. 23d, 1858.

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12. *On the Non-Periodical Fluctuations in the Frequency of Pneumonia, especially during the Years* 1836–1856.—Dr. W. Ziemssen, Lecturer at the University

of Berlin, presents the results of very extensive statistical inquiries, embracing mortuary registers of twenty-two localities scattered over different parts of the world. The variation of frequency differs very considerably; the average mortality from pneumonia in different places in England, Germany, France, Switzerland, Denmark, and America being 1.53; the maximum mortality in the same places from this disease was found to be 2.00, the minimum 1.08 per cent. On the other hand, the variation in the same place from year to year is very considerable; the author calculates that, assuming the mortality from pneumonia to be called 100, the annual fluctuation must be regarded as 19 per cent. This fluctuation does not appear to depend upon the influence of climate. From an examination of our Registrar-General's Reports, Dr. Ziemssen gathers that the fluctuation of the mortality from pneumonia in London does not vary as much as diseases which occur epidemically—as measles and hooping-cough; while it varies much more than that produced by diseases which result chiefly from constitutional and general vital conditions. On examination of the curves representing the mortality of the twenty-one years (from 1836 to 1856), we find that the years 1838, 1840, 1842, 1844, 1847, 1849, 1851, 1853, and 1856 are characterized by a very high position in the scale, 1847 being the year in which the highest point was attained. The years 1836, 1839, 1841, 1845, 1846, 1848, and 1850 were the years in which the mortality was lowest. The fluctuations occurring during these twenty-one years may be regarded as a pneumonic wave, rising from the year 1836 to 1838, and then falling to 1839; again rising to 1840, and falling in 1841; a third elevation occurs, reaching its climax in 1842, and gradually sinking till 1846; the next elevation in 1847 is followed by a slow descent till 1850; a fifth rise takes place in 1851; a sixth, with the climax in 1853 and 1854; and a seventh, of which the highest point was in 1856. We can only find space to dwell upon one or two of the author's remarks.

The year 1847 was a generally pneumonic year. Of nineteen European localities of which the author has obtained the statistics, twelve attained their absolute maximum during this year; they comprised Great Britain, France, Germany, Scandinavia, and Eastern Russia; none of the localities show a diminution during this year. It appears from a statement by Dr. Rigler, which does not, however, possess absolute statistical accuracy, that during the same year pneumonia attained an unusual height in Constantinople; and, again, according to Dr. Stratton, that the same was the case among the North American Indians.

With regard to the relative mortality of the two sexes, it appears that females bore the exact ratio to the general variations that characterized the different years. On examining the different ages, greater fluctuations were found to occur in childhood than in adult life; and it also happened that a pneumonia epidemic prevailing among children scarcely affected grown-up people; while, *vice versâ*, an unusual number of pneumonias might affect adults, and not be equally apparent among the children of the same locality.

In a brief review of the other diseases prevalent at the same time, Dr. Ziemssen concludes that the prevalence of pneumonia bears no proportion to the prevalence of other inflammatory disorders; and that, therefore, to determine the epidemic constitution, it is advisable to classify diseases, not according to nosological divisions, but according to the organs affected.

The whole paper is one of great value, and deserves to be specially studied by those who wish to appreciate the bearing of statistics upon the geography and history of diseases. Before concluding this notice, we would also draw attention to another careful essay by the same author, on the periodical fluctuations of pneumonia during the different seasons of the year;[1] in which he shows that while considerable variations take place in different localities as compared with one another, each locality presents great uniformity as to the comparative frequency of pneumonia at certain seasons.—*Brit. and For. Med.-Chir. Rev.*, Oct., 1858, from *Vierteljahrschrift für die Praktische Heilkunde,* 1858, Band ii.

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13. *Clinical Illustrations of the Pathology and Treatment of Delirium Tremens.*—This is the title of a very interesting paper (*Edinburgh Med. Journ.*,

[1] Archiv für Physiologische Heilkunde, Jahrgang 1857, Drittes und Viertes Heft, p. 393.

Oct. 1858) by Prof. Thos. Laycock. The diversity of opinion in the medical profession, he remarks, as to the nature, causes, and treatment of delirium tremens, is remarkable. On one point only is there an approach to unanimity, and that is as to the propriety, and even necessity, of administering full doses of narcotics to induce sleep. Prof. L.'s experience has long convinced him of the dangers of this practice, and of the fallacies of the theories which have led to it. He has never, he states, witnessed a fatal case in which they had not been administered, while he has known many cases recover very happily without them.

"Delirium tremens," he observes, "is usually understood to be a disease consequent upon the sottish or excessive use of alcoholic or fermented drinks, and characterized by tremors of the limbs, disordered intelligence, hallucinations, and sleeplessness. These leading symptoms may supervene upon other causes, as starvation, fever-poisons in the blood, wounds, epileptic attacks, albuminuria; but in these cases the delirium has another name; or opium, Indian hemp, tobacco, etc., may, in rare instances, induce them.

"But it is only a few of those who drink hard that have delirium tremens at all; while those drunkards who have it are subject to it paroxysmally, or suffer only occasionally under certain conditions. It is of primary importance, therefore, to determine what those conditions are. Now, as the disease is one of cerebral disorder, we may conclude that they have reference, 1, to the condition of the brain or of its vessels; 2, to the condition of the blood circulating therein; 3, to the condition of important viscera in close relation with morbid conditions of the blood or of the brain. Under these three heads may be classed, as follows, the more important of the predisposing and exciting causes of the disease, *i. e.*, the *conditions* necessary to an attack: 1. *Conditions of the brain or of its vessels.* (*a.*) Habitual stimulation from any cause, whether it be (1) more materially and mechanically by drugs, as spirits, wine, malt liquor (with its constituent adulterating drugs), or opium, ether, etc.; or (2) psychically, as from over-thought, over-work mentally, continued anxiety, strong emotions, sexual indulgence. (*b.*) Constitutional predisposition to irregular cerebral action, known as the nervous temperament, and characterized by a predisposition to 'nervousness,' insanity, epilepsy, and other convulsive diseases, neuralgia, etc. To this class of patients the oinomaniacs or dipsomaniacs belong. (*c.*) Conditions the result of antecedent or actually existing (but insidious) disease of the brain or its membranes, such as attacks of 'brain fever,' infantile disorders affecting the brain or membranes, tuberculosis, and especially the sequelæ of mechanical injuries done to the cranium, and which at the time of occurrence attracted perhaps little attention. (*d.*) Recent injuries to the head received in the drunken state, or diseases affecting the brain especially, which have come on very recently, as masked gout, certain forms of bronchitis, pneumonia, and pericarditis; inflammation of the liver, spleen, kidneys, etc. 2. *Conditions affecting the blood.* (*a.*) Alcohol in the blood predominantly. (*b.*) Defective supply of nutrient materials in the blood, consequent on loss of appetite or inability to digest food. (*c.*) The presence of fever-poisons. (*d.*) Retained excreta, as carbon or carbonic acid, bile, urea, etc. 3. *Conditions of important viscera.* (*a.*) Inflammatory affection; gastritis is hardly ever absent; duodenitis, with constipation, frequent; hepatitis and chronic nephritis, or chronic congestion of the kidneys, by no means rare. (*b.*) Structural diseases—as of the liver (fatty degeneration, cirrhosis), of the stomach (chronic thickening, ulceration), and of the kidneys (Bright's disease, cirrhosis), may be looked for. These are mentioned as predominant conditions; they do not exclude, however, other causes of morbid change in the brain and in the blood, and which are presented in cases of delirium tremens in great variety. Perhaps the most noticeable and important is the sleeplessness so constantly observed to precede and accompany the delirium, and which, itself a result of morbid changes, is usually, in its turn, a cause of those further morbid changes in the brain upon which both it and the delirium depend.

"Now, the treatment of delirium tremens consists essentially in the treatment of these conditions; this object being satisfactorily attained, the symptoms cease, *i. e.*, a cure is effected."

Prof. L. adduces a number of cases illustrative of these views.

The general rules laid down by Prof. L. for the treatment of delirium tremens are as follows:—

"1. The patient should be placed in as complete a state of muscular repose as possible. To this end, he should, if practicable, be kept in bed. Muscular activity necessarily exhausts the nervous system; hence, quiet of the muscular system facilitates repair of nervous energy. If, however, the patient cannot be kept in bed without mechanical restraint, it is on all hands allowed to be better not to restrain him, as the waste of motor power is much greater in the continued attempts the patient makes to keep from restraint, than in his usually quiet wanderings after unmeaning objects. Should his delirium be of the violent kind, absolutely needing restraint, chloroform would, perhaps, be in general a safer remedy than the strait waistcoat, although not a remedy to be administered without serious consideration as to its fitness in each case. 2. All *sensational* stimuli should be removed, and all emotions, agitating thoughts, or anxieties, be prevented. 3. Food of a suitable kind should be carefully given from time to time; no alcoholic stimuli of any kind administered as articles of diet, unless specially indicated. 4. Where there is a tendency to diaphoresis, it should be encouraged as an eliminatory process. 5. The surface, and especially the feet, should be kept comfortably warm. If the head be hot, the hair may be cut short, and a gentle douche, for one or two minutes, applied every three or four hours; this is rarely necessary, however, much less shaving the scalp. 6. An experienced nurse must attend the patient. But, above all, it is essential that the practitioner be clear in his etiological diagnosis, so that he may be knowing as to the powers of nature. He should first determine whether the patient be under the influence of alcohol or not, and ascertain clearly whether there be any important complications. If the patient be alcoholized, and no important complications be discovered, he may consider the sleeplessness and delirium as of no great pathological importance, and calmly and confidently await the result of a few days' judicious watching and general management of the case."

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14. *Uses of Bleeding in Diseases.*—Dr. W. O. MARKHAM read (Nov. 23) an instructive paper on this subject before the Royal Med. and Chirurg. Soc. The object of the author is to show, that by arguing from certain admitted facts respecting the effects of bleeding, a rule of practice may be deduced, indicating the right application of the remedy in diseases. The conclusions at which he arrives are these:—

1. There is no proof that venesection has any *directly* beneficial influence over the progress of inflammations, either *external* or *internal.* On the other hand, the injurious effects of large bleedings, especially in those inflammations in which the integrity of the lungs is seriously compromised, have been often demonstrated.

2. Nevertheless, venesection is, at times, of great service *indirectly* in the course of inflammations, and of all other diseases which occasion congestion and oppression of the heart, by removing this *secondary* consequence, which arises accidentally out of the inflammation.

3. In all cases in which venesection is of service, it acts alike—viz., by relieving the cardiac congestion: it neither arrests nor modifies beneficially the inflammatory process.

4. A marked distinction is to be drawn between the effects of bleeding in inflammations and the local abstraction of blood from an inflamed part. Local abstraction of blood materially influences the inflammation, reducing the most characteristic of its phenomena—the pain, the heat, the redness, and the swelling; but it only influences, in this way, *internal* inflammations when there is a direct vascular connection between the part inflamed and the part whence the blood is drawn.

5. It is not denied that local irritation of an external part may influence an internal inflammation (even when there is no direct vascular communication between the skin and the inflamed part) by reflex action, conveyed thence from the skin through the vaso-motor nerves of the inflamed part.

The author demonstrates the inefficacy of venesection over internal inflammations in two ways: 1st, by arguing of what is *seen* of its inutility in external

inflammations; and, 2dly, by the fact of the large and general concurrent testimony of modern experience, which has proved that large bleedings—the only bleedings which have any manifest influence over inflammations—are often very injurious, their good effects being dubious and disputed.

Venesection has been long since abandoned in the treatment of external inflammations because of the danger and inutility of the practice; and though less easily traced in the case of internal inflammations, the same conclusion has gradually forced itself on the minds of observers. The practice is no longer regarded as essential in their treatment; but the author cannot believe that physicians have been during so many ages, and still are, acting under a delusion as to the services rendered by venesection in internal inflammations. He, therefore, endeavours to explain the discrepancy by assuming the position—that venesection, as regards internal inflammations, is of service, not through any *direct* influence which it exercises over the inflammatory process, but in consequence of its removing certain of the *secondary* consequences which arise accidentally out of the inflammation—to wit, the oppressed and congested condition of the heart. He asserts that venesection is never required excepting when this congestion of the heart exists; but at the same time observes, that there are congestions of the heart, and periods in the course of all congestions, in which no relief can be hoped for from the remedy. Modern experience justifies this position: for venesection is rarely ever practised now, except in those diseases in which this congested condition of the heart necessarily plays a prominent part. The benefits, indeed, of venesection become more clearly manifest in proportion as the disease for which it is practised produces a higher degree of this congestion. As illustrations of this, cases are related in which the original disorders, provoking this congestion of the heart, lay respectively in the heart itself, in the lungs, in the abdomen, and in the brain. In all of them the same condition of the heart, and the same symptoms were present, claiming a similar treatment. The relief given by venesection, in three of these cases, was immediate and permanent; in two of them, no inflammation existed; and, in the third—one of pneumonia—the venesection had no influence over the *inflammation* of the lung, for the stethoscope demonstrated that the portion of the lung inflamed was in the same condition of consolidation the day after as on the day of the bleeding; in the fourth case—injury of the head—the man was not bled, and died, the immediate and only apparent cause of death found being extreme congestion of the heart and lungs. Bleeding, it is believed, would have saved this man's life.

He offers the same explanation of the benefit of venesection in wounds of the lung, long before inflammation exists, and he thinks that the same circumstances explain the relief of the pain often attendant on pneumonia, which, while occasionally due to pleurisy, he thinks more frequently produced by cardiac congestion—a pain which is sometimes felt when there is no pleurisy, or may not be felt when pleurisy is present. In certain conditions of disease of the heart and great vessels, in injuries of the head and apoplexy, and even in peritonitis, the benefits occasionally following venesection may all, he thinks, be referred to the relief of cardiac congestion. Such an explanation seems to the author clear and simple, and in complete accordance with our physiological knowledge and our practical experience; and with reference to any other beneficial and direct actions, which venesection is supposed to exert over inflammation, he observes that all our knowledge of the effects of venesection has not yet enabled us to show what those other actions are; and all our modern experience manifestly tends to prove that venesection has no directly beneficial influence over inflammations, but that, if large, it acts injuriously by weakening the system, which has to sustain the force of the inflammatory process.

If the facts here maintained be correct, then it necessarily follows that the objects of, and indications for, venesection become clear and definite, and that a rule of practice may be established from their consideration. It also follows that venesection is now-a-days less frequently practised than is desirable; that it must have been of service in other days, just as it is of service now; that it is requisite now, just as it was requisite then.

The author next refers specifically to venesection in pneumonia. Here there are two main special facts to be considered, which contraindicate to a certain

extent the venesection: 1st, the diseased condition of the lungs, which produces the cardiac congestion, cannot be removed by the bleeding; 2d, the loss of blood is, so long as the pneumonia lasts and in proportion to its extent, an irreparable loss. Hence it follows that the more extensive the inflammation, and the more urgent the symptoms, the greater is the danger of venesection; and in fact, just in proportion as the bleeding is more required to relieve the heart, is the practice of it less applicable. In pneumonia, the function of the chief sanguificating organ of the body is arrested; and therefore to take away blood at such a time is to take away what cannot be restored so long as the inflammation lasts. The loss of blood, which might be borne with impunity in other inflammations, seriously compromises the future of the patient in this inflammation of the lungs. Venesection, he says, is applicable in pneumonia when the general symptoms have arisen rapidly and are severe, aud when the inflammation is limited, as in the case related—when the urgency of the symptoms is, so to say, out of proportion to the extent of the inflammation, as measured by the stethoscope; that is, when the aërating processes are not seriously and extensively compromised. The object of the venesection is, in all cases, to relieve the heart from its temporary embarrassments. When the congestion of the organ is the consequence of its own partial paralysis, then of course venesection cannot restore to it its equilibrium.

With reference to the local abstraction of blood, Dr. Markham points out the importance of vascular connection between the skin and the inflamed organ, reasoning from the analogy of external inflammations. The benefit of leeches in pneumonia he refers simply to the attendant inflammation of the parietal pleura, and thinks that in pericarditis the pain is frequently due to concomitant pleurisy, and is relieved in the same way. In endocarditis he believes they can be of no service; nor can they draw one drop of blood from an inflamed liver or kidney. In such cases he conceives that the benefits attributed to cupping or leeches may be due to other remedies employed at the same time.

The author makes no attempt at any explanation of the mode of action of either venesection or local abstraction of blood. If the facts stated be true and rightly interpreted, their practical deduction may be accepted, without waiting for any theoretical explanation of them.

Dr. Mayo, after passing a high eulogium upon Dr. Markham's paper, proceeded to say that he agreed in the main with the views advanced by the author, in respect both to local and general bloodletting. He thought, however, that we required more definite information as to the necessity of general bleeding in cases of apoplexy. He related a case in which forty ounces of blood were taken away with immediate and complete relief. General bleeding was now almost prohibited in apoplectic seizures, but he thought that this was to some extent a mistake. He agreed with the author that the simultaneous employment of bleeding and stimulants was not inconsistent in certain diseases; for whilst the one unloaded the vessels, the other exhilarated, and by the combined measures the circulation was accelerated. Even in neuralgic cases, the administration of steel was occasionally rendered beneficial by the previous extraction of a few ounces of blood. In periostitis, bleeding often rendered the disease more manageable, and in a case of severe sprain of the ankle, attended by intense neuralgia, which had come under his observation, the extraction of seventy ounces of blood by Mr. Wardrop had been followed by complete relief. The life of the patient was, however, for some time placed in jeopardy.

Dr. J. A. Wilson alluded to the great prevalence of bleeding a quarter of a century ago. He thought that in certain cases, even of intense hysteria, bleeding might be applied with great advantage. He related a case of this affection, which had supervened upon great mental emotion, and the symptoms of which were immediately arrested by the copious abstraction of blood. Bloodletting was not resorted to in inflammation for the direct relief of that condition, but to restore action and relieve the circulation. Bleeding, in years gone by, was no doubt carried to an unjustifiable extent, but at the present time he thought we had gone to the opposite extreme. Cases occurred to him in consultation in which the abstraction of eight or ten ounces of blood was evidently indicated, but in which he found the greatest difficulty in obtaining consent to that pro-

ceeding. No general rule could be laid down as to the necessity of abstraction of blood; in fact, every case must be judged of by the symptoms exhibited to the practitioner. He ridiculed the notion which had lately prevailed, that Englishmen were now less able to bear the loss of blood than formerly, because, forsooth, they had degenerated in physical power. No doubt the effects of bloodletting were better understood now than formerly, but he emphatically denied that there was any degeneracy in the constitution of English people. On the contrary, better education and better food had improved the constitution of the people of this country; and if the necessity for the copious abstraction of blood should again arise, they would be found to bear it as well, if not better, than they did in former times.

Dr. Druitt contended that Dr. Markham was wrong in asserting that any single circumstance was sufficient in itself to explain the beneficial effects of bloodletting in inflammation. Theory must succumb to practice; even if theory were advanced in support of the non-abstraction of blood, logical deductions might be drawn to show its fallacy. Thus, if it was contended that blood was in excess, bleeding undoubtedly reduced the quantity of that fluid. If the blood were of too high a specific gravity, bloodletting reduced it. If it was necessary to aërate the blood, bleeding rendered that process more easy.

Dr. Webster differed with the author with respect to local bloodletting in internal inflammations. He had found that in inflammatory diseases of the chest in children the abstraction of the blood by cupping was often of the greatest service. It was nothing new to find the practice vary in regard to the employment of bleeding. It had been the case from the earliest periods; bloodletting being, at one time, almost constantly employed, whilst at another time it was almost completely abandoned. Disease assumed different types at different periods. This might explain why bleeding was less resorted to now than twenty years ago. But it must be remembered that the time might soon arrive when the necessity for bloodletting would again be indicated.

Dr. Sibson coincided in opinion with Dr. Markham as to the importance of local bloodletting in cases of local inflammation. He thought, however, that he had taken too confined a view as to the effects of general bloodletting. Dr. Markham had considered that the pain in pneumonia was attributable to direct obstruction in the heart itself; but this opinion was contradicted by the fact, that the pain in nine cases out of ten was restricted to the seat of inflammation. In these cases the pain was the result of pleuritis, and was relieved by the local abstraction of blood. In pneumonia, bleeding was of essential service, not only from the relief it afforded to the heart itself, but by the influence it exerted in preventing the extension of the disease. But it must be remembered that there were other cases besides those mentioned in the paper, in which bloodletting afforded great relief, independent of removing any obstruction in the circulation of the heart; such, for instance, as in cerebral congestion. Whatever might be said with respect to other inflammations, venesection in cases of pneumonia was beneficial. In most other inflammations, blood could be abstracted by side currents; but in pneumonia, as every drop of blood had to pass through the inflamed lung, and there was a diminished channel for its course, it was incumbent that its quantity should be diminished.—*Lancet, Dec.* 4, 1858.

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15. *Changes produced in the amount of Blood-Corpuscles by the administration of Cod-liver Oil.*—Dr. Theophilus Thompson read (Nov. 18, 1858) a paper on this subject before the Royal Society.

The author had presented to this Society, on the 27th of April, 1854, a communication descriptive of the chemical changes produced in the blood by the administration of cod-liver oil and of cocoa-nut oil, and advanced the conclusion, deduced from chemical analysis, that any favourable result derived from the use of these oils is associated with an increase in the proportion of red corpuscles. The present communication was an extension of the inquiry, but was confined to experiments on the influence of cod-liver oil on the blood. It comprehended the principal details regarding fourteen patients affected with pulmonary consumption in various stages of progress, and the result of analyses of their blood. In two instances no oil had been given; in the remaining twelve

that medicine had been more or less freely administered, and an obvious contrast was noted in the condition of the blood, the proportion of red corpuscles to a thousand parts of blood in the two cases where no oil had been given being respectively 98.20 and 119.64, and in ten of the other patients varying from 142.32 to 174.76. In these ten cases the use of the oil had been attended with marked gain in weight and other evidences of amelioration. In another instance, in which the disease advanced, and a loss of seven pounds in weight occurred, notwithstanding four months' administration of oil, the proportion was 114.39. In one example only was a favourable effect of the oil accompanied with a low proportion of corpuscles, viz, 84.83; but in this patient, hæmoptysis, so profuse as to endanger life by increasing the poverty of the blood, had apparently modified to some extent the ordinary influence of the remedy. The analysis was conducted by Mr. Dugald Campbell in the following manner: The whole quantity of blood abstracted having been weighed, the coagulum was drained on bibulous paper for four or five hours, weighed, and divided into two portions. One portion was weighed, and then dried in a water-oven to determine the water. The other was macerated in cold water until it became colourless, then moderately dried, and digested with ether and alcohol to remove fat, and finally dried completely and weighed as fibrin. From the respective weights of the fibrin and the dry clot that of the corpuscles was calculated.

Dr. COPLAND observed that consumption is a disease which tends to produce a continual waste of blood-corpuscles, and that whatever promotes nutrition and excites the vital forces must have a beneficial tendency in such a disease; for with improved assimilation, there must evidently be a renovation of blood-corpuscles. On this principle, cod-liver oil, he believed, would be found efficacious in anæmia and rickets as well as in consumption, although he was not sure that it had any particular advantage over iron as a remedy.

Dr. GARROD thought that any future researches on this subject would be still more valuable if the analyses were rendered more specific, by ascertaining the proportions not only of the red corpuscles generally, but also of the constituent parts of the corpuscles. Without such information, it was difficult to explain the fact that cod-liver oil is so far more useful in consumption than in anæmia; and it would be desirable to determine the amount of change produced by such a remedy in the proportion of hæmatin, globulin, iron, and fat, entering into the composition of the blood-cells.—*Lancet*, Nov. 27, 1858.

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16. *Narcotic Injections in Neuralgia.*—CHAS. HUNTER, Esq., House Surgeon St. George's Hospital, records (*Med. Times and Gaz.*, Oct. 16th) the following cases of neuralgia treated by narcotic injection into the part, as proposed by Dr. A. Wood, of Edinburgh.

CASE I.—J. G., aged 55, was admitted into St. George's Hospital, July 21, under Dr. Pitman, with tic douloureux. He had been constantly subject to it for four years, with but little intermission; at one time he obtained for a few weeks from seven to eight hours' sleep at night, but with that exception he used always to be in pain day and night, and seldom slept an hour without a violent paroxysm.

On admission he was suffering these repeated violent attacks of pain all over the left side of the face, which caused him day and night to keep up a cry of anguish. Various remedies to palliate the pain were attempted, but unsuccessfully till the 7th of August, when the local injection of morphia was commenced. About one grain and one-third of the acetate of morphia was injected at 8 P.M.; the man fell asleep very soon after, and continued to do so for seven hours. During the next few nights the same dose was regularly injected, and he slept either all night or for several hours.

On the 11th, he was asleep when visited, so no more morphia was injected; he, however, slept two hours; the next few nights the injection was not given; he slept either not at all, or most indifferently.

16*th.* A larger dose was injected into the cheek from within the mouth; he went off to sleep at once, and did not awake all night; he was also easy the whole of the next day; after this the original dose was continued, both night and morning.

20*th.* He sleeps a good deal; has good nights, and two or three hours' sleep

in the day. The paroxysms are now so slight, that often no one except the patient can tell when they are on; no continued pain is felt, and the paroxysms are "sometimes off for half a day, often for several hours."

30th. Until to-day the morphia has been injected night and morning; but, for the present the administration is left off on account of a considerable sized abscess which has been gradually forming the last few days, and which was opened to-day.

The part injected was the gum over a back upper tooth, as that was the most painful part, and the spot which, if touched, always brought on a paroxysm; latterly, the adjacent tissue of the cheek was injected close to the gum.

Thus, not only was sleep procured, but the patient obtained considerable ease during the day while the injection was gone on with. The constant recurrence of the attack of pain was put an end to, and the paroxysms when they did occur were far milder; but a large abscess formed in the cheek.

Case II.—E. P., aged 18, was admitted into St. George's Hospital, July 25, under Dr. Tatum, suffering from excessive neuralgia in the right eye, which was also extensively diseased. As there were no hopes of saving the eye, and the pain was constant, the globe was removed for fear the other eye should also suffer; unfortunately it did, and ran a most rapid course—the lids becoming swollen, hard, thick, and everted; the neuralgia in this eye became even worse than it had been in the other.

All kinds of remedies were tried—aconite, morphia, hyoscyamus, opium, quinia, etc., all failed to give relief; chloroform was then used and frequently, but it only gave her ease and sleep for a few minutes, or at the most an hour or so.

Sept. 9. $\frac{3}{4}$ gr. of morphia (the acetate) was injected under chloroform into the eyelid, but produced no sleep, as sickness (which had commenced in the afternoon after a dose of morphia by the stomach) continued during the night.

10th. No morphia given by the stomach, $1\frac{1}{3}$ gr. injected under chloroform into the eyelid; she went off to sleep for seven hours continuously, which she had not done for some months. She slept also once or twice the next day without chloroform.

11th. Injection repeated 10 P. M.; a part escaped; she slept four hours; had acute paroxysms between the periods of sleep.

12th. Sleep produced by the injection, and the severity of the paroxysms much diminished.

In the next few days the morphia was injected, and gave ease and sleep in proportion to the amount injected; from this time no chloroform was employed while inserting the point of the syringe in the skin.

16th. Slept four hours last night. The pain now is nothing to be compared to what it previously was, the swelling is going from the eye. In the evening nearly three grains of morphia were injected; sleep was immediately produced, and continued eight hours. The next day she was far quieter and easier, and appeared so comfortable at night that no morphia was injected.

18th. No morphia having been injected, no sleep was obtained last night, although a six-hour dose (gr. i.) was continued to be administered by the stomach.

19th. $1\frac{1}{3}$ gr. injected into the eyebrow, gave sleep for several hours at night, and a little in the day; at night two grains were given by the stomach; it gave no sleep, but after an hour or so caused considerable sickness.

Oct. 4. The morphia injection is still continued, and with considerable relief to the patient.

Remarks.—In this patient, then, it appears—

1. That a very great change has been made for the better, the progress of the affection appears arrested; or, at all events, for the present kept at bay; the health of the patient is improved.

2. That the local affection appears so far improved that all the hardness, thickness, and eversion of the conjunctiva have subsided; the pain in the head is very much less, the pain in the eye is far less acute, and the attacks much less frequent, so that sleep is every now and then obtained during the day without medicine.

3. But it must be observed that this girl, like the man, has had abscess as a

result of the local injection; the eyelid, the eyebrow, and the side of the eye, have all been opened for the liberation of matter.

4. It is very interesting to observe, that in this girl the injection of morphia into the cellular tissue was most effectual; but that morphia given by the stomach was of no benefit at all, but always did harm; that general irritation to the nervous system was produced; that sleep hardly ever followed, and was then probably accidental, because so seldom, but that sickness, nausea, giddiness, etc., almost always accompanied its administration by the stomach, whatever the strength of the dose happened to be.

In considering the results of the trial of the local treatment in the two cases, the advantages obtained appear to me to be—

1. That much less constitutional (nervous) irritation attends the local introduction of the narcotic than when it is given by the stomach.
2. That the effect of the narcotic is more immediately produced.
3. The action of the narcotic appears more sure when injected. The exact amount taken into the circulation can be more readily seen, and the risk of contamination or alteration which it is exposed to, given by the stomach, is avoided.
4. It appears to exert more benefit on the local affection when it has to be absorbed from the part affected itself, probably from being brought more directly into contact with the nerves involved in the disease.

On the other hand, there are the disadvantages; these are chiefly—

1. The pain occasioned by the introduction of the fine canula.
2. The chance of the fluid escaping from the wound or puncture.
3. The production of local inflammation, effusion of blood, abscess.

To conclude: are the disadvantages of such import that they ought to preclude the local employment of narcotics by injection? do the advantages preponderate over them? I think they do; and that the disadvantages are only those which, with care and experience, may either be avoided, or much diminished; for instance—1. By employing such a syringe as that used for the perchloride of iron (to inject aneurisms, etc.), with a very fine point to the nozzle, the pain is not more than that occasioned by the prick of a needle. 2. By having the injecting tube no larger than that of such fine syringes, the puncture in the integument is so small that the fluid does not escape. 3. With regard to the formation of abscess; it is only, for the most part, after repeated injections have been made in one place that such happens. One great thing then to avoid it is, to vary as much as possible the exact site to be injected, still injecting in the painful part, or to cease injecting for a time. The necessarily acid state of the solution of the morphia (for it must be strong), is certainly another disadvantage; but as irritation to the integument appears produced, as little acid as possible ought to be employed, and any excess in the solution neutralized by potash. These inconveniences being obviated as much as possible by the means pointed out, I think such advantages as the more rapid introduction of the remedy into the system, the avoidance of constitutional (especially nervous) irritation, the greater certainty of the effect, and the more concentrated effect of the remedy on the painful part ought not to hinder the local treatment of neuralgia from having a fair trial.

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17. *Use of Ox-Gall in Hypertrophies.*—Dr. BONORDEN believes that this remedy is much less employed than it deserves to be. It has usually been employed internally only as a resolvent in chronic constipation, and externally in opacities of the cornea. He believes it exerts a special effect on the metamorphoses taking place in the capillaries; and for that reason is highly efficacious in all forms of hypertrophy. In induration and hypertrophy of the *mamma*, it exerts a surprisingly rapid effect, and in this way tumours and indurations have been dispersed by him, which would have been by others removed by the knife. He usually employs it in combination with olive oil, adding conium if there is pain, and liq. ammon. if there is torpidity. The following formula is very useful: ℞.—Fell. tauri inspiss. ʒiij; ext. conii mac. ʒj; saponis natronat. ʒij; olei oliv. ℥j. M. and rub four times daily. Formerly, he was in the habit of excising hypertrophied *tonsils*, a practice which he has quite left off since he has been aware of the powerful agency of this substance. The gall, rubbed up with water into the consistency of an ointment, is applied by means of a good-sized camel's hair

pencil, twice a day to the entire surface of the tonsil. It causes a slight irritation which lasts about half an hour, and is succeeded by an augmented secretion of mucus. Unpleasant to the patient at first, he soon gets accustomed to it; and indurations which have lasted for years give way under its use in a surprisingly short time. In all hypertrophic affections of the *eye*, as hypertrophic opacity of the cornea, pannus, and staphyloma, the ox-gall does good service. Either the fresh gall may be dropped into the eye several times a day, or it may be applied to it with a pencil. In various other hypertrophies, which are accessible to external applications, we may resort to it, as when they affect the ear, mouth, vagina, uterus, or skin. He suggests its employment in hypertrophy of the heart, in consequence of the remarkable power it possesses of diminishing the action of this organ.—*Med. Times and Gaz.*, Oct. 2, 1858, from *Berlin Med. Zeitung*, No. 6.

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18. *Dropsy treated with Lemons.*—Dr. Trinkowsky, a Russian medical officer, reports that in many cases of dropsy, which he has treated within the last seventeen years, he has observed the diuretic operation of lemons in a most remarkable manner, and even where other remedies had failed.

He is in the habit of directing that a lemon, freed from its skin, should be cut in pieces and sprinkled with sugar, and eaten by the patient. The dose at first to be one lemon in the day, gradually increasing the quantity, so that in one of his recorded cases eighteen were consumed in twenty-four hours. If pyrosis be produced, magnesia is given; and if the bowels be acted on, the use of lemons should be intermitted for a day.—*Dublin Hosp. Gaz.*, Aug. 15, 1858.

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19. *Relative Value of the Different Anthelmintics in the Treatment of Tœnia.*—Dr. Peacock states, that as a general result of his experience, both in public and in private practice, he prefers the oil of male fern to all other remedies, and that he holds the kousso in very light estimation indeed. It appears that of the hospital cases respecting which notes have been preserved, the fern oil was given in thirty-five. Of these, in sixteen no other remedy had been previously tried, and in this group the result was always satisfactory, the animal being expelled in a dead or dying state. In seven cases the oil was given after the partially successful use of kousso, and in all these more of the worm was brought away. In three, after partial success by pomegranate bark, the oil brought away other portions of the parasite, and in one a like result was obtained after the use of the turpentine draught. In six cases in which the oil was used, either the result was not satisfactory, or the patient did not attend again. The dose of the oil given was from half a drachm to a drachm and a half to children, and from a drachm to three drachms to adults.[1]

The cases in which the kameela was given are seven. In five of these no other remedy had been previously tried, and in all these portions of worm (generally quite alive) were expelled. In one the expulsion of worm was caused after kousso had been tried without effect, and in the fifth, which was under similar circumstances, a like negative result followed its use also. In two cases, after the successful employment of the kameela, the oil of fern was employed without procuring the expulsion of any more of the worm. The dose of kameela prescribed was from half a drachm to a drachm for children, and from one to three drachms to adults.

It would from the above facts appear that kameela is more efficient than kousso, but that it must rank as a vermifuge rather than a true vermicide. After the fern oil the animal is usually voided dead. An important statement with regard to the comparative value of kameela, is made by Mr. Henry Callaway, formerly of Finsbury-circus, but now a medical missionary amongst the Zulus. The ka-

[1] We are informed that great care is necessary on the part of the dispenser, in order to avoid disappointment in the use of the oil of fern. Its ethereal solution, which is by far its best preparation, on standing deposits its resinous principle. A prolonged shaking is necessary to secure readmixture. Unless the dispenser pay more than usual attention to this matter, the patient is very likely to get a dose which is but little more than ether.

meela is the native remedy among the Aborigines; but in a letter to the *Pharmaceutical Journal*, Mr. Callaway states, that from experience they have learned already to put much more confidence in "the white man's dose." The latter consisted of turpentine and castor oil, the time-honoured remedy among ourselves. We are not able from Dr. Peacock's cases, to institute any comparison between turpentine and the fern oil, and can only state that we believe he is supported by several other hospital physicians who have given much attention to this matter, in maintaining that the latter ought to stand *facile princeps* among our anthelmintic drugs.

As regards the economics of the question, which are important in hospital and and Union practice, it will, of course, be easily granted that all things considered the most efficient remedy will probably in the end prove the cheapest. A dose of castor oil and turpentine, undoubtedly, costs far less than any of the others. Next to it comes the kousso, which has as rapidly fallen in price as it has in general estimation. The kameela is, as yet, rather expensive, though not nearly so much so as the fern oil. A full dose of the last costs eight-pence, of the kameela about four-pence, of the kousso three-pence, and of the turpentine and castor oil not more than three-halfpence.

Kuchenmeister, in his *Manual on Parasites* (Sydenham Society's edition), writes of the oil of turpentine as follows: "As has already been remarked, the touchstone of a remedy for tapeworm is not whether it expels *bothriocephalus latus* or *tœnia solium*, but whether it is also capable of effecting this with *t. medio-canellata.* That oil of turpentine is efficacious in the latter case I can prove at any time; for the finest specimen of *tœnia med.* that I ever saw was expelled by it. In general also it acts pretty rapidly. Lastly, it has also the advantage that it expels the worm entire." Of the kousso he writes: "For my part, I have always been more or less unlucky with this remedy. . . I have generally seen the worm expelled in innumerable fragments. . . . I have never found the head. In one case I detected fragments in the evacuations for three months." Professor Martius, of Erlangen, who also has used kousso largely, never saw the head brought away. Of the male fern, Kuchenmeister states: "This remedy, which will always maintain its renown against the *bothriocephali*, appears hardly to maintain its reputation with regard to tænia." The kameela he had of course not tried.

Of the desirability of having the intestinal canal as empty as may be before giving anthelmintics, most practitioners are aware. To administer them fasting in the morning is usually thought sufficient, but in cases where difficulty has been encountered in destroying the animal it may be well, as an introductory measure, to give a sharp purgative.—*Med. Times and Gaz.*, Nov. 6th, 1858.

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20. *Hysteric Condition of Joints.*—Mr. BARWELL read before the Medical Society of London (Nov. 15, 1858) a paper on this subject. These affections, the author observed, are not rare, especially amongst the more luxurious classes, and they have often been mistaken for actual joint diseases, when blisters and issues, increasing the evil have been applied, or even more heroic and disastrous treatment adopted. It must be confessed that the literature of the subject, and the cases collected, are meagre and unsatisfactory, and, therefore, this paper is intended to present a concise, yet detailed sketch of the disease, and of some new points in its treatment.

Although in a malady so Protean as hysteria, no short description of invariable symptoms can be given, yet two peculiarities may be fixed upon as especially characteristic; and these are, the absence of the ordinary signs of inflammation, and "anomaly." One may be inclined to add to these symptoms, the hysteric condition; yet, though such condition is present in many cases, it is in others quite absent, or so slightly marked as hardly to exceed the ordinary mobility of the feminine character. When hysteria breaks out in the paroxysm, it is usually sated by that manifestation, and produces no such serious effects as a pseudo malady; indeed, the imitative tendency of hysteria is often checked by a regular fit, and a simulated disease may occasionally thus end; but, in other cases, the imitation may continue uninterrupted by any other hysteric symptom, and we are then thrown for our diagnosis upon a purely local investigation. Let us first

take the knee, as the more frequently affected joint. The pain is, in some cases, so severe as to make the patient hold her leg constantly semi-flexed and immovable; in other cases, it is so slight that the patient, though complaining, walks about. The pain is not in direct, but rather in inverse, ratio with any other hysterical symptom. It may be increased at the menstrual period. It is generally referred to a spot on either side of the ligamentum patellæ, and is increased on the slightest touch at this spot, but especially if a piece of the subcutaneous fat here situated be pinched. In other cases, the tenderness is spread over a larger space, but is always superficial. The articulating surfaces are not tender; they may be forced together, by pressing the foot upward, without producing pain. In the severer cases, when the knee is kept fixed, the surgeon, if he attempt to change its position, will feel the muscles of the limb thrown into strong action. A striking characteristic is the absence of heat about the affected joint—it feels quite as cool, and sometimes, the author is inclined to think, even cooler than the other. Swelling, in any marked degree, is absent in cases of knee-joint disease; if measurements be taken, the swelling will be found greater than is ordinarily supposed; but the hysteric knee, when not inflamed by irritant treatment, is seldom swollen, and never more than about three-quarters of an inch. The swelling is tegumentary merely; the healthy parts may be felt beneath.

When the disease affects the hip, it is, by a skilful eye, even more easily detected. When the patient is lying down, the limb is drawn up, the knee bent, and there is great superficial tenderness over the whole haunch, hip, and thigh, but no pain on pressing the articular surfaces together from the heel upwards; if the joint be not moved, there is no greater heat on that side than on the other. If the surgeon, by perseverance, get his patient to stand, he will observe a marked twisting of the pelvis, in part an imitation, in part exaggeration, of the position assumed in hip disease. The glutei may be felt in strong action, and the nates, instead of being flat, on that side are protuberant. Swelling is hardly to be measured at the hip, because it is surrounded by muscles whose greater or less action must alter the dimensions of the part. That creaking of certain joints which sometimes comes on with puberty may gradually become more fixed, till it settle down into hysteric joint disease; therefore, there sometimes accompanies this malady, a parchment-like crepitation, which is easily distinguished from the crepitus of rheumatic arthritis. Besides these signs, it must be remarked that an hysteric patient has not the worn aspect of one whose cartilages are ulcerating.

Now, the peculiarities of hysteric disease impress upon the local complaint a quality of unreality which requires some examination. It is not to be supposed that these patients willingly deceive their medical attendant, nor that the pain complained of has no real existence; but it is not produced by a local condition —the malady is centric, not eccentric. Hysteria has, perhaps, been too much regarded as the *bête noire* of medicine, connected with an obscure and sometimes undiscoverable menstrual disorder, and, therefore, to be treated with iron and emmenagogues, and such-like medicines. Yet, in truth, though the disease may be originally produced by the circumstances and conditions of woman's life, it soon becomes independent of uterine action or inaction; it becomes a neuropathy which can be called forth by the feelings and imaginings of the patient, who is more or less aware of the power she exercises over her condition, and, believing her sufferings real, is yet delighted to direct them by such mental acts. Thus the malady must be treated on other principles than such as would follow a mere uterine pathology. Great harm is done by the indiscriminate use of steel, ethers, aloes, &c., which are often given when a lower diet and more exercise would much better cure the disease. If, however, the above view be correct, the treatment must rather be directed to the cerebral condition which produces the neuralgia-like pain, and which has the faculty of swaying the disease by its own emotional state; for it must be evident, from that view, that if this emotional state can itself be dominated, the disease will be governed with it; if the patient's faith can be so far mastered as that she shall fully expect to be cured by any given proceeding at a certain time, she *will* be cured by that method at the time specified. The author has tried several means whereby, the patients' confidence

having been sufficiently gained, he could call away their attention from the part affected *to some distant spot*, in which a disorder working its own cure had been artificially produced. Of all such means, a seton seems in most instances the best; this is to be made of a single ligature (silk) set in at a distance from the affected joint, and embracing only a small portion of skin. The placing of a seton is sufficiently painful and like a surgical operation to attract strongly the patient's attention, and yet not so much so as to be cruel or greatly repugnant to her feelings. Another advantage is that, besides a distinct beginning, it has a certain end, which the patient is to watch; and if she believe, as can well be managed, that as the seton works through the skin she will get better, and when it comes quite away she will be well, the result is certain to follow her belief. Mr. Barwell read several cases which he had thus treated, and quoted in support a case in which Mr. Hancock, by giving a patient thus affected chloroform, and performing a mock operation, had produced a cure. He observed, in conclusion, that the most essential points were—to be quite certain in the diagnosis, to master the confidence of the patient, and to place the seton or other agent at a sufficient distance from the part affected.—*Lancet*, Nov. 20, 1858.

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21. *Pathology of Rheumatism.*—Dr. FRANCIS T. BOND analyzes (*Midland Quarterly Journal*, April and July, 1858) the prevailing doctrines regarding the intimate nature of rheumatism, and objects, with regard to the lactic-acid theory, which may be said to be the one most generally prevailing at present—1. That lactic acid has not been shown to be in excess in the blood of rheumatic patients; 2. That, even supposing it to be present in excess, it would be difficult to trace the connection between this circumstance and the exudations in and about the different fibrous structures of the body; 3. That other acids being in excess in the secretions, and therefore possibly in the blood, they may be as much the cause of the phenomena as lactic acid; 4. That, in regard to the theory attributing the disease to suppression of the cutaneous excretions, it is doubtful whether it is preceded by greater suppression than the prodromata of all inflammatory diseases bring with them; and 5. That the extreme tendency to sweating which occurs during an acute attack of the disease may be much better explained by another theory.

In order to establish a theory of rheumatism, Dr. Bond next analyzes the phenomena of the disease, and finds that fatigue, exposure to cold, mental emotions, or some other depressing agent, exercise a paramount influence in its production; febrile symptoms making their first appearance, followed by local affections in some fibrous tissue. A hyperinotic condition of the blood exists from the first, and the excessive fibrin having a special affinity for the fibrous structure, is specially deposited in and about them; hence the joints and the valves of the heart become the chief seats of the local affection. The preference shown in different cases for particular joints depends upon their greater weakness, or upon their labouring under some abnormal condition, upon the principle enunciated by Mr. Paget, that the depressed nutrition of a joint makes it more liable than any other part to be the seat of inflammation excited by the diseased blood. Dr. Bond's theory, then, reverses the order in which the different constituents of the diseases are commonly supposed to stand. Instead of regarding the hyperinosis merely as an effect of the reaction of the local disease upon the system at large, he considers it to be the primary source of the exudation, the causative agent of the latter, without which it could never exist. The increase in the urinary and cutaneous secretions, and the greater amount of urea, uric acid, lactic, phosphoric, and other acids in them, the author attributes to the metamorphosis of the fibrin; these substances being the products of the degradation of fibrinous matter, "the relations of urea and uric acid to highly nitrogenized matters—as exhibited by the experiments of Lehmann, by the recent manufacture of urea by oxidizing albuminous substances by M. Béchamp, and by the general excess of these excreta in the hyperinotic states of the blood, combined with that of lactic acid, to the muscular juice as determined by the researches of Liebig—amply corroborate this statement as far as these three bodies are concerned; the others, from the smallness of their amount, may be put out of consideration."

Dr. Bond considers the sources of an excess of fibrin in the system to fall under

three heads: 1. As a result of imperfect primary assimilation; 2. As a result of a metamorphic process, normal in nature, but extreme in amount; 3. As a result of defective elimination of the fibrin by the excretory processes provided for the purpose.

Having said thus much, we must refer our readers for the conclusions which the author draws as to treatment to the paper itself; we will merely add that his theory possesses a great resemblance to that propounded by Mr. Toynbee, a short time back, at the Medico-Chirurgical Society, shortly after the publication of the first part of Dr. Bond's paper.—*Brit. and For. Med.-Chir. Rev.*, Oct., 1858.

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22. *Bronzed-skin Cachexia, with Congenital Absence of the Supra-renal Capsules.*—Mr. J. K. Spender records (*British Medical Journ.*, Sept. 11, 1858) an example of this. The subject of it was a female, 53 years of age. "She had been for some time 'out of health'—ailing—suffering from something difficult to define, and out of the category of ordinary nosologies. She looked very pale, the pallor having that dark earthy tint which is ordinarily associated with the existence of malignant disease. When she sat down, she stooped forward like a very aged person, from physical inability to sustain herself upright; and her lassitude and exhaustion appeared to be extreme. No pain was complained of, but she had lately suffered from diarrhœa, although this was not of sufficiently long continuance, nor of sufficient severity, to explain the aggravated spanæmia. Her health, until two months previously, had always been good; and she had apparently been one of those persons who, by 'never having known a day's illness,' are said by a great humourist to miss one of the finest disciplines of life."

The treatment was simple, and did not influence the progress of the disease.

"The *post-mortem* examination revealed the entire absence of the supra-renal capsules. The kidneys were healthy; but there was a remarkably anæmic condition of the whole mucous membrane of the alimentary canal—a point to which attention was first directed by Dr. Simpson. Black pigment was accumulated to a considerable extent in the mesenteric and bronchial glands. The latter were so swollen with pigment that they appeared like tough inky tumours, thus literally realizing Rokitansky's words (vol. iv. p. 393, Syd. Soc. edit.). Black pigment was also noticed in the parenchyma of the lungs. The other thoracic and abdominal organs were healthy. The cavity of the head was not examined. The tegumentary discoloration was tolerably uniform, and had a metallic shining character by reflected light. Over the flexures of the great joints, the dark tinge was much increased."

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23. *Bronzed Skin and Healthy Supra-renal Capsules.*—Dr. Harley showed to the Pathological Society of London different parts of discoloured skin, and also the healthy capsules, which were taken from a man, aged sixty-six, who died in University College Hospital. The patient was admitted into the hospital, under the care of Dr. Parkes, a month before his death. At the time of his admission he presented a curious appearance, being more like a half-caste than a native of a temperate climate. The whole body, except the lower extremities and a few isolated patches on the abdomen, was of a dark bronze colour, the darkest parts being about the head and neck. His history was, that seven years ago he had a five-weeks' attack of jaundice, from which he perfectly recovered. Three or four months afterwards he observed a change taking place in the colour of his skin; some parts seemed to become whiter, others darker. The dark places gradually increased in size, and at the end of six months had extended to nearly the degree they presented on his admission. During the last three or four months of his life he had become gradually weaker, lost flesh, and had little or no inclination for food. His bowels, too, were irregular. In fact, the case presented the signs and symptoms of Addison's disease. On post-mortem examination, however, the capsules were found *perfectly* healthy in every respect, both by naked eye and microscopical examination. The peritoneum, as well as the rete mucosum of the skin, contained pigmentary matter. The man died from ascites, the result of a diseased liver.—*Lancet*, Nov. 27, 1858.

24. *Intestines of a Pig, which, for six weeks before death, had been fed with "Typhoid Dejections."*—Dr. MURCHISON exhibited these to the Pathological Society of London, and observed that although it was generally admitted that the true typhus fever is eminently contagious, many still entertained doubts as to the contagious nature of the so-called "typhoid fever;" yet it was difficult to explain, in any other way, the facts which had been adduced by Bretonneau, Gendron, Piedvache, and others. Some observers, and more particularly Dr. Budd, of Bristol, and the late Dr. Snow, had thought that typhoid fever was propagated by the dejections from the bowels. Without questioning the validity of this supposition, Dr. Murchison expressed his belief that many of the facts which had been urged in its support might be explained on the hypothesis of a spontaneous origin of the fever from the putrid emanations from the drains, which had been thought merely to convey the poison. All those who had considered that the fever might be communicated by the dejections had been strong opponents of the possibility of its spontaneous origin. It was obviously of great importance, both in a medical and a sanitary point of view, to determine whether fever might be communicated in the manner just alluded to. The experiment had been undertaken in order to throw some light upon this question; and its results were offered simply for what the results of one experiment might be worth. A pig had been selected for the experiment for the following reasons: 1. Because in its diet it approached most nearly to man; and it was thought that less difficulty would be encountered in making it submit to the experiment than with other animals. 2. There were few or no animals in which the structures that became specially diseased in typhoid fever, viz, Peyer's patches, were so well developed. 3. Because there was evidence that the pig was liable to typhoid fever. Cases of the disease, in this animal, in which the characteristic lesions had been found after death, have been described by Falke and other writers on veterinary medicine. The pig selected was between three and four months old. Care was taken that the dejections were obtained from typhoid patients in whom they presented the light ochrey colour peculiar to the disease in the most marked degree; they were mixed up with barley-meal and other articles of food. The first was given on Sept. 9th, 1858. For the first three weeks one was given every day, or every second or third day. During the next fortnight, two or three were given every day; and, during the last week, one every second day. They were eaten greedily. On two different occasions, during the first fortnight, the animal had slight diarrhœa, lasting for twelve hours, and its ears felt rather hot; but these symptoms speedily subsided. With these exceptions, the animal exhibited no abnormal symptoms; its stools were of normal consistence, and it increased greatly in weight and size, as was shown by measurements taken at the commencement and at the termination of the experiment. On Oct. 23d it was killed, and its body opened. There was abundance of subcutaneous fat, and the muscular tissue appeared healthy in every respect. The intestines throughout were healthy. There was not the slightest trace of any recent or old ulceration anywhere, nor of any thickening or alteration of Peyer's patches, or of the solitary glands. The mesenteric glands were not enlarged.—*Ibid.*

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25. *On a peculiar Black or Blue partial Coloration of the Skin, which is sometimes observed in Women, particularly round the Eyelids.* By LEROY DE MERICOURT. Besides the four cases of this singular affection described by Neligan, M. quotes one case described by Yonge in 1709. She was a girl of 16, native of Portsmouth, never menstruated, and black coloration gradually disappeared in six months; and another described by Billard in 1813, also a girl of 16, whose face, neck, and upper part of the breast, particularly the brow, alæ nasi, and round the mouth, presented a beautiful blue colour, which could be wiped off with a towel, and coloured the white linen. She had menstruated regularly for two years; and from that date had observed the blue coloration round her eyes, which disappeared in the open air, but speedily returned, so soon as she began to work in a warm close room. After a year, the blue coloration spread over her face, neck, and belly, and no longer disappeared in the open air. Subject to a dry cough, she occasionally expectorated a little blood, especially

about her menstrual period, after this had passed, accompanied by vomiting and expectoration of blood; she was paler, breathed more freely, and the blue coloration was almost gone; increased heat and vascular excitement brought out the colour stronger; the blue colour was tested by various re-agents; and, as amongst those which neutralized the colour, bicarbonate of soda seemed the least hurtful, it was given internally, and in twelve days the coloration was once more restricted to the circumference of the eye, the brow, and the alæ nasi. M. has himself observed in Brest no fewer than five cases; the first three he relates summarily, as they occurred some years ago, and the phenomena were incompletely manifested. The respective individuals were from seventeen to twenty years of age, previous health in two of them normal, in the third dysmenorrhœa, hysteria, and megrim co-existed. Twice the dark coloration came on, after sudden suppression of the menses. In one case, fainting, headache, palpitation, and oppressed breathing, were the immediate results of the suppression, the coloration beginning two days subsequently on the upper and lower eyelids, other dark stains likewise making their appearance on various parts of the body. The dark colour was paler in the morning, and became darker after exposure to any excitement or high temperature. After two years her catamenia recurred; the dark colour, however, remained, withstanding the effects of marriage and several confinements, experiencing, however, a perceptible diminution during lactation. Since then the colour has become markedly paler, although the menses are still incomplete. In the second case, there was also markedly less colour in the morning, which could also at such times be partly wiped off, but speedily recurred; in this case, as well as in the third, the colour remained during pregnancy. The fourth case was a newly married woman, aged twenty-two, who first menstruated in her seventeenth year, and a year after, while menstruating, fell into the water up to her waist, whereby the menses were suddenly suppressed, and she was seized with headache, palpitation, oppressed breathing, and colic pains, and also expectorated blood several times. Four days subsequently, she remarked a dark coloration of the lower lids, which speedily increased in extent and intensity. Four months after the menses recurred, the black colour remaining, however, much the same, paler in the morning, more remarkable after excitement or exposure to high temperature; lately, however, it has become much less. The fifth case was a brunette child's-maid, aged twenty, who menstruated first at seventeen, and had been hitherto in good health. About two months ago, three days after normal menstruation, she remarked a dull blue coloration of both lower eyelids, which had next day assumed a darker tint, like china ink, and extended down the cheeks. Examination with a magnifying glass showed that, as in Neligan's case, the coloration depended on a multitude of dark points, wiping with a towel stained the latter; but neither wiping nor washing sufficed to remove the colour. This coloration, according to M., consists in a pigment deposit on the surface of the epidermis. Neligan and Hebra have supposed, from the punctuated appearance of the coloration, that its seat was in the sebaceous follicles. M. rejects this, because any connection between menstruation and these follicles is unknown, while pigmentation stands in acknowledged relation to many uterine conditions, as pregnancy, for example. M. particularly refers to the evanescence and mutability of the coloration in several cases as incompatible with this theory of its origin, and without speaking positively, seems to regard the punctuated appearance as more probably depending on pigmental alterations of the openings of the perspiratory ducts. The eyelids are the chief seat of this coloration, partly because of the fineness of their integuments, partly because of the acknowledged sympathy of the eye with the sexual organs. M. considers that Neligan's definition, "Stearrhœa nigricans" is erroneous and premature, and that a circumlocutory title is to be preferred, until more is known regarding the nature of the affection. From the ten cases already described, M. draws the following conclusion: *Etiology.* The individuals affected were from sixteen to twenty-two years old; two were sixteen; eight had not yet menstruated; the disease always commenced in the unmarried state. In eight cases there were either dysmenorrhœa or amenorrhœa; only in one case was menstruation unaffected; thrice there was sudden suppression of the menses (twice after exposure to cold, once after mental excitement). Nine cases occurred in towns situate near the sea,

five of these in Brest. Two patients were of fair complexion (Blondinen). The *eruption* of the disease was usually sudden, yet it always took some days to reach its height. The shortest duration of the disease has been three months; another case has already lasted seven years. In tedious cases, the coloration endures in spite of the return of the menses, or parturition itself, though both bring about variations in it. In one case nursing was beneficial. Its disappearance was never sudden, always gradual. The *therapeutics* must always have respect to the apparent prime cause, anormal menstruation; the due regulation of that is sometimes followed by disappearance of the coloration, always by a diminution of its tint.—*Ed. Med. Journ.*, Nov. 1858, from *Archiv. Gén.*, 1857.

26. *Lesions and Pathological Phenomena caused by the Presence of Lumbrici in the Biliary Ducts.*—Dr. E. BONFILS, after combàting Cruveilhier's opinion that intestinal worms can be introduced into the biliary ducts only after death or during the death struggle, analyzes the 23 cases which he has collected, in which lumbrici were discovered in the ductus communis choledochus, in the gall-bladder, or in the hepatic duct; in 2 cases the lumbrici were perfectly fresh and still living; in 1 the worm was dead and slightly altered, was of a pure white, and softened; in 1, reported by M. Forget, a lumbricus occupying the ductus communis and the ductus hepaticus was perfectly fresh, while another occupying an abscess in the right lobe of the liver was softened and macerated, evidently having been long dead; in 1 case a lumbricus formed the nucleus of a biliary calculus. The symptoms varied much in the different cases, but the author considers that the presence of the following circumstances justifies the conclusion that we have to deal with the presence of a lumbricus in the biliary ducts: the sudden appearance of morbid phenomena, without appreciable moral or physical causes, of considerable intensity, characterized by very violent pain, combined with deep colour of the skin, vomiting, &c., similar to the symptoms accompanying calculus in the biliary passages; a rapid disappearance of all phenomena on the discharge of the worm; the concurrence of these symptoms, unassociated with general colicky pains (*coliques extérieures*), are regarded by the author as indicative of a lumbricus being the foreign body which has entered the biliary ducts, and having thus arrested the passage of the bile.—*Brit. and For. Med.-Chir. Rev.*, Oct., 1858, from *Arch. Générales*, June, 1858.

27. *Development of the Larvæ of Dipteræ in the Frontal Sinuses and Nasal Fossæ of Man, at Cayenne.* By C. COQUEREL, Surgeon to the Imperial Marine. —Five cases are reported by Drs. St. Pair and Chapuis, the medical officers attached to the Convict Hospital at Cayenne, in which the most violent symptoms, followed in three by death, were produced by the immigration and subsequent multiplication of a diptera into the nasal and frontal cavities. In most of the cases several hundred larvæ were evacuated by ulceration and necrosis of parts investing the cavities. The cases all present symptoms so closely resembling one another, that it will suffice to give one in detail.

Goujon, a watchman, was admitted into the Hospital of Cayenne on the 5th of September, 1855, complaining of intense pain in the supra-orbital region and in the right side of the face; there was neither tumefaction nor change of colour. On the following day there was severe headache, the other symptoms remaining. On the 8th of September the right side of the face was swollen, and a sanguinolent fluid was discharged from the nasal fossæ. On the 9th the whole face presented an erysipelatous swelling, with œdema of the eyelids and of the upper lip; the skin was stretched, hot, and shining; on the bridge of the nose a tumour of a purple hue appeared, from which, on being incised, black fetid blood escaped. The patient having sat down, a larva was discharged by the nose. An injection of warm water into the nasal fossæ brought away about a dozen larvæ resembling the first. Delirium ensued; the head was bent back; the pulse hard and quick; the skin hot, with much thirst; the swelling of the face extended to the forehead, while the tumefaction of the eyelids entirely concealed the ball of the eye. Several injections of a solution of chloride of soda were made, and about twenty larvæ removed with the current. On the night of the 9th the symptoms previously mentioned became more severe; local and general bloodletting, revulsive remedies

applied to the intestinal tube and the extremities, cold affusion to the head, and injections into the nares, failed to produce any amelioration. . Death ensued on the 10th of September, six days after admission.

Autopsy.—The tissues covering the nasal bones were gangrenous, and discharged a black and fetid liquid. The mucous membrane of the nasal fossæ was inflamed in its whole extent; about ten more larvæ were found crawling on the pituitary membrane. On a level with the middle passage the mucous membrane was red, thick, and softened. On opening the cranium, the meninges were found of a dark red, gorged with blood, which was particularly the case at the base of the brain. The ventricles contained a sanguinolent fluid; the brain, on incision, showed a large number of red dots.

In his remarks on the treatment, Dr. Coquerel lays great stress on the importance of injections, though he admits the extreme difficulty of removing the animals, especially after inflammation of the mucous membrane has set in; it does not appear that the plan of trephining the frontal sinuses, which he recommends, has been adopted. He advises the most energetic antiphlogistic treatment to be employed at the same time, a proceeding for which we can see no warranty, so long as the irritant cause is not dislodged.

None of the patients know how the larvæ were introduced, though it is probable that they were the issue of eggs deposited in the nasal fossæ. After being warned to prevent the entrance of insects, one of the convicts caught a fly which was about to enter, and it appears that this was a member of the tribe Lucilia, belonging to the order Dipteræ. Dr. Coquerel gives to the Cayenne variety the name *Lucilia Homini-Vorax,* and describes it thus: Length, nine millimetres (0.35 inch); yellow palpæ; the head and mandibles of a fallow colour, covered with a golden-yellow down; a very large head, broader at its base than at its junction with the thorax; the latter of a deep blue, with a purple reflex; on each side of the thorax, and in its middle, a transverse band of blackish blue, the middle one being narrower than the others, and separated from them by a golden-yellow line of little brilliancy, and presenting a few purple reflexes. The abdomen was of the colour of the thorax, with purple reflexes accompanying each segment. The feet were black, the wings transparent, slightly opaque, especially towards the base, with black veins.

Dr. Coquerel does not regard the insect in question as a parasite, but considers the development of the larvæ in the site in which they were found as purely accidental. He quotes several analogous cases to those forming the subject matter of this paper, from authors who had observed similar occurrences in Europe.—*Brit. and For. Med.-Chir. Rev.*, Oct., 1858, from *Archives Générales,* May, 1858.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

28. *On Chronic Inflammation of the Articular Extremity of the Bones.*—Mr. T. Bryant, in a paper read before the Medical Society of London (Oct. 18th), commenced by stating that, under the name of chronic inflammation of the articular extremity of the bones, he would venture to call the attention of the Society to a large class of interesting diseases, which, in their origin, are seated exclusively in the bones, but which, from their proximity to the joint, are almost always followed by disease of the articulation, unless arrested in an early stage. It had been described by authors under different names, and is now generally known by the term strumous or scrofulous disease of a joint; but he could not for one moment doubt that the majority of the cases which are described by surgeons under that name depend upon a chronic inflammation in the bone. The disease is, in its origin and progress, inflammatory, and by early treatment may be arrested. The pathological conditions found upon examination are those which an inflammatory process will produce, and it is quite exceptional to find in any bone that yellow, cheesy, material which pathologists so well knew as

strumous deposit; he did not deny that such a deposit may be occasionally present, but the cases in which it is found are so rare, that we may fairly regard such a specimen as a pathological curiosity. If, then, we confine the term strumous disease of a bone, as he (Mr. Bryant) believed we should, to such instances only where such a deposit is present, we shall, as surgeons, seldom have occasion to employ it. He then went on to describe the pathological changes which are visible in such cases; how, in the earliest condition, simple vascularity of the bone is the principal morbid condition associated with the presence of pinkish serum in its cancellated portion, which is much enlarged and easily broken down on pressure, from the loss of its earthy constituents. In its more advanced stage, the bone will be found much enlarged, the columns radiating from the shaft in a palm-like fashion, as if they had been spread out from downward pressure. He stated that if the inflammatory processes were of a tolerably healthy character, parts of the bone would appear denser and more indurated than the remainder; but that more frequently suppuration and death of the bone was the result, and that a small or large sequestrum would be seen. The denser portion of the bone would also appear thinner than natural, and the articulating surfaces more vascular. In more advanced cases, portions would be found loose, and lying in the joint, having been like a slough thrown off, the denser portions of bone dying more rapidly than the cancellated. The author then went on to show how the cartilages would be first affected when the disease had extended to the articular surfaces of the bone, and that in advanced disease they would be either thrown off, and found lying loosely upon the bone, or, in more chronic cases, to have entirely disappeared. He stated that the disease, having involved the articular cartilages, would soon spread to the synovial membrane, and that effusion into the joint would then appear; the symptoms being very acute when any portion of the bone had died and been discharged into the joint; in that case, rapid disorganization of the joint would be the result. Mr. Bryant then remarked upon the chronic character of this disease, and that it might take months, or even years, before the final destruction of the joint took place; but that, unless arrested, such a termination must, sooner or later, be observed. He then proceeded to the consideration of the symptoms by which such changes can be diagnosed, and dwelt upon the importance of observing the earliest conditions of this serious malady, for it is only in an early stage that much hope can be entertained of arresting its progress. The earliest symptom, which will generally call attention to the part, is some slight pain or aching, increased by pressure over some portion of the bone, and it is most frequent in delicate and strumous children. In more advanced cases, an evident enlargement, or dilatation, of the bone will be observed, accompanied with stiffness upon movement, and increased pain. He then described how, as the disease advances, the cellular tissue external to the joint becomes involved, as known by thickening, and that when the disease had spread to the cartilages and synovial membrane, the pain became much greater, associated with inability to move the joint without great torture, and attended with all the local and constitutional effects of an acute synovitis. Suppuration sooner or later appears, and abscesses open in all parts of the joint, the character of the disease being well declared in the total destruction of the articulation. The author then briefly alluded to the symptoms as displayed in different joints, and remarked that in delicate strumous children, the earliest complaint of pain or aching of a joint should not be disregarded, and that if it is at all lasting, the earliest stage of this chronic inflammatory condition of the bone should be suspected. He stated that parents are too apt to treat the complaints of children, particularly of joints, as "growing pains," and that on this account frequently the earliest symptoms of this severe disease are overlooked, to the sacrifice of the child's joint, or even life. In considering the treatment, the author stated that there were two principal facts which we should always remember—first, that the disease was inflammatory in its origin and in its progress; and secondly, that it was almost always found in subjects of a weakly constitution and of small power. The first point, then, was to correct, if possible, the disposition to disease; to supply power to the patient to resist the morbid inflammatory process and to overcome its effects. Tonics then became valuable remedies, in any orm that might suit the patient; he preferred iron, in the form of the syrup of

the iodide, or the phosphate combined with the phosphate of lime. Cod-liver oil was also a good tonic, and might be given at the same time. Good diet and bracing air were also recommended, and, in fact, all general considerations should be employed to improve the health and weakly powers. With respect to local treatment, Mr. Bryant stated that rest was an absolute necessity; that no movement or pressure upon the joint by standing should be allowed upon any consideration; and that, in the earliest cases, this local treatment, combined with the constitutional, was frequently sufficient; that when pain was great, leeching was of vast benefit, and hot or cold applications, according to the relief they conferred. When the cellular tissue became involved, the author believed that mercurial strapping was a valuable remedy, and that splints were also required to preserve perfect rest. In recent cases, where symptoms clearly indicate disease of the joint, he strongly advised the use of mild mercurials, in the form of the bichloride, or gray powder, combined with tonics; and stated that under its use, with rest and other constitutional remedies, a speedy cure may often be obtained. When by suppuration of the part the cartilages had clearly become destroyed, and all hope of restoration had passed away, it should be the object of the surgeon to obtain an anchylosis of the joint; and this might very frequently be accomplished by preserving perfect rest and immobility of the part by splints and strapping, and attending at the same time to the constitutional treatment of the patient; but this end could only be obtained by great care and much patience, on the part both of the surgeon and his patient. The author then went on to consider the subject of operative interference in these cases, and expressed an opinion that when necrosed bone was present, and perfect disorganization of the joint had taken place, if the part diseased should be the shoulder and elbow, there was no doubt that excision of the joint was the right operation to be performed, as perfect success might there generally be expected, and even partial use was better than the loss of a limb; but that when the hip and knee-joints were the diseased parts, it was impossible to speak of the operation of excision in the same positive and unqualified terms. He believed that when it was possible to diagnose the existence of necrosed bone, the joint being as a result already disorganized, and if the surgeon was tolerably certain about being able to remove it, the operation of excision of the diseased portion was certainly the correct treatment, if other general considerations were not opposed to such a practice; but in the hip-joint such a diagnosis was seldom possible, and the removal of a portion only was quite useless, for the part would heal up partly, only again to open, and be followed by all the symptoms which had previously existed. He believed that the only cases in which removal of the head of the femur was advisable, were those in which the bone was dislocated upon the dorsum, and necrotic, and which by its presence was keeping up profuse suppuration and constitutional irritation; but he thought that such cases could hardly be regarded as cases of excision of a joint, but should be classified more naturally with the operations for caries or necrosis. When discussing the question of operation as applied to the knee-joint, he believed that when the case was a good one for excision—that is, when the disease was chiefly confined to the articular facets of the joint, and the powers of the patient were good—it might be confidently asserted that there was generally a fair chance of obtaining an anchylosed limb, and that the operation of excision should then never be performed. In other cases, where the disease was more extensive, involving a large portion of the articular extremities of the bones, and where the powers of the patient were bad, the chances of success by excision became small, and amputation had then better be resorted to. There might be some intermediate cases, where the disease was in the head of bones, and where that disease might be removed; where the chances of recovery if left to nature were slight—for she might be unable to throw off, or get rid of, the necrosed or carious bone—and where the powers of the patient were tolerably good; in such, if the surgeon could remove the bone which was keeping up, by its presence, the disease in the joint, and was thus preventing its recovery, the operation of excision might be pronounced of value. The author had no doubt that limbs were now saved more frequently than of old by means of excision, but there was also no doubt that many of these were useless, if not absolutely injurious and in the way; and although he would not wish the Society to believe

that he was at all an opponent to what is called conservative surgery, still mere conservation must be injurious, if it were not associated with practical tendencies; and unless a limb could be restored sufficiently to enable its owner to pursue his duties or his occupation, its absence would be less injurious, and amputation had better be performed at once. The author stated that he might have quoted numerous cases, to illustrate the different points brought forward, from his notes of two thousand cases of injuries and diseases of the joints which he had in his possession; but he felt sure that the experience of those surgeons who were present would supply the deficiency. He concluded by stating that he had given, as briefly as possible, the results of inquiries which he had been pursuing for many years; and that if he had been able to place a large and serious class of cases in their proper position in our surgical pathology, his end had been obtained, and he trusted that the time of the Society had not been taken up in vain.

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29. *A rare Form of Fracture of the Clavicle.* By M. ROBERT.—In the adult, fractures of the clavicle are almost always oblique, the periosteum being torn and the fragments displaced. Sometimes, however, in the adult, but oftener in the child, the fracture may be transverse, without laceration of the periosteum or displacement. This was the case with a miserable-looking lad, aged sixteen, brought to M. Robert at the Hôtel Dieu, his left clavicle having come in contact with a table during a fall. There was no displacement, and the bone presented quite its normal appearance, there being neither ecchymosis, projection, nor depression. On passing the fingers along it, however, with a gentle pressure, a painful spot, with a slight mobility at it, was felt; and whenever the long thin bones—such as the fibula, ribs, or clavicle—have been exposed to violence, and no deformity is observed, pain limited to a very small portion of their course is quite sufficient to give rise to the belief in the existence of a fracture. To search here for crepitation would only rupture the periosteum still entire, and thus increase the gravity of the case. All that was required was to keep the arm against the chest, and caution the patient against using it—a caution of importance, for there being no displacement and but little pain, the patient, believing the bone not broken, might easily employ the limb dangerously.

In a few days the tissue surrounding the fractured point became inflamed, and the swollen periosteum formed a projecting ring uniting and maintaining the fragments in contact; confirming entirely the diagnosis derived from the localized pain and slight mobility first observed. Cases come to the hospital a week or a fortnight after the accident, exhibiting merely this circular ring, the nature of which may be yet more difficult to understand, as the patients have often forgotten that a fall or a blow has taken place. It is well to be aware of this, as the projection may be mistaken for a periostitis or an exostosis. It is easily ascertained to be a ring constituting a provisional callus, by imparting to the bone movements which are easily perceived, the ring being as yet only in its fibrous or cartilaginous condition. It gradually ossifies, and is replaced by a small definitive callus. Ordinary fractures of the clavicle are hardly ever accompanied by a provisional callus, this only being found when the periosteum is preserved entire.—*Brit. and For. Med.-Chir. Rev.*, Oct., 1858, from *L'Union Médicale*, No. 79.

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30. *Dislocation of the Right Femur on the inferior part of the Dorsum Ilii—seven weeks' duration; Reduction by Dr. Reid's Plan.*—Mr. W. J. SQUARE records (*Med. Times and Gaz.*,) a case of this, which is interesting from the luxation being readily reduced by manipulation after failure by the use of pulleys and chloroform.

The subject of it was a farm-labourer, 30 years of age; the dislocation was the result of a fall in wrestling, his antagonist falling on him.

"Chloroform used. Anæsthesia imperfect. The patient being placed on his back on a convenient table, the pulleys were applied, and extension kept up for twenty-five minutes. On slackening them, it was at first thought that the head of the bone was in the acetabulum, but during examination it slipped with a sort of snap into its old position. The pulleys being reapplied, extension was continued for twenty-five minutes longer, with the advantage of more complete anæsthesia, but reduction was not accomplished.

"Mr. Whipple now suggested to me the employment of Dr. Reid's (U.S.) mode of reduction of the femur on the dorsum ilii, which I at once commenced.

"The patient being still imperfectly under the influence of chloroform, I placed him on his back, grasped his ankle with my right hand, and his knee with my left.

"I then bent the leg at right angles with the thigh, and the thigh with the body, slowly and firmly pressed the knee and dislocated femur upwards and inwards towards the patient's face, and then swept it outwards and downwards in a circular direction along the right side of the body. While this last movement of rotation was in progress, the head of the femur slipped with a jerk into its socket.

"The limb instead of being directed inwards was now rotated outwards, with apparent elongation.

"The thighs being bandaged together, with an interposed pad, the patient was placed in bed.

"*April* 15. Can walk without pain or lameness. The lower extremities are of equal length, and their axes alike. Some atrophy of the muscles about the hip-joint continues, and in walking he feels weaker than before the accident."

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31. *Dislocation of the Forearm outwards.*—The following interesting case of this uncommon form of dislocation is recorded in the *Dublin Hospital Gazette* (Aug. 15, 1858):—

"John Reilley, aged twelve years, a fine, strong, healthy-looking boy, was brought to the Meath Hospital on the 22d July, 1858, immediately after having fallen from a high wall, on the top of which he had been creeping all-fours. He unfortunately placed his hand on a loose brick, which gave way, and caused him to be precipitated. On admission, he complained of very severe pain in the right elbow, and, his clothes being removed, the nature of the remedy became manifest, from the prominence outside the external condyle of the humerus. The head of the radius was well marked, and the articulating surface of the humerus could be distinctly felt below and on the inside. An attempt was at first made to reduce the luxation in the same manner as one of the forearm backwards, by flexing the limb, whilst the humeral extremity of the forearm is drawn downwards, or, as Sir A. Cooper says, 'bending the arm over the knee, without attending to the direction, outwards or inwards.' By this manœuvre, the muscles were put into violent action, and, as the effort gave a great deal of pain, chloroform was called for, but, before it could be brought, the reduction was effected by pressing the head of the radius downwards, forwards, and inwards, the internal condyle of the humerus being at the same moment pushed in the opposite direction. The arm was then placed on a cushion, and a cold evaporating lotion kept constantly applied. No great amount of inflammatory action ensued; and, although some trifling swelling of the joint remained, still the boy was well enough to leave the hospital on the third day after the accident.

"These are not dislocations in the reduction of which much difficulty is to be expected, when recently seen; but, in the case just stated, the facility with which it was accomplished was most remarkable and satisfactory, when the pressure was made across the axis of the joint; proving that a slight amount of force will often accomplish the desired replacement of a bone, when made in the proper direction."

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32. *Displacement of the Coccyx sideways.* By Roeser.—"A corpulent woman, aged thirty-six, fell from a table on a chair, so that its back came right between her thighs. She instantly felt severe pain in the coccyx, but continued able to move about till evening, sitting increasing the pain very much. In the evening the pain was so great, extending up the spine, that she was obliged to go to bed, and soon after could neither turn nor rise up. After a painful night, R. found this otherwise blooming woman quite immovable, with distorted features; she complained of violent pain in the coccyx, and a painful tensive drawing feeling from below, up to the neck, which also extended down the arm. She could move the forearm a little. The slightest motion of the body or head to one side was impossible, and still more so sitting up in bed; confused head-

ache, and some mental disturbance, were also present. She made no complaints of her lower extremities, nor of her arms, and urinated without difficulty. After placing her on her right side, a small swelling, the size of a hazel-nut, was felt near the notch of the buttocks next the left ischium, which, on closer examination, proved to be the coccyx separated from the sacrum, and forced from the mesian line towards the left ascending ramus of the ischium. The obtuse end of the sacrum could easily be felt between the buttocks. By placing one finger in the rectum the dislocation of the coccyx could be still more easily felt; forcible pressure downwards and towards the right buttock caused it suddenly to glide into its normal position, whereupon the patient declared herself relieved, as if roused from a dream, and all her pains vanished. She could move about freely; but pain in the sacro-coccygeal region prevented her sitting up; her expression was also completely changed. After a few days, a dull pain in the sacro-coccygeal region preventing sitting, was all the uneasiness that remained; and in five days, all of this that was left was a slight burning sensation at the injured spot." The irritation of the spinal marrow observed in this case, in which only the very lowest filaments could have been disturbed, and which, nevertheless, sent the stream of disturbance to the brain itself, is a most interesting example of mechanical irritation, as evidenced by its instant disappearance on the reduction of the dislocation.—*Ed. Med. Journ.*, Nov. 1858, from *Betz. Memmorab. aus der Praxis*, 1857, and *Prag. Vitljschift*, 1858.

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33. *Impermeable Stricture.*—M. CHARLES PHILLIPS terminates a series of papers upon this subject with the following conclusions: 1. The transformation of tissues produced by urethritis may completely obliterate the canal. 2. Complete obliteration takes place more frequently after traumatic action than after simple inflammation. 3. It is always complicated with urinary fistulæ. 4. Complete obliteration is perfectly distinct from stricture termed impermeable. 5. This latter always allows a certain portion of urine to pass, either at more or less close intervals, or continuously drop by drop. 6. Wherever urine can pass, a bougie may be always introduced, on condition of our proceeding slowly, patiently, and with full confidence in the power of the instrument. 7. Perforation is the basis of the treatment of complete obliteration. 8. If the obstacle is situated in the straight portion of the urethra, it should be attacked by a trocar, the finger being able to follow and guide this through the tissues. When the obstacle is situated in the curved portion, we should first introduce a grooved canula into the perineal fistula, which may serve as a guide to the trocar passed by the meatus. 9. If retention of the urine is produced by stricture, catheterism should never be performed by means of a metallic instrument. Filiform bougies should be employed, which should be introduced slowly, and after a few minutes withdrawn. Each time a little urine is discharged, with relief to the patient; and when his suffering becomes abated, we may fix the bougie, and the whole of the urine will be discharged over it. 10. If the introduction of the bougie is for the time impossible, and the retention becomes insupportable, supra-pubic puncture of the bladder should be resorted to. 11. If retention is complicated with infiltration of urine, and the introduction of the bougie cannot at once be accomplished, the supra-pubic puncture should be made, as should be large incisions into the perineum. After a few days the tissues will have become sufficiently disgorged to admit of new attempts at catheterism. Not being now pressed by the patient's sufferings from retention, we may proceed slowly and cautiously, and we shall traverse the stricture erroneously believed to be impermeable.—*Brit. and For. Med.-Chir. Rev.*, Oct., 1858, from *Bulletin de Thérap.*, tom. liv.

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34. *Internal Urethrotomy.*—Prof. SEDILLOT has been publishing, in the *Gazette Hebdomadaire*, a series of cases showing the efficacy of cutting stricture upon a filiform bougie. The *sine quâ non* of the operation is, the passing of this bougie into the bladder, and it is upon it that the secator runs along and divides the stricture. The most desperate cases, not only of stricture, but of retention, were *instantaneously* cured in this manner, the patients not requiring afterwards the use of dilating instruments. Both straight and curved secators are used, as the stricture is anterior or posterior to the pubic arch. M. Sédillot

deprecates the use of bougies after the operation, and now looks upon the external incision, or perineal section, as it is called, as only exceptionally of use. The propositions which the author wishes to establish are the following:—

1. Dilatation in simple cases.

2. Internal urethrotomy when the stricture cannot be permanently dilated, and allows the introduction of a bougie; whether the case be complicated or not with inflammatory or traumatic retention.

3. Perineal section, when the permeability of the canal does not suffice for the disappearance and cure of the complications; and when burrowing and infiltrations about the perinæum require the incision of that region.

4. Perineal section is again required in those happily very rare cases where the morbid changes in the canal offer an insurmountable obstacle to the introduction of a filiform bougie into the bladder.—*Lancet*, Nov. 27, 1858.

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35. *Vesico-Vaginal Fistula.*—Dr. Geo. Buchanan has published (*Glasgow Med. Journ.*, Oct. 1858) an account of a case of this, successfully operated on, in the Glasgow Royal Infirmary, by our countryman, Dr. Bozeman. We omit the details of this case, but give Dr. Buchanan's remarks, as we are sure they will be read with interest.

"I have published this case at Dr. Bozeman's request, and because the merits of this operation are still doubted in some quarters. True, it has not succeeded in every case, and in some instances death has been the issue; but the same can be said of every operation in surgery, however simple. There can be no doubt, however, that a measure of success has followed this proceeding which cannot be affirmed of any other mode of treatment. I do not intend to add any statistical or critical account of its success, but it may be proper to allude to the plan of Dr. Sims, of New York, who claims a large amount of success for his operation. To him belongs the credit of having introduced all the preliminary steps of the operation as performed by Dr. Bozeman. The use of the single broad-bladed speculum, and the position of the patient on her knees, does away with a great part of the difficulty of operating on a part so concealed and inaccessible as the wall of the vagina. The formation of a broad, bevelled raw edge round the fistula affords an extensive surface for union. The introduction of the sutures at a distance from the edge of the wound, allows an amount of traction and support which cannot be attained by stitches placed close to the incision. Above all, the employment of thin silver wire as a substitute for silk thread prevents the ulceration, or cutting out, which is inevitably caused by organic substances imbedded in the living tissues. It has recently been found, by various experimenters, that it is of little consequence what metal is used, and iron, copper, and platinum wires have been found to act as well as silver; and the material which may ultimately be chosen as the best suture, will depend on the flexibility of the metal.

"Dr. Bozeman, in the accounts which he has published of his operation, freely acknowledges that, in the proceedings above mentioned, he has closely followed the directions of Dr. Sims, and only claims for his method of fixing the wires by a metallic shield, an amount of success which has not followed that of Dr. Sims, who employs two bars of lead, after the manner of the quill suture. This method of drawing the edges of the wound together, called by Dr. Sims the 'clamp suture,' is a decided improvement on the common interrupted suture; but the use of a metallic shield, as recommended by Dr. Bozeman, seems to make the operation perfect. By its use, the wires can be pulled perfectly tight, and the vaginal aspect of the incision is drawn up into the concavity of the shield; so that towards the interior of the bladder is presented, not the line of the wound, but smooth lips of mucous membrane. The shield also prevents the vaginal and uterine secretions from coming in contact with the wound, which is thus protected, both externally and internally, from the presence of irritating fluids. In the case detailed, the menses flowed during the progress of the cure; and, had it not been for the shield, there is little doubt that a considerable irritation would have been set up.

"It is much to the credit of American surgery that this distressing affection can now be treated, with a probability of success even greater than many other

surgical diseases; and the surgeons of this country cannot fail to recognize the claims both of Dr. Sims and his follower, Dr. Bozeman. Dr. Sims has done much by perfecting the operative part of the proceedings, but we cannot deny to Dr. Bozeman the merit of adding the last, a very essential part, of the treatment. It only remains to add, that the whole hospital staff, as well as the other medical men who were present at the operation here detailed, besides being satisfied with the perfect adaptation of the operation to effect the end proposed, were unanimous in according to the skill of the operator a large share of its successful result. There was but one opinion, that while he was entitled to share in the credit of devising the means, his mode of employing them exhibited the master-hand of an accomplished surgeon."

Another case of vesico-vaginal fistula, with anteversion and incarceration of the cervix uteri in the bladder, operated on by Dr. Bozeman at the Royal Maternity Hospital, Edinburgh, is recorded by Dr. ALEX. KEILLER (*Edinburgh Med. Journ.*, Oct. 1858). This case terminated unfortunately, but Dr. K. bears testimony to the ingenuity displayed by the operator in effecting the objects of the operation, and to the skill with which it was performed. Indeed, the *post-mortem* examination, he says, showed that the special mode of operation adopted in this case fulfilled the object intended, and he ascribes the unfortunate result, in great measure, to the unfavourable bodily and mental conditions of the patient.

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36. *Rupture of an enormous Ovarian Cyst into the Peritoneal Cavity; permanent Cure.*—The following remarkable example of this is recorded by C. FARRAR in the *British Medical Journal*, Oct. 23, 1858:—

"Mary B——, aged 36, a tall, spare woman, apparently pretty healthy, about eleven years since, being at the time five months pregnant, fell over a trough on the left side, and from the injury was confined to her bed for some weeks. After her delivery it was found that the abdomen had scarcely diminished in size, and a large fluctuating tumour was detected in the left side. For several years the tumour steadily increased, until it completely distended the abdominal cavity, and acquired the magnitude which it had at the time of the occurrence of the accident.

"About eighteen months since she stumbled and fell upon the abdomen on a brick floor. She became collapsed, and had violent rigors; and in a few hours inflammatory fever set in, accompanied with acute abdominal pain. She lost flesh rapidly, was unable to lie down, and in a few days enormous anasarca of the legs and body up to the waist came on. The urine was nearly totally suppressed, and so severe were the symptoms that no hopes were entertained of her life. In about a fortnight she began to improve, and passed daily a large quantity of turbid urine. At the same time the anasarca rapidly disappeared. One month from the date of injury the tumour had nearly disappeared.

"I examined her fourteen months after the fall. She expresses herself, with unbounded delight, as being perfectly well, and able to walk any distance, which she had not done for some years. On examining the abdomen, there are merely the hardened pedicle and sac of the former tumour to be felt, and these appear to be rapidly diminishing in size. Of course there is a large quantity of pendulous skin upon the abdomen, which time alone will reduce to its proper form and size.

"Her general health has improved very much. She has gained flesh, and has lost the wan appearance that she previously presented."

OPHTHALMOLOGY.

37. *Coloboma Iridis.*—Mr. J. F. STREATFEILD, the editor of the *Ophthalmic Hospital Reports*, states in a recent No. of his excellent journal (No. IV., for July, 1858), that so many cases of coloboma iridis have come under his observation, that he believes it to be a very common congenital defect, and he relates the two following examples of it:—

"In the first instance, a boy was brought to the Royal London Ophthalmic Hospital, with simple conjunctivitis. For the same kind of ophthalmia he had been at the hospital before; he had also a moderate sized cleft of both irides in the common downward direction. The parents seemed quite aware of the peculiarity, and said he was, like others of their children, very 'long-sighted.' I then went to their house, and found their statement true: his eldest brother had a similar defect in both eyes, and his youngest brother the same in one eye, and the symmetrical indication of the deformity in the other. The mother's father was said to have had the same defect in both eyes, and his brother also in both eyes, and her eldest brother was similarly affected, and his eldest son had the 'long-sight' (as it was called in their family). This young man I afterwards saw, and found his irides just as they had described, but of the incomplete form. In the four of these seven cases, I examined (and in the others, as well as I could learn the facts), the vision and power of accommodation of the defective eyes were good; the irides were gray, excepting in the case (at the end) last mentioned; they were all healthy, and had no other defects. The cleft iris seems to have belonged to the males of the mother's family. The mother herself had perfectly natural pupils, and four of her children (a boy and three girls) were also unaffected. The parents were positive that those of their children that had the 'long-sight' had also a predisposition to 'colds in the eyes,' such as that for which the first-mentioned case had been to the hospital in the morning.

"In the second instance, the patient I had an opportunity of examining was a gentleman who had the cleft in both irides extending in a direction down and inwards through the irides to the greater circumference, and perfectly alike in the two eyes. His irides had the usual markings, were brown in colour, and active. Vision was in all respects good. and there was no other defect known or perceptible. His sister and two cousins, he knew, had the same peculiarity of their pupils, and in colour of irides and the other particulars I have mentioned of his case, their cases, as he affirmed, resembled his own. His grandfather, he believed, had the same defect.

"The extent to which the cleft, or even the indication of coloboma may reach from the pupil towards the attachment of the iris (if it extends no further) is very variable. A young woman, who 'never had anything the matter with her eyes;' and who had no adhesions of the iris in either eye, has been lately shown to me. The apertures of both pupils were conical; the right pointed down and outwards, and only extended from the axis, through about half the breadth of the iris; the point of the left was directed up and inwards, and did not reach further from the centre than in the other eye; but a little beyond the apex of the cleft, the coloboma, in the *incomplete* form, was extended in the radial line, exposing the dark uvea, nearly to the greater circumference of the iris.

"*Double* coloboma of the *same* iris has been recorded, and so have cases of *incomplete* cleft iris, but the case of the young man, above referred to, is peculiar in having combined these two characteristics, and in having no cleft of either iris but the symmetrical indication of it in *both* irides. The patient was, as I have related, one of a family of which several members had cleft irides; his sight was perfect, and his eyes had never been inflamed; his irides were rather deficient in the usual markings, of brown colour, and very active; his *true* pupils (ophthalmoscope) were both circular. The right iris had the indication of coloboma vertically downwards from the pupil, the dark fissure not extending through the uvea of the iris, and not further than midway between the pupillary edge and the greater circumference. The left iris had the double defect; the separation exposing the uvea in this eye was wide at the margin of the pupil, and extended below nearly to the greater circumference of the iris, in two divisions, one of which corresponded to the furrowed iris of the right eye, and the other was to its outer side. This mark of the iris had somewhat the effect of a deep impression of the letter W on its surface."

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38. *Irideremia.*—Mr. Dixon records the following case of this:—

Caroline P., aged 36, applied at the Royal London Ophthalmic Hospital in August, 1857. She had evidently suffered from chronic inflammation of the cor-

nea, both were slightly hazy and traversed by a few tortuous veins. There was no general redness of the sclerotic, no intolerance of light, nor any signs of acute disease. Both upper lids drooped very slightly, giving the patient rather a sleepy and heavy appearance. She could not read ordinary type, but with the right eye, where the cornea was less hazy than in the left, she could make out large capitals. Opposite the centre of each cornea was a white speck, evidently situated in the lens, but neither the outline of the pupils nor the tissue of the irides could be distinguished. It seemed unlikely that such a degree of corneal haziness as permitted an opacity of the lens to be seen could, at the same time, prevent a view of the iris, and a more careful scrutiny convinced me that the irides were wholly absent. A moment's glance with the ophthalmoscope proved this to be the case, the whole area of the cornea presenting the reddish glow of an illuminated retina. Across this field a few corneal veins were seen ramifying, while, in the centre, the black, nuclear opacity of the lens, stood out in bold relief, surrounded by a few widely separated striæ radiating from the circumference of the lens. When speaking of the irides as absent, I should observe that, behind the upper and inner margin of the left cornea a very narrow, scarcely traceable, brownish line indicated a rudiment of iris.

It seems singular, that with central and radiating opacities of the lenses, hazy cornea, and absence of irides, the patient still enjoyed a useful amount of sight, and her previous history is still more remarkable. She assured me that until the occurrence of the inflammatory attack a few years ago her sight had been excellent, and she was quite surprised at my having had any doubt on the subject. She went to school when ten years old, and took her place with other girls at the ordinary tasks and needlework, and so good was her progress, that at sixteen she went as housemaid into the household of a nobleman, remaining there and in another situation for eight or ten years. She stated that, as a girl she never suffered any inconvenience from bright sunlight or candlelight, and saw distant and near objects equally well. Both her parents, and several brothers and sisters, had good sight.

The case is still (June, 1858) under treatment, and some improvement in the condition of the corneæ has taken place. The movements of the eyeballs have always been perfectly natural, no trace existing of that unsteadiness which, as far as I have seen, characterizes irideremia in infants.—*Ophthalmic Hospital Reports*, July, 1858.

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39. *Inutility of Depletion in Syphilitic Iritis.*—Mr. J. HAMILTON, Surgeon to the Richmond Hospital, states (*Dublin Hospital Gaz.*, May 15, 1857) his belief "that in the treatment of syphilitic iritis, even the most acute cases, all that is necessary to be done is to administer mercury properly, suited to the constitution of the patient, and the nature of the case, and till full salivation; and the application of the extract of belladonna round the eye, or of the solution of atropine in the eye. I totally disagree with those authors—Mr. Tyrrell, for instance—who recommend, in cases where the patient is broken down, to administer tonics, &c., till he is able to bear the mercurial course, the real fact being, that the best tonic is the mercury, combined with opium, which by expelling a depressing poison from the system, invigorates it, at the same time that it arrests the ravages of a destructive specific disease; whereas, while waiting for the effects of tonics and diet, the eye may be lost. There could not be, apparently, more feeble or depressed subjects than No. 3, Mary Byrne, or No. 4, John Callaghan, particularly the latter, who was literally nothing but skin and bone, with a pale sallow face, contrasting with the large red tubercles with which it was studded, and so weak he could scarcely stand; yet, under the beneficial action of the mercury, while the eye was saved, his flesh, strength, and complexion, all became rapidly restored, so that in his last letter to me, he describes himself, in language more remarkable for strength than orthography, 'as strong as a hoss, and as fat as a wheal!'

"Many surgeons do not deplete, but the large majority still do, by leeches and cupping; rarely, I believe, in this country, by venesection, as recommended by Mr. Mackenzie. During fourteen years, a very large number of cases of syphili-

tic iritis have been under my care in the Richmond Hospital, and I have only cupped in one case; and with my present experience, I am sure if that case presented itself now, I should not do so."

As this is one of those practical questions best decided by facts, Mr. Hamilton quotes five cases from his case-book in support of his views. One of these cases we quote:—

"John Callaghan, æt. 24, transmitted into No. 4 ward of the Richmond, from the Whitworth Hospital, February 26th, 1857. He is one of the city police, and was once a stout powerful man, but is now sickly-looking, sallow, and emaciated. A thickly scattered eruption of tubercles over the face, on the eyebrows, sides of the nose and chin. He became infected with syphilis about ten months ago, and has since suffered from pains in his bones, sore throat, and eruptions, with rapid decline of health and strength. He has taken mercury irregularly. Ten days ago the right eye became tender and inflamed, and quickly got very bad. His only treatment had been one leech and a blister to the temple, and bark mixture; but he had taken no mercury for a month.

"The right eye is affected with acute iritis; the sclerotic of a deep dull red, most marked round the cornea; the conjunctiva also is traversed by many large red vessels; the iris of a dull yellowish-gray, contrasting with the clear bluish-gray of the other eye; the pupil hazy and irregular, adhesion existing at the lower and outer rim, where the iris is of a dull reddish-brown, as if a tubercle was about to form there; the pupil is nearly as large as the other, perhaps slightly affected by the extract of belladonna which was applied last night; sight very much injured—though he can see me in a bright line at three feet, he cannot discern a feature of my face; intolerance of light, and some lachrymation; pain in the brow, extending to the eyeball and temple, begins at ten o'clock at night, and lasts till one o'clock A. M. Submur. hydrarg. ℈j, opii gr. ij, in pilulas x.

"Third day. Eye somewhat clearer; the deposition of rusty-coloured lymph appears less; not so much pain last night. He has taken eight pills, but no perceptible effect on the mouth, nor any griping. The belladonna has had no influence on the pupil.

"Fifth day. The mouth is sore, and there is some griping. The eye is better, and he can distinguish my features, and the studs on my shirt. To take a pill night and morning.

"Seventh day. Mouth fully sore; a very decided improvement in his vision, and the appearance of the eye; the iris clearing, and the rusty lymph absorbing; pupil clear and black, and the redness much less. He bears light better; no nocturnal pain of the brow the last two nights; the eruption of tubercles on his face and body are fast disappearing.

"On the twelfth day the eye was not so well, more vascular and uneasy—evidently an attempt at a relapse. By increasing the quantity of mercury for two days, he got better; all traces of the iritis afterwards entirely disappeared.

"On the twenty-second day, having been quite well for several days, he requested his dismissal, wishing to go to the country. I had a letter from him a few days since, saying that he had regained strength and flesh, that the sight of the eye was as good as ever, and no traces of the eruption existed. He had continued to take the mercury so as to keep up the mercurial action in the system, altogether for about ten weeks."

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40. *Hemeralopia treated by Azotized Vapour.*—M. Deval relates (*L'Union Médicale*, No. 78, 1858) a case of hemeralopia, occurring in a lad ten years and a half old, and dating about ten days. No cause whatever could be assigned for its production. Quinine, valerian, and opium, were resorted to on account of the periodical character of the affection, in the absence of any definite indication; but as these proved of no avail, M. Deval determined to try the effect of a remedy much praised of late. This consists in exposing the eyes to the vapour arising from a decoction of ox-liver, the application taking place for ten or fifteen minutes morning and evening. Great was his surprise to find the hemeralopia entirely gone after the second of these applications, although they were continued a while longer as a matter of precaution. The employment of the remedy dates

as far back as 1762, when Dupont described it as used in the garrison at Strasburg. The vapours arising from the livers of sheep or calves have since been employed with advantage by Stoeber and others, and quite recently by several Italian practitioners.—*B. and F. Med.-Chirurg. Rev.*, Oct., 1858.

MIDWIFERY.

41. *Duration of Pregnancy.*—ELSASSER records his observations on the duration of pregnancy, made in the Stuttgard Institution. He is of opinion that no single method of computing the length of pregnancy can be taken as trustworthy; neither the date of conception, nor menstruation, nor the first fœtal movements. By the reckoning from the day of conception, out of 260 mature children, only 23, or 8.8 per cent., were born on the 280th day; 166, or 63.8 per cent., before, and 71, or 27.3 per cent., after this day. The greatest number of births (126 = 48.4 per cent.) fell between the 271st and 280th days. Of the recorded cases, 140 were first births; of these, gestation lasted in 14 exactly 280 days; in 96 a shorter time, and in 39 longer. In 111 pluriparæ, 9 lasted 280 days, 70 less, and 32 longer. The sex of the child appeared to exercise no influence. In reckoning from the commencement of the last menstruation, out of 175 deliveries, 12 = 6.8 per cent. took place on the 280th day; 43 = 24.5 per cent., before; and 120 = 68.5 per cent., after. The greatest number of deliveries fell between the 280th and 290th days. Thus, by the reckoning from the beginning of the last menstruation, 93 per cent., and by the reckoning from conception, 91.1 per cent. of all the cases lasted a less or longer time than 280 days. The weight of the children is in no constant relation with the length of the gestation.—*Brit. and F. Med.-Chirurg. Rev.*, Oct., 1858, from *Henke's Zeitschrift*, 1857.

42. *Port Wine Enemata as a Substitute for Transfusion of Blood in cases of Post-Partum Hemorrhage.*—Dr. H. L. WILLIAMS recommends enemata of port wine in cases of post-partum hemorrhage, and records (*British Med. Journ.*, Sept. 4, 1858) a case in which he successfully resorted to it. The patient was in the most alarming state of prostration, pulseless at the wrist, with cold extremities, &c. Dr. W. commenced by administering four ounces of port wine with twenty drops of tincture of opium. The patient speedily manifested signs of improvement. In half an hour he repeated the enema, with marked advantage, and the patient was soon out of danger.

43. *Inversion of the Uterus successfully reduced.*—Dr. F. S. VERITY, of Hemmingford, records (*Medical Chronicle*, Montreal, Nov., 1858) the following interesting example of this:—

Mrs. R. was about 40 years of age, and the mother of 9 children; her figure was squat and round, showing a large roomy pelvis; the abdomen pendulous; her health strong and rugged. She was taken in labour with her 10th child, and, while walking up and down, a sudden pain expelled the child, which fell on the floor, and was not materially hurt. Not so, however, the mother: the same pain which forced the fœtus from the uterus, "brought down," to use the words of the messenger who came for me, "the whole of her inside." I arrived at the scene of the accident about an hour afterwards. I found the woman lying on her back on a mattress placed on the floor, deluged in blood. She was moaning and sighing, tossing her arms wildly about, and gasping for cold air. Her pulse could scarcely be felt at the wrist, and her countenance was blanched and ghastly. When the nurse turned down the bedclothes I was stunned; I saw before me my first, and I devoutly hope my last, case of "inversion of the uterus." Occupying the space between her thighs and nearly reaching down to her knees, was a large red membranous-looking mass, from which blood was oozing, and at its lowest part (the fundus), almost disguised by clotted blood, was attached the placenta. I immediately administered a tumbler of spirit and water, cold, with

tr. opii in it; applied warmth and friction to the extremities, and, without waiting, forthwith proceeded to reduce the uterine mass to its proper position. After cleansing it from the clotted blood, the question arose in my mind, shall I reduce without removing placenta or not? Fearful of increasing the hemorrhage, I determined to reduce with the placenta attached. Recollecting the rules laid down in the books, I began the attempt, and an attempt it was only. As soon as I touched the uterus, it contracted and shrunk, and gave me the feeling as if I was holding a live eel in my hand. I tried two or three times gently, but firmly, to reduce it according to the usual directions; but I made not the least impression on it. The weight of the placenta bothered me greatly; for on attempting to return the part that had last protruded, it was constantly dragged out of my fingers by the weighty placenta. The rules were now useless to me. What was to be done, hemorrhage still going on and the woman sinking? I determined to remove the placenta, and reduce the uterus by pressure on the fundus. I quickly detached the placenta, and was most agreeably surprised to find there was very little hemorrhage; in fact after it was removed the mass shrunk in volume. I now placed my left hand and forearm under the organ, and supporting it in a line with the proper axis, with my right hand half shut, I pressed the tips of my rounded fingers firmly against the fundus, and pushed it upwards until my fingers were arrested by the constricted os. I made firm, but cautious, pressure against it, and in about half a minute I felt it yield. I then boldly, but cautiously, carried my hand upward in the axis of the pelvis, and, when my wrist was passing the constricted os, the fundus suddenly shot from my hand, and the organ resumed its usual position. Retaining my hand within the uterus for a short time, constriction took place, and the uterus returned to its proper state and condition. Of course I waited and watched. I gave her an opiate, and, at the end of 6 hours, left my patient safe for the present. The loss of blood had been frightful: I ordered broths and nourishment, and on leaving gave the strictest injunctions to maintain the horizontal position. The next day she was very much improved, and in good spirits. I introduced the catheter twice in the 24 hours; kept her perfectly still; nourished her well with broths, &c., and forbade her on any account to rise in bed. On the third day, I was suddenly sent for, when I had the inexpressible mortification to find she had just died. It appears that her nurse did not think her clothes clean enough, so a change of night-dress was resolved on. She sat up in bed to make the requisite change. After complaining "of giddiness and singing in the head," she fell back on her pillow and expired.

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44. *On some of the Morbid Conditions which give rise to Sterility in Women.*—Dr. Carl Mayer read a paper on this subject before the Obstetrical Society of Berlin. He showed that notwithstanding the important investigations of modern times upon the subject of conception, we have not yet made much progress in reference to it, inasmuch as we are still ignorant of the conditions under which this interesting occurrence is brought about, and of its further course. Although, too, the brilliant results recently afforded by the microscope deserve the greatest attention, and have both already borne, and shall certainly still bear, important fruits, a great number of practical questions have as yet received no elucidation. We still know as little as we formerly did, why conception results from a particular connection, while in a hundred or a thousand instances it does not follow from intercourse under the same external conditions. We are as yet unable to explain why many healthy women conceive only at regular intervals of three, four, five, or more years, notwithstanding the connection regularly takes place during the intervals. We cannot say why women often conceive for the first time after several years of married life (the speaker saw it once occur after the lapse of twenty-five years); we cannot give a reason why one marriage should prove unfruitful, while the same individuals entering into another should beget and bring forth children. But such questions are very important in a practical point of view, for in the existing state of ignorance of the conditions necessary to conception, we grope in the dark in the investigation of the causes and in the treatment of sterility. Hence, too, it comes, that physicians, in cases in which they give advice to barren women as to the attainment of their most ardent wish, in

general catch quite empirically at this or that remedy famed of old, and preserve only the appearance of a rational treatment when among the bath cures recommended in sterility, they prefer, bearing in mind the general constitution or existing morbid condition of the patient, a strengthening chalybeate or sea-bath for the weakly, frail, or nervous—a resolvent or ioduretted spring for the strong, plethoric, overfed, or too fat patient, suffering from abdominal derangements; and a more stimulating bath for the unexcitable, passive, insensitive woman. When in such cases the several springs have the desired effect, it will be doubtful whether the fortunate result is due to the bath or to some other circumstance. Often has the abstinence from intercourse occasioned by the journey the best effect. As to the capacity for conception of insensitive, unexcitable women, it is certain that sensual excitation is not necessary[1] to conception, that even women conceive who have a decided aversion to intercourse, while, on the contrary, very sensitive women frequently have no children.

But there is also a series of pathological conditions in the female organs, which are more or less easily recognizable and curable, and it is the bounden duty of the physician to direct his most earnest and fullest attention to these states, and by a careful investigation to ascertain the cause of sterility in any given case which may come before him. One would think that such an investigation should be a matter of course, and that no physician would omit it; but unfortunately experience shows that in numberless instances unfruitful women have been for years treated by various medical men, and sent to the most different baths, until at last an examination has proved that conception was quite impossible on account of the existence of local obstacles. Among the patients treated by the author there was, for example, the wife of an official, a person of weakly constitution, who had for several consecutive years been sent by her physicians to the sea, on account of sterility. To the question, whether an examination had been made, a negative was returned. On investigation it was ascertained that the vagina, which was scarcely one inch and a half in length, presented no trace of an os uteri, the short cul-de-sac was formed by a very dense hymen, which by reason of its great dilatability had permitted an imperfect connection to take place. At the upper edge of the hymen was a small opening of the circumference of a quill, through which the menses had found an exit. The introduction of a probe through the opening, demonstrated the existence of a vagina, examination through the rectum proved the presence of the uterus. A slight operation with the knife destroyed the membrane, and in four weeks afterwards conception ensued, and the lady is now the mother of several children. This case is by no means singular; on the contrary, the author can state that in the greater number of women seeking his advice under the circumstances, no examination had been made; he therefore considers it not superfluous, but urgently necessary, to remind his brethren that they neglect their duty when they omit an examination in cases of sterility, that they act inexcusably, and are unworthy of the confidence reposed in them, when without this preliminary step they lay down any plan of treatment whatever.

The author divides the several malformations, abnormities of development, and pathological changes affecting the female genitals, into two groups, according to their situation. 1. Those which occupy the external genitals, the orifice of the vagina, the vagina, the external and internal os uteri, and either wholly or in part prevent intercourse, rendering impossible the necessary penetration of the semen into the cavity of the uterus, and its contact with a mature ovum capable of fructification. 2. Those implicating the ovaries, the tubes, and the cavity of the uterus, and preventing either the development or separation of a mature ovum, or its further normal progress, or its organic connection with and attachment to the soil appointed by nature for its growth. The conditions belonging to the first group are recognizable by an exact examination, and are frequently capable of being even easily removed; those of the second are during the lifetime of the patient not at all accessible to the eye, and only with difficulty to the sense of touch, and render the diagnosis uncertain, and the cure very difficult.

[1] This is fully illustrated in Dr. Montgomery's important and valuable work on the "Signs and Symptoms of Pregnancy." Second edition, pp. 361–365.

The author does not go through all the anomalies belonging to this class, but quotes only the more important, adding some short observations from his own experience.

Among the diseases of the second group the ovaries play the most important part, as they undergo the most manifold changes, and are then of course not in a state to form healthy ova, and so cause sterility. If an increase of circumference does not at the same time take place, this condition is neither to be recognized nor removed. It is only the frequently occurring and often overlooked chronic inflammation of the ovaries, which is recognizable and curable, and is consequently of greatest importance in connection with the present subject. The chronic inflammation often lasts for years, causes the most violent dysmenorrhœa and sterility, is seldom combined with violent local pains, except at the time of menstruation, but it is almost always attended with consensual and hysterical nervous affections. On simultaneous internal and external examination, we find the ovaries swollen and painful, and in general soft. In such cases the ordinary anti-spasmodic and narcotic remedies are of no avail; it is only local antiphlogosis, with a mild derivative treatment, which is of use. The employment of leeches must sometimes be frequently repeated, and in a case more fully quoted by the author it had to be resorted to nine times before a permanent cure was effected. The opinion often put forward by physicians, as well as by non-professional people, that the pains of menstruation cease after marriage, does not at all hold good in chronic inflammation of the ovaries, on the contrary, the inflammation is increased in consequence of intercourse.

Another pathological condition of the second group is formed by closing of the tubes, with or without adhesion or attachment of the fimbriæ to the neighbouring organs. But we have not as yet succeeded in recognizing this change in the living subject, and the tube-sound recommended by Tyler Smith neither attains its object, nor is free from danger.

The several morbid conditions of the inner wall of the uterus are also neither to be seen nor felt in the living subject; but from their results we can with tolerable accuracy draw inferences as to their existence, and under certain conditions employ instruments and remedies for their relief. Thus chronic endometritis gives rise to a profuse, gelatinous, transparent secretion, which with the aid of the speculum we can see issuing forth from the os uteri, and which is particularly characteristic, and very important in reference to sterility, inasmuch as it hinders the access of the spermatozoa to the internal genital organs, and does not provide a suitable soil in which the ovum, arrived in the uterus through the tube, may take root; the ovum must perish, even though it come in contact with the most healthy spermatozoa. The formation of this peculiar secretion constitutes one of the most obstinate varieties of uterine affection, and the author never saw a woman labouring under it become pregnant, while the flow of mucus which so very frequently occurs in consequence of various morbid conditions of the mucous membrane of the lips of the os uteri, by no means causes sterility, and even cancer of the uterus admits of conception taking place. The injections into the cavity of the uterus recommended in this affection are dangerous to life, and have given rise to fatal peritonitis. The author himself saw a very small quantity of a weak solution of nitrate of silver, though very carefully injected, and which certainly could not have reached the tubes, instantaneously produce the most violent nervous symptoms; uterine colic with swooning, icy coldness of the extremities, etc., which did not give way until after the lapse of some hours, and quite resembled the effects produced in another case by a leech having made its way through the os uteri. Chronic endometritis produces various anomalies of menstruation, neuralgias of all kinds, especially in the stomach; menstruation itself is at one time scanty, at another profuse, and is always combined with boring, tearing, or dragging pains in the womb, the sacrum, the hips, and thighs. A diminution of the transparent mucus attends menstruation. The entire womb is painful on examination; in the speculum the soft, turgescent vaginal portion appears very red, hyperæmic; the lips of the os uteri are ulcerated around the opening, are of a scarlet red colour, and secrete a white, cream-like mucus. The author has not seen the oft-boasted advantage attend dilatation of the os uteri,

or the division of its edges with the knife; as little benefit has he observed from the use of caustic or astringent applications, or from the employment of cold; on the contrary, he has seen a cure effected by a very long-continued derivative treatment, small local abstractions of blood, the application of blisters, followed by preparations of iodine, lukewarm injections, and suitable baths, and lastly iron, which is best given in the form of chalybeate mineral water.

In the first group of changes of the female organs, more or less obstructing intercourse, are the degenerations of the external genitals, which admit of immediate recognition, as enormous hypertrophies, elephantiasis, tumours, and excrescences, and atresias. Less apparent, but still always sufficiently recognizable, on examination, are inflammations of the parts surrounding the aperture, sometimes attended with so high a degree of sensitiveness and pain, that the slightest touch cannot be borne, and intercourse is consequently impossible. Such conditions are often, through false modesty, long concealed from the physician; and the author has seen many women in whom, after several years of married life, during which they have been always under medical treatment for constant hysterical affections, he has found an uninjured hymen, and such excessive tenderness of the genitals, that the examination, consented to with much reluctance, has been attended with convulsions and fainting fits. This great sensitiveness often exists from childhood in consequence of eruptions, ascarides, or onanism, or it is the result of frequently repeated violent efforts of powerful men to accomplish intercourse in cases in which an insuperable disproportion exists between the organs. Local abstractions of blood, tepid fomentations, baths, etc., and dilatation with sponge, are the remedies on which we should in such cases rely for the removal of sterility.

Not unfrequently the impossibility of accomplishing intercourse, arising from excessive painfulness, is caused by a peculiar affection of the urethra, a relaxed, turgid, and prolapsed condition of the mucous membrane, which Morgagni called fungous excrescences, also occurring from childhood. The projections have a dark red, spongy appearance, and protrude from the orifice of the urethra, varying from the size of a lentil to that of a cherry,; they are usually attended with a secretion of bloody mucus, and frequently give rise to but slight annoyance; but, particularly when the affection of the mucous membrane extends deeply into the urethra, cause such violent pain to the touch, and consequently during intercourse, as to render the latter intolerable. It is a very obstinate affection, against which the most certain remedy is the knife or the actual cautery; caustics are usually ineffective.

Similar sufferings are produced by inflammation of Bartholini's glands and their excretory ducts, and by its results, an affection which has been so well described by Huguier. These conditions are obstinate, not difficult to recognize, and yield best to an antiphlogistic mode of treatment. In the advanced stage incisions are necessary. Pencilling with tincture of iodine has also proved useful in several cases.

Prolapse and procidentia of the uterus would hinder intercourse only in the rare cases in which the parts are not capable of replacement even in the horizontal posture. But if hypertrophy or prolongation of the vaginal portion be present, a condition easily mistaken for prolapse, reposition cannot avail, the vaginal portion would be only compressed and crookened by the attempt, and the semen could then not reach the cavity of the uterus. In such cases amputation alone remains, an operation much recommended by the author, and lately performed by him in a case which he has detailed. In this instance a soft, fleshy mass, of the thickness of a plum, about three inches long, round, very red, and painful on pressure, protruded from the genitals, having at its lower free extremity the soundest os uteri, giving exit to a clear, transparent mucus; this mass was easily recognized as the hypertrophied vaginal portion, and was returned with difficulty. The sound passed five and a half inches, that is, three inches too far, into the uterus. Amputation with the knife gave rise to enormous hemorrhage, which was arrested only by the actual cautery; the wound healed slowly, but perfectly, and at the end of six months the uterus resembled that of a woman who had borne children.

The closing of the external and internal ora uteri is important in reference to

impregnation. This may occur in three modes: 1, by growing together or adhesion, by atresia; 2, by altered abnormal position of the external os in the altered directions or obliquities of the uterus, which are called versions; 3, by compression of the os internum, in flexions.

Actual complete atresia, at an age when conception is possible, is of the most extreme rarity, while in old women it is frequently met with. On the contrary, we more frequently find the external or internal os uteri exceedingly narrow and small, so that the finest probe can scarcely be introduced. The menstrual flow is then attended with various sufferings, and it is very advisable to enlarge the os uteri, by slitting up its edges with a knife. If little polypous or fibrous excrescences close the os, they must be removed by operation.

In the various versions of the uterus, especially anteversion and retroversion, the os uteri is more or less pressed upon the neighbouring parts, and consequently closed against the seminal fluid. These versions occur tolerably frequently in women who have borne children, and are attended with many troublesome symptoms; they are more rarely met with in women who have not had children. Impregnation can take place only when we are able to give the uterus permanently its normal position, and to relieve the pathological conditions and complications on which its abnormal direction depended.

The third and last form of closing the orifice of the uterus is found in the flexions of the organ, which are to be carefully distinguished from the versions. Flexion takes place always in the situation of the os internum, and attains to various degrees; conception cannot take place where flexion exists. According to the cases of flexions collected by Rockwitz from the journals of the author (*Verhandlungen der Gesellschaft für Geburtshülfe*, 1852), there were among 117 patients 26 barren, according to Valleix 19 in 126. From more recent observations the author found, that of 272 barren women 97 suffered from flexions, and more particularly 60 from anteflexions, and 36 from retroflexions. Of these 97 cases only 29 were complicated with chronic endometritis, chronic oophoritis, hypertrophy of the uterus, ovarian tumours, or with polypi, to which the sterility could be at the same time attributed. In 68 cases, therefore, flexion remains the probable cause of sterility, recognizable by examination, and it is to be observed that none of these women had ever conceived; the great number of those, therefore, who after a miscarriage or delivery at the full term were attacked with flexion and did not again conceive, is not taken into calculation. After such statistics it cannot be doubted that flexion constitutes an obstacle to conception. But it is the duty of the physician to remove this obstacle, and at the same time the various sufferings, which, especially during menstruation, are the results of flexion. A correct and certain diagnosis of flexions is attainable only by means of the skilful use of the uterine sound, in addition to the other methods of investigation; and the employment of this instrument should, therefore, not be omitted in such cases. The inspection of the os uteri through the speculum is also useful, inasmuch as the opening, which is usually rather gaping, is in anteflexions directed downwards, and in retroflexions upwards; and, in the latter, the posterior lip is more frequently hypertrophied, presenting a darkened vascular appearance. The curability of flexions is certain, and, therefore, treatment is necessary. An experienced and cautious physician will soon recognize the cases in which, either on account of great hypertrophy of the uterus, of tumours, of intergrowth between the coverings of the uterus and the neighbouring organs, a cure is impossible. In all other instances a persevering mode of treatment must be adopted, and even should no result be obtained in six or eight weeks, the attempt must be renewed. The author has himself cured a great many cases, and has not only removed severe affections of several years' standing; but in eight cases has had the pleasure (once after twelve years' unfruitful married life) of seeing his patients conceive and bear living children.

In conclusion, the author classifies the anatomico-pathological condition of the 272 barren women examined by him. He found in 2, no uterus; in 97, flexions, namely, 60 anteflexions, and 37 retroflexions; in 38, versions, namely, 35 anteversions, and 3 retroversions; in 42, inflammatory irritation of the external genitals and of the orifice of the vagina, and among these in 14 women the hymen was found uninjured after several years of married life; in 51 chronic endometri-

tis; in 25, chronic oophoritis; in 23, ovarian tumours; in 12, uterine polypi; in 6, fibroid tumours on the uterus; in 9, hypertrophy of this organ; in 1, elephantiasis of the external genitals; in 6, no morbid condition was to be discovered; 16 anteflexions were complicated; 1 with irritation of the pudenda, 4 with chronic endometritis, 5 with chronic oophoritis, 3 with tumour of the ovary, 1 with polypus of the uterus, 2 with hypertrophy of the uterus; 13 retroflexions were complicated: 1 with irritation of the pudenda, 6 with chronic endometritis, 2 with chronic oophoritis, 2 with tumour of the ovary, 1 with fibroid tumour of the uterus, 1 with elephantiasis of the pudenda; 10 anteversions were combined; 2 with irritation of the pudenda, 3 with chronic endometritis, 2 with tumour of the ovary, 1 with polypus of the uterus, 2 with hypertrophy of the uterus; 1 retroversion was combined with chronic oophoritis.—*Med. Times and Gaz.*, Oct. 16, from *Monatschrift für Geburtskunde und Frauerkrankheiten*, Nov., 1856.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

45. *On the Ligature of the Œsophagus in Animals in Toxicological Experiments.*—A discussion has lately taken place among the members of the French Academy of Medicine, on the ligature of the œsophagus in dogs, as a means of preventing vomiting after the ingestion of chemical substances, and as a measure consequently indispensable to the study of the physiological action of these substances upon animals.

It is a well known fact, that the principal conquests of toxicology have been achieved by this kind of experimentation, and that the greater part of Orfila's experiments were made under the protection of the previous operation in question. But how did the illustrious professor, whose labours have for so many years exercised universal influence over forensic medicine, perform the ligature of the œsophagus? It is a secret which would appear to be totally lost, so different would the results he arrived at seem to be from those spoken of by our present experimentalists.

Unhappily, Orfila has not, in his works, sufficiently explained his *modus operandi*, and he has barely escaped paying the penalty of the omission with his glory. What is known is, that the eminent professor made a *puncture in the œsophagus*, for the purpose of introducing into the stomach the substance, the action of which he desired to study, and that he then tied this duct below the artificial aperture, with a *tolerably large and moderately tight ligature.* We likewise read the following, on the effects of this operation, in his *Treatise on Toxicology*, p. 46:—

"It is proved by fifty experiments, several of which have been publicly made before a numerous audience, and in presence of several members of the Academy of Medicine, that if the œsophagus, after having been isolated, is tied, and if the ligature is preserved for twenty-four or thirty-six hours, the animals thus operated on merely feel slight depression and a moderate amount of feverishness. As soon as the ligature is removed, the dogs drink, soon begin to eat, and are perfectly restored to health."

Whether, however, dogs die, or not, from the wound made in the œsophagus, after the first eight-and-forty hours, is unimportant; the end was attained, and there was no reason for investigating the effects of a permanent ligature, which is useless in toxicology. What Orfila sought by a ligature of the œsophagus he obtained—the possession of a preservative means by which chemical substances might be retained in the stomach without itself occasioning symptoms which might be confounded with those resulting from the introduction of these substances, or capable of causing poisonous properties to be ascribed to a harmless substance.

Now, after an affirmation so positive as the one we have just quoted, and which the learned world had generally accepted as gospel truth, the surprise of the Academy will be readily imagined when, two years since, two Professors of

the Veterinary School of Alfort, Messrs. Bouley and Reynal, came forward with assertions diametrically opposed to Orfila's. Messrs. Bouley and Reynal sought to prove that the deligation of the œsophagus, far from being perfectly harmless, is generally attended with serious consequences; that it is almost invariably mortal, when the ligature remains permanently applied, since it is capable of causing death in a short time, by the sole agency of the disturbances it occasions in the system, and so much the more rapidly that the substances ingested more powerfully excite vomiting; that, in short, it is almost always followed, a short time after its application, by grave symptoms, which must be taken into consideration in the appreciation of the phenomena observed when the effects of substances ingested into the stomach are investigated.

The importance of a communication which shook all Orfila's toxicological edifice did not admit of indifference, and the Academy appointed a committee to resume and control the experiments of the Professors of Alfort, and the report of this committee was read by Dr. Trousseau on the 29th July last.

In this remarkable production, Dr. Trousseau successfully examined the two principal questions of fact and of interpretation. It was first necessary to ascertain whether the ligature of the œsophagus is simple and harmless, as Orfila declared it to be, or whether it is attended with serious disturbance, as Messrs. Bouley and Reynal assert. Further experiments were made at the Hospital of Val-de-Grace. Messrs. Bouley and Reynal performed anew the ligature of the œsophagus on five dogs, carefully avoiding to comprise the nervous filaments in the loops. Notwithstanding this precaution, extreme agitation, retchings, and nervous disturbance, were observed in the animals, and these symptoms singularly contrasted with the mere depression spoken of by Orfila. The temporary ligature, however, killed but one dog out of thirty-one. The permanent ligature, as was to be expected, was fatal to twenty-two dogs out of twenty-three. The greater part of these animals died from the third to the sixth day.

The most important question of the debate was to ascertain whether the fatal results of ligature of the œsophagus, after the ingestion into the stomach of substances subjected to experimentation, might not be the consequence of the combined action of the ligature of the œsophagus and the necessarily powerless efforts to reject these substances, even when not possessed of poisonous properties. This fact seemed confirmed by the death of a certain number of dogs that had taken, before the operation, from one to three drachms of sea-salt, of sulphate of zinc, of nitrate of bismuth, and even of lukewarm water. We will say nothing on the question of interpretation, it remained full of obscurity, and it is not precisely known why and how the victims of these experiments died under circumstances in which, according to Orfila's affirmations, they should have experienced scarcely more than discomfort, or some insignificant disturbance.

How deceptive or dangerous soever the results of experiments made on animals, in less practised hands than those of Orfila, may now be, we cannot admit that a man gifted in so high a degree with the talent of observation could have deluded himself for forty years, and have confounded the phenomena peculiar to the action of poisonous substances with those which owe their origin to the ligature of the œsophagus.

Dr. Cloquet, who witnessed several hundred experiments performed by the late Dean of the Faculty of Medicine (Orfila), proved, in a few words in the discussion, that the ligature of the œsophagus, performed with a thick and rather loose string, had never appeared to him to have the gravity now ascribed to it, and that Orfila knew perfectly well how to make allowance for the disturbance of functions peculiar to the operation. We may add that, after the vote on the conclusions of the report of the committee, which were considerably modified, on the motion of Dr. Devergie, Dr. Velpeau expressed the regret that, before proceeding to a somewhat precipitate vote, the Academy did not take into consideration a letter from Dr. Nonat, in which this gentleman declares that a series of twenty-two experiments made by him fifteen years since, with Messrs. Sandras, Deville, and Guibourt, will convince any one who will read the details, that the ligature of the œsophagus has never occasioned any accidents of a nature to cast doubts upon the results of toxicological researches.

The upshot of these contradictory assertions is, in our estimation, a very posi-

tive fact, viz, that the ligature of the œsophagus was performed by Messrs. Bouley and Reynal, if not with less care, at least *otherwise* than by Orfila. The whole question here lies in the difference of the manner of operating. It is for experimentalists to investigate that of the master; but, in the mean time, we cannot see, in the conclusions of the report of the committee, matter important enough to assail the imperishable monument raised to the science of poisons by the great and noble mind of Orfila.—*Dublin Hosp. Gaz.*, Oct. 1, 1858, from *Journal of Pract. Med. and Surg.*

MISCELLANEOUS.

46. *Ozonometer.*—Dr. LANKESTER exhibited to the Chemical Section of the British Association for the Advancement of Science, at its late meeting in Leeds, an instrument for measuring the constant intensity of ozone. This instrument consisted of two small rollers included in a box, which were moved by means of ordinary clockwork. Over the roller a strip of paper, prepared with iodide of potassium and starch is allowed to revolve, the paper becoming exposed to the air for an inch of its surface in the lid of the box. Twenty-four inches of paper pass over the rollers in the course of twenty-four hours, and thus registers, by its colour, the intensity of the action of ozone in the atmosphere. By this instrument, the intensity of the ozone for every hour in the twenty-four could be registered, and *minima* and *maxima*, with an average, ascertained. The register of ozone could also be compared with those of the anemometer, and the relation of ozone to the direction and force of the wind ascertained. Dr. Lankaster pointed out the importance of ascertaining the presence of ozone, on account of its undoubted relation to health. He drew attention to a series of tables which had been drawn up from the registrations of the anemometer made at London, Blackheath, and Felixstow, on the coast of Suffolk. From these it was seen that the relation of these three places was 0, 22, and 55. The instrument acted also as a clock, and the time could be accurately marked upon the ozonized paper.

Mr. MARSHALL made some remarks on his own observations during the last twelve months, and stated that he had not been able to discover, though assisted in the investigation by medical gentlemen, that there was any obvious connection between ozone and the state of health.—*British Med. Journ.*, Oct. 16, 1858.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

A somewhat Remarkable Case of Strangulated Hernia. By WALTER F. ATLEE, M. D.

The following case of strangulated hernia appears to be worthy of being recorded, from the long persistence of the symptoms after relief had been afforded to the obstruction, by an operation, and also from the nature of the matters vomited.

Mrs. S——, aged 60, residing in Logan Street, in this city, after suffering from Saturday until the afternoon of the ensuing Wednesday from strangulation of a femoral hernia, of six years' standing, for which a bandage had never been worn, consented to undergo an operation for its relief. Stercoraceous vomiting had then existed for twenty-four hours, before her attending physician, Dr. Shapleigh, could obtain her consent to his calling in any further aid from a surgeon.

The contents of the sac—which was about 7 inches in circumference—consisting of both intestine and omentum, were reduced after enlarging the opening in Poupart's ligament, and dividing freely the neck of the sac

The stercoraceous vomiting, however, continued until the following Sunday, when early in the morning an enormous discharge of feces occurred from the bowels, and all symptoms of strangulation ceased. The day after the operation enemata containing assafœtida were administered, and soon afterwards the matters that were thrown off from the mouth contained that substance, or, at least, were most strongly impregnated by its odour.

The patient entirely recovered.

DOMESTIC SUMMARY.

On the Action of Certain Vegetable Diuretics. By WILLIAM A. HAMMOND, M. D., Assistant Surgeon U. S. Army.—The ensuing investigations consist mainly of repetitions of those performed some years since by Krahmer, and subsequently by Bird. They have reference to the appreciation of the influence of squill, juniper, digitalis, and colchicum, over the quantity of the urine, its specific gravity and the amount of its solid organic and inorganic constituents. They were all performed upon healthy adult males.

The quantity of urine was determined in cubic centimetres, and the weight of solids in grammes.

The method employed for the determination of the whole amount of solid matter was as follows:—

Ten cubic centimetres of the urine were evaporated to as complete dryness as possible *in vacuo* over sulphuric acid, and the residue accurately weighed. By simple proportion the amount of solids in the whole quantity of urine was easily ascertained.

Although it is impossible to get rid of all the water by this process, the quan-

tity remaining is extremely small, and the results obtained are far more accurate than those yielded by evaporating to dryness in the water-bath as generally practised. No matter how carefully this latter method is conducted, the loss of urea by decomposition is always an important item, and involves far more serious errors than the imperfect desiccation by the former process.

For the determination of the amounts of organic and inorganic constituents separately, the solid residue obtained as above was mixed with ten or fifteen drops of moderately strong nitric acid, and gently heated till the mass was well dried. The heat was then gradually raised till all the carbon was consumed, and the mass in consequence became white. It was then cooled *in vacuo* over sulphuric acid and weighed. The inorganic matter was thus determined and the loss showed the proportion of organic substance.

Digitalis.—The subject of the experiments with this substance, was about twenty-five years of age and in good health. For the three days immediately preceding the commencement of the investigations the average quantity of urine daily excreted by him was 1474.5 cubic centimetres, the specific gravity was 1024.30, and the average total amount of solid matter was 75.31 grammes, of which 30.17 grammes were inorganic, and 45.14 organic constituents. The digitalis was given in the form of the officinal tincture in doses of 20 minims three times in 24 hours, and was continued for three consecutive days. During this period the manner of living (food, drink exercise, &c.,) was as nearly as possible the same as during the preliminary investigations.

1st day. The urine passed on this day was of a pale straw colour and feeble acid reaction; quantity 1950 cubic centimetres; specific gravity 1013.25; total solids 69.98 grammes, of which amount 31.27 were inorganic and 38.71 organic matter. The action of the digitalis was not manifested otherwise than by its effect upon the urine.

2d day. The urine passed on this day was of similar physical character to that above mentioned. The quantity was 1873.6 centimetres, the specific gravity 1014.32, and the total solids 63.74 grammes. The inorganic solids amounted to 30.15 grammes, and the organic to 33.49.

The pulse on this day was somewhat slower and fuller than on the previous day.

3d day. The quantity of urine evacuated on this day was 1624.9 cubic centimetres, and of specific gravity 1020.04. The total amount of solid matter was 67.29 grammes, of which 33.19 were inorganic and 34.10 organic.

The colour, reaction, and odour of the urine were similar to those of the two previous days.

The characteristic effects of the digitalis upon the action of the heart were well marked during this day.

The effect of the digitalis in increasing the amount of urine is seen to have been greatest on the first day. On the second day it had fallen somewhat, and on the third was but 150 cubic centimetres greater than when no digitalis was taken. The solids, it is seen, were less than the normal standard from the commencement, were still further reduced on the second day, and on the third were slightly increased. This diminution is perceived to have been owing to the lessened amount of organic matter excreted. The inorganic substances were somewhat increased in amount over the ordinary proportion.

Juniper.—The experiments with this substance were conducted on a healthy man thirty-five years of age. The average condition of his urine for the three days immediately preceding the investigations was as follows: quantity 1237.5 cubic centimetres, specific gravity 1022.50; total solids 61.23 grammes, of which 23.12 were inorganic, and 38.11 organic matter. It was of ordinary colour and odour, and of strong acid reaction.

Sixteen ounces of the officinal infusion of the berries of the *Juniperus communis* were taken during the twenty-four hours, and the manner of living kept as nearly as possible to correspond with that of the preliminary experiments.

1st day. For this day the quantity of urine amounted to 1732 cubic centimetres, the specific gravity of which was 1016.38; the total solids were 62.75 grammes; of this amount 25.43 grammes were inorganic, and 37.32 organic constituents.

The urine was of a pale straw colour and gave off the characteristic odour produced by juniper. The reaction was feebly acid.

2d day. The quantity of urine passed on this day was 1885.2 cubic centimetres. The specific gravity was 1014.15, and the total solids 58.49 grammes, 22.17 of which were inorganic, and 36.22 organic matter. The physical characteristics were similar to those of the day before. The reaction was barely acid.

3d day. On this day the quantity of urine was 1672.5 cubic centimetres, with a specific gravity of 1018.41. The total solids amounted to 63.27 grammes, of which 27.50 where inorganic, and 35.73 organic matter. The physical characteristics and reaction were the same as on the previous day.

From these experiments it is seen that whilst the quantity of urine was materially increased by the juniper, the amount of solid matter, as a whole, was but slightly affected, the loss in organic matter being about compensated for by the increase in the inorganic.

Squill.—The experiments with this substance were instituted upon myself, and were conducted upon the same general principles as the foregoing series. The average daily quantity of urine, for the three days preceding the investigations, was 1358 cubic centimetres. The specific gravity was 1023.51, and the total solids 69.35 grammes; of this amount 27.22 were inorganic, and 42.13 organic matter.

I took two grains of the dried bulb of the *Scilla maritima*, three times in the twenty-four hours. The other conditions remaining the same as in the preliminary examination of the urine.

1st day. The quantity of urine passed on this day was 1572 cubic centimetres of 1020.34 specific gravity. The total solid matter was 6067 grammes, 31.07 of this amount being inorganic, and 29.60 organic constituents. The urine was of feeble acid reaction.

2d day. Quantity of urine 1493.5 cubic centimetres, specific gravity 1020.90, total solids 58.22 grammes, inorganic matter 30.15, organic 28.07 grammes. The reaction, &c., were the same as on the preceding day.

3d day. On this day the quantity of urine amounted to 1535 cubic centimetres, and was of 1019.37 specific gravity. The total amount of solid matter was 61.58 grammes, of which 30.58 were inorganic, and 31.00 organic constituents. The reaction, colour, &c., were unchanged.

From the above experiments it is percieved that the action of the squill was similar to that of the digitalis and juniper, *i. e.*, causing an increase in the water of the urine and inorganic solids, but a reduction of the amount of organic matter. The loss of organic matter was considerably greater than with either of the other substances.

Colchicum.—The investigations into the action of this substance were performed upon a healthy man 28 years of age. The urine for the three days immediately preceding the commencement of the experiments, was of the following daily average character; Quantity 1230 cubic centimetres, specific gravity 1025.08; total solids 63.12 grammes, inorganic matter 29.83 and organic 33.29. The reaction was very strongly acid.

One and a half drachms of the officinal tincture of the seeds of the *Colchicum autumnale* were given three times in twenty-four hours, and continued for three days. During this period the food, exercise, &c., were as nearly as possible the same as during the preliminary series.

1st day. The quantity of urine passed on this day was 1595.7 cubic centimetres, with a specific gravity of 1024.37. The total solids amounted to 77.29 grammes, the inorganic matter of which was 36.50 grammes, and the organic 20.79 grammes. The reaction was strongly acid.

2d day. Quantity of urine 1484.1 cubic centimetres, specific gravity 1024.31; total solids 75.22 grammes. The amount of inorganic matter was 35.01 grammes, and of organic 40.21. The reaction was very strongly acid.

3d day. On this day the quantity of urine amounted to 1620 cubic centimetres, and was of 1022.06 specific gravity. The total amount of solid matter was 79.33 grammes, of which 34.20 were inorganic, and 45.13 organic constituents. Reaction strongly acid.

It is thus perceived that the action of the colchicum, as compared with that of

the other substances experimented with, was very remarkable, it being the only one with which there was an increase in the amount of solid matter eliminated, both organic and inorganic.

From the foregoing experiments the following table embracing the averages of each series of investigations is constructed.

	Quantity of urine.	Specific gravity.	Total solids.	Inorganic solids.	Organic solids.
Normal standard	1474.5	1024.30	75.31	30.17	45.14
Digitalis	1822.8	1015.87	67.00	31.54	35.43
Normal standard	1237.5	1022.50	61.23	23.12	38.11
Juniper	1763.2	1016.28	61.50	25.03	36.42
Normal standard	1358.0	1023.51	69.35	27.22	42.13
Squill	1533.5	1020.20	60.15	30.60	29.55
Normal standard	1280.0	1025.08	63.12	29.83	33.29
Colchicum	1556.6	1023.58	77.28	35.23	42.04

From the foregoing investigations, I think it is deducible that neither digitalis, juniper, nor squill, increases the total amount of solid mater eliminated by the kidneys, and that the organic matter is considerably reduced through their influence. Although they do increase the amount of inorganic matter removed through the urine, yet as it is the organic matter which is generally considered as contaminating the blood in disease, it is evident they exert no effect whatever in depurating this fluid, but on the contrary are positively injurious.

The results obtained, in so far as the experiments with digitalis, squill, or juniper, are concerned, are similar to those obtained by Krahmer, but are materially different as regards the colchicum. For, although Krahmer found that under the influence of this medicine there was an increase in the amount of organic matter excreted, this was so small as to lead to the supposition that it may have been accidental, and besides there was a reduction in the quantity of inorganic substance removed. It is desirable, therefore, that we should have further observations with this article.—*Proc. Biol. Depart. Acad. Nat. Sciences of Philadelphia*, Dec., 1858.

Clinical Study of the Heart-sounds.—Prof. AUSTIN FLINT has published (*New Orleans Med. News and Hospital Gazette*, Sept. and Oct., 1858) two very interesting letters on this subject. The following series of propositions are given by him as embodying the practical points pertaining to the diagnostic signification of the abnormal modification of the heart-sounds:—

1. Increased intensity of the first sound, the two elements composing this sound being affected equally, is a sign of excited, muscular action of the heart, and is observed in cases of functional disorder without organic disease.

2. Increased intensity of the element of impulsion in the first sound, the intensity of the valvular element not being proportionately augmented, if at all, is a sign of hypertrophy affecting the left ventricle.

3. Diminished intensity of the element of impulsion is a sign of weakened muscular power of the left ventricle, either from organic affections, such as dilatation, or fatty degeneration, or from functional debility of the organ. Cases are to be excluded in which, from the presence of liquid effusion in the pericardium or pleura, or from emphysema, the heart is prevented from coming into contact with the thoracic walls.

4. Abnormal intensity of the mitral valvular element of the first sound, is a sign of excited muscular action of the heart, and is accompanied by a corresponding increase of the intensity of the element of impulsion, as stated in prop. 1st. Abnormal weakness and suppression of this element, the element of impulsion retaining or exceeding its normal intensity, are signs of more or less injury of the mitral valves. A murmur referable to the mitral orifice coexists in the

vast majority of cases. Notwithstanding the murmur, if the valvular element of the first sound referable to the mitral valves, retain nearly or quite its normal intensity, the valves are not seriously damaged. In judging of the normal intensity of the mitral valvular element, it may be compared with the sound emanating from the tricuspid valves.

5. Abnormal intensity of the valvular element referable to the tricuspid valves, is a sign of hypertrophy of the right ventricle, and is generally associated with diminished intensity of the valvular element referable to the mitral valves. Abnormal weakness of the tricuspid valvular element is not available as a physical sign of disease.

6. A *positive* increase of the intensity of the pulmonic second sound of the heart, is a sign of hypertrophy of the right ventricle, in the majority of cases dependent on mitral contraction or insufficiency, or both. A *relative* increase of this sound, *i. e.*, as compared with the aortic second sound, may result from abnormal feebleness of the aortic sound, due to mitral obstruction or regurgitation.

7. Abnormal intensity of the aortic second sound, is not a sign of much importance. But non-diminution of its intensity, in cases in which a murmur referable to the aorta is present, is a sign of much value, indicating that, although aortic lesions exist, the integrity of the valves is not seriously compromised.

8. Diminished intensity of the aortic second sound, in cases in which a murmur referable to the aorta is present, is a sign that the aortic valves are damaged, provided that neither mitral obstruction nor regurgitation exists, an effect of the latter being abnormal feebleness of this sound. If the diminished intensity of the aortic sound be due to injury of the valves of the aorta, there will be likely to be present an aortic regurgitant murmur, in other words, a diastolic murmur referable to the aorta.

9. In cases in which a diastolic murmur is present, referable either to the direct current of blood through the mitral orifice, or to aortic regurgitation, a normal intensity of the aortic second sound is evidence that the lesions giving rise to the murmur are seated at the mitral orifice.

Report of thirteen Cases of Ununited Fracture treated by Subcutaneous Perforation of the Bone.—Prof. Brainard states (*Chicago Medical Journ.*, Sept., 1858), that since the publication of his essay on this mode of treating fractures (*Trans. Am. Med. Assoc.*, vol. vii. 1854), a more extended experience has enabled him to form a more just appreciation of the value of this treatment, to ascertain its advantages and defects, and to suggest some improvements in the manner of its performance; and further, that the views formerly expressed concerning the efficacy and safety of the operation are in the main confirmed.

Of the 13 cases of ununited fracture reported in the present paper, *four* were of the *humerus; four* of the *femur; three* of the *tibia*, and *two* of the *ulna.*

1. *Of the humerus.*—The first of these was of four months' standing, in a healthy man, 30 years of age. Two operations at an interval of ten days were performed, and a cure effected in one month.

The second was of eight months' standing, in a healthy man, 29 years of age. Four operations by perforation were performed at intervals of ten days. Union was advancing, but the patient then put himself under the care of another surgeon.

The third case was one of six months' standing, in a healthy man 24 years of age. Five operations were performed at intervals of a week, and a cure was effected in six weeks.

The fourth case was of five months' standing, in a man whose age and general condition are not stated. He was treated for five weeks by six perforations without benefit. The seton and then resection were tried without producing union.

2. *Of the femur.*—The *first* of these was in a man 35 years of age, delicate health, of five months' standing. Treatment by eleven perforations during five months; cure.

The *second* was of four months' standing, in a man 56 years of age, treated by four operations, and cured in six weeks.

The *third* was also of four months' standing, in a healthy young man; cured by four perforations.

The *fourth* case was of five months' standing, in a healthy man aged 36 years. Cured in four weeks by one operation.

3. *Tibia.*—The *first* of these was of four months' standing, in a man cured in two weeks by one perforation.

The *second* was in a man 25 years of age, of good constitution; cured in five weeks by four perforations.

The *third* was of seven months' standing, in a man of good constitution, 21 years of age; cured in four weeks by four perforations.

4. *Ulna.*—The *first* of these was in a labourer, and was of three months' standing. Cured in 22 days by two perforations.

The *second* in a man 35 years of age, a drunkard, of 18 weeks' standing; cured in four weeks by two perforations.

In none of the above cases, Dr. B. states, did any serious accident occur. "In one," he observes, "a small abscess, and in another, a subject of bad constitution, some swelling, resembling erysipelas, which, however, soon subsided. These were the most serious results of about sixty perforations. We may therefore assert, with great certainty, that this operation, unless performed upon patients in a condition unfit for any operation, is entirely safe.

It will also be noticed, that, while in cases of fracture of the tibia, where apposition is perfect and the movement slight, a single perforation speedily induced union in a few days; on the other hand, fractures of the humerus and of the femur did not, in most cases, require less than four operations, nor unite in less than four weeks, while one required five months and eleven perforations to effect a cure, and another did not unite at all.

My practice at present is to commence the treatment by two or three perforations of the bone through a single opening of the skin, using an instrument of small size, repeating this every ten days or two weeks, gradually increasing the size of the instrument and the extent of the wound of the bone, until tenderness and some swelling are induced. I have very uniformly found that when pain and throbbing are felt in the seat of fracture, union has commenced.

"*Direction of the Perforator.*—That point and direction of puncture should be chosen which affords the easiest access to the bony surfaces with least exposure of vessels. In many cases of oblique fracture, traversing the bones answers well. In others, as of the tibia, I have found that following the direction of fracture is best. In others, still, when the ends are not perfectly in contact, I make a perforation between, and direct the instrument first in one direction, then in the osher; while in others, still, the instrument can be passed most readily between the bones and attack them at the side.

"*Size of the Instrument.*—In cases of ununited fracture of the tibia, or radius and ulna, where the ends are in contact and the wounds slight, I use a perforator no more than two or three *millimetres* in breadth; while, in old cases, situated in the femur and humerus, and when there is great mobility, it is as well to use an instrument one-eighth of an inch and over in breadth. In such cases, very extensive wounding and perfect denudation is required. It was not found that the bones had in any case lost their natural feeling of density.

"*Causes of Want of Union.*—In all the above cases, the causes of non-union were imperfect apposition, or a dressing admitting of too great mobility, or accidents producing displacements, or indocility of the patient. In three of the cases, the fragments were separated from each other by a sensible space, as shown by the instrument in perforating them. It is especially to be noticed, that the most efficient means for securing immobility were in every case conjoined with the treatment by perforation. These means were such as are generally known and used."

A case of ununited fracture of the forearm, treated unsuccessfully by this method, will be found in the original department of this number. (p. 136–141.)

Dislocation of the Fourth and Fifth Cervical Vertebræ.—Dr. W. M. RYER records (*Pacific Med. & Surg. Journ.*, Sept. 1858) a case of this rare accident. The subject of it was a girl 7 years of age, of lymphatic constitution, the daugh-

ter of Dr. Hepburn, of Mokelumne Hill. When seen by Dr. R., the patient's head "was most singularly and immovably fixed, much bent to the side, the ear approximating but little in advance of the right shoulder, and in a position no child in a normal condition could for a moment assume; the slightest motion tending to change the relative position of the head and body producing intense pain.

"The father, Dr. Hepburn, an aged and very intelligent practitioner of medicine, had watched the child with a parent's solicitude for the six previous days and nights, and neither during sleeping or waking did the child move its head from the position it had assumed from the instant of the accident. As the right clavicle was fractured at the time, the Doctor was inclined at first to believe the child was favouring the fracture and was unwilling to entertain the unpleasant thought of so serious a complication as luxation of the spine.

"The child had fallen six days previous to my visit, from a high bed, and is supposed to have struck the back and left side of her head. The father saw her within a half minute after, and found her head and neck distorted precisely as at the time of my examination; there had been no change for six days. Such distortion, I believed, must have arisen from muscular contraction or bony displacement. We examined every muscle whose contraction would be likely to produce the deformity, and found them loose, soft, uncontracted. Upon tracing the spinous processes from below to the articulation of the fourth and fifth cervical vertebræ we found them form, at this point, an obtuse angle, and depart from the natural direction about 40 degrees. The intellectual faculties were good, and sensation and motion not greatly impaired. I could form no other diagnosis than was formed by the medical gentlemen in attendance previous to my visit. It was clearly a dislocation of the left oblique articulating process—the process of the fourth riding over the upper margin of the one with which it was articulated below.

"As objections were made to the administration of chloroform, we attempted the reduction without it, and failed. We then administered this anæsthetic and succeeded to our fullest anticipations, Dr. Soher, and other gentlemen who assisted; distinctly recognizing the instant of time when the reduction was effected. The child immediately had full motion of her head and neck, and is now entirely recovered."

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Changes of the Blood-Cells in the Spleen.—The opinions of physiologists as to the functions of the spleen have been various. Some, as Funke, Hewson, Bennet, &c., believe it to be a generator of blood-cells, while Kölliker and others maintain that it is a destroyer of them. Dr. Henry Draper relates (*N. Y. Journ. Med.*, Sept. 1858) some microscopic investigations made by him on the blood of frogs taken from the splenic artery and splenic vein, and he found the latter to contain at least double the general average of imperfect cells; whence he infers that "the spleen must be an organ for the disintegration of blood-cells."

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The Extent to which Ether should be used in Midwifery.—In a discussion on this subject at a late meeting of the Norfolk District Med. Soc. (Mass.), Dr. Cotting of Roxbury, gave the following as the result of his experience:—

"In our own individual experience in several hundred cases of normal labour, we have been led to observe—that only a very few patients were capable of taking just that amount which would deaden the acuteness of the suffering without at the same time diminishing the frequency and effectiveness of the uterine contractions—that generally, as suspension of consciousness approached, there was a marked and proportionally complete suspension of the expulsative efforts—that, with the greatest care possible under the circumstances, there was frequently more or less irritation of the air-passages; often troublesome coughing; sometimes nausea and vomiting, attributable directly to the anæsthetic; also, occasionally strong tendencies to hysterical manifestations, which sometimes continued after the labor was over; with other minor inconveniences, such as unwonted impatience, jactitation, &c. &c.; so, also, instances, not a few, of subsequent retention of urine; as well as post-partum hemorrhage from imperfect uterine contraction, apparently due to the same agent—that, although something was apparently

gained by the occasionally greater relaxation of the organs, the duration of these labours was unmistakably longer than those of a similar character in which an anæsthetic was not used; and, in general, there seemed to be greater subsequent debility, and a slower getting up than was to have been expected—that we have never witnessed any undoubted evidence of subsequent *permanent* injury to the life or health of the mother or child arising from the use of ether during labour.

"In abnormal cases—from a considerable experience in all the various operations from podalic version to craniotomy and other disintegration of the fœtus, both before and since the discovery of these anæsthetics—our conclusion is that while the judicious use of ether immeasurably increases the ease, certainty and effectiveness of obstetric operations, the insensibility of the patient, when desirable, and her comparative safety, are benefits to be obtained through its administration whose value is beyond all estimation.

"In puerperal convulsions, whether identical with uræmia, according to the latest theory, or otherwise, anæsthetics during the paroxysms seem to be supplanting what but yesterday was considered the only orthodox practice. The convulsions seemed to be completely controlled by the use of these agents in the few cases in which we have had occasion to administer them.

"Such has been our private experience. We do not know that it is at variance with that of any observant practitioner. Whatever suggestions we may have gained from the reports and practice of others, it is not improper to say that we here advance nothing practical which has not been confirmed by personal observation, the results of which alone are suited to this occasion and the object of the present discussion.

"Bearing in mind, then, that the great object of our art is the diminution of human suffering; and that in the economy of nature the pains of parturition may have some ultimate beneficial purpose; and further that, as sufficient time has not yet elapsed since the discovery of the anæsthetic powers of these agents to fully disclose all the consequent effects of their administration, much must be left in each individual case to the intelligence, judgment and tact of the medical attendant—bearing all this in mind, we conclude with the following generalizations:—

"I. That in ordinary cases of midwifery, while *ether* may be allowed in moderation when importunately demanded by the patient, it is quite as well in the long run, to say the least, to let normal, uncomplicated labours proceed uninterfered with.

"II. That in painful, laborious, or complicated labour, and in cases of great tenderness or great rigidity of the organs, of extraordinary susceptibility to pain, and where there is great nervous irritability, or undue apprehension of danger, ether, if favourably received, should be used to the extent of overcoming the abnormal condition and suffering.

"III. That in cases requiring manual or instrumental interference, ether should be used to the same extent, and upon the same general principles as in other operations involving pain and danger to the patient.

"IV. That in puerperal convulsions, especially in those having the characteristics of uræmic eclampsia, ether should be given as soon as there are indications of an approaching fit, and be continued, if seemingly efficacious, until the paroxysm has subsided and quiet sleep is induced; or until other medicine, if desirable, can be swallowed—care being taken to allow a sufficiently large quantity of pure air, and not to continue the ether if coma supervene.

"V. That all the volatile anæsthetics yet tried, except ether, have been known to cause severe accidents, and even instant death, though given with the greatest care by experienced practitioners, and this, too, before any considerable quantity had been inhaled; ether only should be used as an anæsthetic in midwifery. Ether, likewise, should be administered with the greatest caution, so that the safety of the patient may not be unnecessarily put at hazard."—*Boston Med. and Surg. Journ.*, Dec. 9, 1858.

Reduction of an Inverted Uterus of Fifteen Years' duration.—Prof. WHITE reported to the Buffalo Med. Assoc. (Sept. 7, 1858) a case of this. The patient was 32 years of age, and suffered the inversion when she was seventeen years

old, after labour with her second child. Fourteen days after the accident, she was seen by Dr. White and inversio uteri was diagnosed. She has since been subject to repeated hemorrhages and constant leucorrhœa, and is much reduced and anæmic. Reduction was effected on the 24th of August, in the presence of Drs. Flint, Rochester, Flint, Jr., and Mason. The time occupied in reduction was about fifty minutes. The patient was placed in the position before described, and chloroform given to moderate anæsthesia. The manipulations were the same as in the cases previously reported by Prof. White, and the difficulty of reduction was little, if any, greater than in the case of six months' standing. After the operation a speculum was introduced and all present saw the mouth of the uterus; the reduction was verified by measurements. She was then put to bed, and the fundus gently supported by a small rectum bougie for four hours. She improved rapidly, with no untoward symptoms during the succeeding eight days, and at the end of that time considered herself perfectly well. On the morning of the ninth day, however, after returning from breakfast, she imprudently strained considerably at stool, and was suddenly seized with violent pains in the abdomen. She was at the same time startled by the coming in of a friend while she was on the vessel. The mother and husband had left her a day or two before, considering her perfectly well. She immediately went to bed, seized with excruciating pain, and sent for Dr. White. She died on the seventh day, however, of peritonitis.

A post-mortem examination was made, six hours after death, in the presence of the gentlemen who were present at the reduction. A considerable quantity of liquid effusion, turbid and containing flocculi of lymph, was contained within the peritoneal cavity. The liquid was not measured; it would probably amount to from two to three pints. No fecal odour, nor much fetor on opening the abdomen. Collections of recently exudated lymph at different points between the convolutions of the intestines, and within the pelvic cavity. The intestines moderately tympanitic. Appendix vermiformis normal.

The uterus, with its appendages, having been removed, the exterior surface of the organ presented a few patches of soft, loosely adherent lymph; otherwise the appearance was normal. The organ was that of a normal uterus from the body of a person who has borne children. The os presented nothing abnormal. There was no trace of laceration anywhere. The structure of the organ and its inner surface, presented a healthy appearance. The ovaries were normal in size and exterior aspect.

Other organs not examined. No morbid appearances were observed exclusive of those which denoted recent, acute, general peritonitis.—*Buffalo Med. Journ.*, Oct. 1858.

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Obituary Record.—Died, at New Haven, Ct., on the 22d of Sept., 1858, Timothy Phelps Beers, M. D., in the 69th year of his age.

Dr. Beers attended medical lectures at the University of Pennsylvania in the winter of 1811–12, and in the spring of 1812, commenced the practice of his profession in his native city of New Haven, where he spent the remaining period of his life. In the summer of 1813 he served for several months as surgeon in a regiment of militia stationed at New London, Ct.; which place was at that time blockaded by a British fleet.

Dr. Beers was Professor of Obstetrics in the Medical Institution of Yale College from the year 1830 to 1856. He, for a long time, and up to the period of his decease, stood in the front rank in his profession; wise, skilful, and successful in general practice—in the obstetric department his skill was *pre-eminent*, and his practice therein very extensive—for many years much more extensive than that of any other man in this city or vicinity. As a professor and teacher of medicine, his lectures were truthful, instructive and interesting.

Dr. Beers was a man of first rate common sense, and of a kind, genial, and cheerful disposition, and possessed of a ready humour ever at command. He was of excellent moral and Christian principles, which ruled and adorned his daily life. On the occasion of his interment a brief address was pronounced by his intimate and affectionate friend of more than half a century, Prof. J. Knight, of this city; also by the Rev. Dr. Dutton, of whose church Dr. Beers had for a long time been a member. G. O. S.

THE

AMERICAN JOURNAL

OF THE MEDICAL SCIENCES

FOR APRIL 1859.

CONTRIBUTORS TO THIS VOLUME.

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TO READERS AND CORRESPONDENTS.

The following works have been received:—

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Volume the Forty-First. London: Longman, Brown, Green, Longman, and Roberts, 1858.

The Transactions of the American Medical Association. Instituted 1847. Vol. XI. Philadelphia, 1858.

Theory of Consumption: Dr. McCormac's Letter to the Imperial Academy of Medicine. Published in "L'Union Medicale" and "Gazette des Hopitaux," with comments on the mortality among the Guards, and a reply to the "Westminster Review," followed by twenty aphorisms in respect of health and healthy respiration. London, 1858.

Excision of the Knee. By Geo. M. Humphry, Surgeon to Addenbrooke's Hospital. London, 1858. (From the Author.)

Neuralgia and Paraplegia, supposed to be due to the long-continued use of Arsenic, of which a trace was found in the Liver and Bones. Sugar in the Tears of Diabetes. Double Sub-pericranial Cephalhæmatoma. By Dr. G. D. Gibb, M. A., F. G. S., &c. London, 1858. (From the Author.)

Third Report of the Clinical Hospital, Manchester: Containing results on physical development, hooping-cough, and transmitted diseases. By James Whitehead, M. D., London. John Churchill. (From the Author.)

A Treatise on Human Physiology, designed for the use of Students and Practitioners of Medicine. By Jno. C. Dalton, Jr., M. D., Prof. of Phys. and Microscopic Anat. in the College of Phys. and Surg., New York; Member of the New York Acad. of Medicine, etc. etc. With 250 illustrations. Philadelphia: Blanchard & Lea, 1859. (From the Publishers.)

On Poisons in Relation to Medical Jurisprudence and Medicine. By Alfred Swaine Taylor, M. D., F. R. S., Fellow of the Royal College of Physicians, Hon. M. D. Univ. St. Andrews. Second American, from the second and revised London edition. Philadelphia: Blanchard & Lea, 1859. (From the Publishers.)

Nature in Disease. Illustrated in various Discourses and Essays. To which are added Miscellaneous Writings chiefly on Medical Subjects. By Jacob Bigelow, M. D., Physician and Lect. on Clin. Med. in the Mass. Gen. Hosp., Prof. of Mat. Med. in Harvard Univ., etc. Second edition, enlarged. Boston: Philips, Sampson & Co., 1859. (From the Publishers.)

Practical Dissections. By Richard M. Hodges, M. D., Demonstrator of Anatomy in the Med. Dep. of Harvard University. Cambridge: John Bartlett, 1858. (From the Author.)

Contributions to Operative Surgery and Surgical Pathology. By J. M. Carnochan, M. D., Prof. of Surg. in the N. Y. Med. Coll. With illustrations drawn from nature. Philadelphia: Lindsay & Blakiston, 1858.

A Treatise on Venereal Diseases. By A. Vidal (de Cassis), Surgeon to the Venereal Hospital of Paris, &c., with coloured plates. Translated, with annotations, by Geo. C. Blackman, M. D., Prof. Surg. in Med. Coll. of Ohio, &c. Third Edition. New York: Saml. S. & Wm. Wood, 1859. (From the Publishers.)

Trials of a Public Benefactor, as illustrated in the discovery of Etherization. By Nathan P. Rice, M. D., New York, 1859. (From the Author.)

Some account of the recent experiments made in connection with the case of M. Groux. By J. B. Upham, M. D., of Boston. (From the Author.)

Frequency, Importance, and Treatment of Ulcerations of the Os and Cervix Uteri. By D. McRuer, M. D. (From the Author.)

On the Management of the Shoulders, in Examinations of the Chest; including a new Physical Sign. Read before the New York Academy of Medicine. By John W. Corson, M. D., &c. New York, 1859. (From the Author.)

An Essay on the Treatment of Cataract. By MARK STEPHENSON, M.D. Read before the Am. Med. Ass. Philadelphia, 1858. (From the Author.)

Report of the Committee on Medical Literature. By A. B. PALMER, M.D. Extracted from the Transactions of the American Medical Association. Philadelphia, 1858.

Transfusion in Yellow Fever. By N. B. BENEDICT, M.D. (From the Author.)

Membranous Croup. By G. H. GAY, M.D. (From the Author.)

Congenital Exstrophy of the Urinary Bladder and its Complications successfully treated by a new Plastic Operation. Illustrated. By DANIEL AYRES, M.D., LL.D., Surgeon to the Long Island College Hospital, etc. etc. New York, 1859.

The Treatment of Dysentery. By W. S. FORWOOD, M.D., of Darlington, Md. Charleston, S. C., 1859.

The Hymen: An Essay delivered (by appointment) before the "New York Medical Union." By T. GAILLARD THOMAS, M.D. (From the Author.)

Quarterly Report of the City Physician. Jan., 1859. Boston, 1859. (From Dr. Clarke.)

Twentieth Annual Report of the Board of Trustees and Officers of the Central Ohio Lunatic Asylum to the Governor of the State of Ohio, for the year 1858. Columbus, 1859.

Fourth Annual Report of the Board of Trustees and Officers of the Northern Ohio Lunatic Asylum to the Governor of the State of Ohio, for the year 1858. Columbus, 1859.

Annual Report of the Inspectors of the Western Penitentiary, for 1858. (From Dr. A. M. Pollock.)

Report of the General Assembly of Kentucky relating to the Registry and Returns of Births, Marriages, and Deaths, in the State of Kentucky, from January 1st to December 31, 1857. Frankfort, Ky., 1858.

Report of the Pennsylvania Hospital for the Insane, for the year 1858. By THOS. S. KIRKBRIDE, M.D., Physician to the Institution. Philadelphia, 1859. (From the Author.)

Annual Reports of the Officers of the New Jersey State Lunatic Asylum, at Trenton, for the year 1858. Trenton, 1859. (From Dr. Butolph.)

Report of the Select Committee appointed to investigate the Health Department of the City of New York. Transmitted to the Legislature, Feb. 3, 1859. Albany, 1859.

Anniversary Address delivered to the Officers and Students of the New York Ophthalmic Hospital, on the 6th March, 1856. By ISAAC FERRIS, D.D., LL.D., President of the Board of Directors. New York, 1857.

Historical and Biographical Address delivered before the Cortland County Medical Society, at the Fiftieth Anniversary Meeting, August 10, 1858. By GEO. W. BRADFORD, M.D. Homer, 1859.

Valedictory Address to the Graduates of the Medical Department of Pennsylvania College. By DAVID GILBERT, M.D., Prof. of Mid. and Dis. of Women and Children. With a list of Graduates. Philadelphia, 1859.

Valedictory Address to the Graduating Class of the Philadelphia College of Medicine, at the Annual Commencement, March 2, 1859. By J. AITKEN MEIGS, M.D., Prof. of the Institutes of Medicine. Philadelphia, 1859.

Catalogue of the Faculty, Trustees, and Students of the Medical College of the State of South Carolina. Session of 1858–'59. Charleston, S. C.

State University of Michigan. A Catalogue of the Officers and Students, for 1859.

A Catalogue of the Trustees, Officers, and Students of the University of Pennsylvania. Session 1858–'59. Philadelphia, 1859.

The following Journals have been received in exchange:—

Journal de Médecine de Bordeaux. Nov., 1858. Jan., 1859.

Moniteur des Hôpitaux. December, 1858, Jan., Feb., 1859.

Annales Médico-Psychologiques. Par MM. les Docteurs BAILLARGER, CERISE, et MOREAU. January, 1859.

Journal de la Physiologie de l'Homme et des Animaux. Publié sous la direction du Docteur E. Brown-Séquard. October, 1858.

British Medical Journal. Edited by Andrew Winter, M.D. Dec., 1858, Jan., Feb., 1859.

Ophthalmic Hospital Reports, and Journal of the Royal London Ophthalmic Hospital. Edited by J. F. Streatfeild. October, 1858.

Dublin Hospital Gazette. December, 1858. Jan., Feb., 1859.

Edinburgh Medical Journal. December, 1858. Jan., Feb., 1859.

Medical Times and Gazette. December, 1858. Jan., Feb., 1859.

The Glasgow Medical Journal. January, 1859.

The British and Foreign Medico-Chirurgical Review. January, 1859.

The Liverpool Medico-Chirurgical Journal. January, 1859.

The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M. D. January, 1859.

Half Yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M. D., and C. B. Radcliffe, M. D. July—December, 1858.

Braithwaite's Retrospect of Medicine. July—December, 1858.

Dublin Medical Press. January, 1859.

Archives of Medicine. Edited by Lionel S. Beale, M. D. December, 1858.

The Medical Chronicle and Montreal Monthly Journal of Medicine and Surgery. Edited by Drs. Wright and MacCallum. Jan., Feb., March, 1859.

The Sanitary Review. Edited by B. W. Richardson, M. D. Jan., 1859.

Edinburgh Veterinary Review. January, 1859.

The Dublin Quarterly Journal of Medical Science. February, 1859.

Proceedings of the Academy of Natural Sciences of Philadelphia. Jan., Feb., March, 1859.

The Boston Medical and Surgical Journal. Edited by W. W. Morland, M. D., and Francis Minot, M. D. Dec., 1858. Jan., Feb., March, 1859.

The Maine Medical and Surgical Reporter. Conducted by Drs. W. R. Richardson and R. W. Cummins. Dec., 1858. Jan., Feb., 1859.

The Medical Journal of North Carolina. Edited by Edward Warren, M. D. December, 1858, Jan., Feb., 1859.

The Peninsular and Independent Medical Journal. Edited by A. B. Palmer, M. D., Moses Gunn, M. D., and F. Stearns. Dec., 1858. Jan., Feb., March, 1859.

The Medical and Surgical Reporter. Edited by S. W. Butler, M. D., and W. B. Atkinson, M. D. Dec., 1858. Jan., Feb., March, 1859.

The Belmont Medical Journal. December, 1858.

The North American Medico-Chirurgical Review. Edited by S. D. Gross, M. D., and T. G. Richardson, M. D. Jan., March, 1859.

The New York Journal of Medicine. Edited by Stephen Smith, M. D. Jan., March, 1859.

The American Journal of Insanity. January, 1859.

American Medical Monthly. Edited by Drs. Parker and Douglass. Jan., Feb., March, 1859.

New Orleans Medical News and Hospital Gazette. Edited by D. W. Brickell, M. D., and E. D. Fenner, M. D. Jan., Feb., March, 1859.

The Virginia Medical Journal. Edited by James B. McCaw, M. D. and J. Otis, M. D. February, March, 1859.

The American Journal of Science and Arts. Conducted by B. Silliman, B. Silliman, Jr., and J. D. Dana, March, 1859.

Oglethorpe Medical Journal. Edited by H. L. Byrd, M. D. Feb., 1859.

The New Orleans Medical and Surgical Journal. Edited by Bennett Dowler, M. D. March, 1859.

The St. Joseph Journal of Medicine and Surgery. Editorial Comm. J. H. Crane, M. D., O. B. Knode, M. D., G. C. Catlett, M. D. March, 1859.

The New Hampshire Journal of Medicine. Edited by G. H. Hubbard, M. D. December, 1858.

The Pacific Medical and Surgical Journal. Edited by John B. Trask, M. D., and David Worster, M. D. Dec., 1858. Jan., Feb., 1859.

Buffalo Medical Journal. Edited by Austin Flint, Jr., M. D. January, February, March, 1859.

Charleston Medical Journal and Review. Edited by J. DICKSON BRUNS, M. D. January, March, 1859.

American Druggists' Circular. Jan., Feb., March, 1859.

The New York Medical Press. Edited by Drs. KIERNAN and O'MEGHER. Jan., Feb., March, 1859.

American Medical Gazette. Edited by D. M. REESE, M. D. January, Feb., March, 1859.

Nashville Journal of Medicine and Surgery. Edited by W. K. BOWLING, M. D., R. C. FOSTER, M. D., and GEORGE S. BLACKIE, M. D. Jan., Feb., March, 1859.

The Cincinnati Lancet and Observer. Edited by Drs. STEVENS and MURPHEY. January, February, March, 1859.

The American Journal of Dental Science. Edited by CHAPIN A. HARRIS, M. D. and A. S. PIGGOT, M. D. January, 1859.

The Nashville Monthly Record of Medical and Physical Science. Edited by D. F. WRIGHT, M. D., and R. O. CURRY, M. D. December, 1858. Jan., Feb., March, 1859.

The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D., and J. W. HAMILTON, M. D. January, March, 1859.

Semi-Monthly Medical News. Edited by S. M. BEMISS, M. D., and J. W. BENSON, M. D. January, February, March, 1859.

The Chicago Medical Journal. Edited by N. S. DAVIS, M. D., and W. H. BYFORD, M. D. December, 1858. January, 1859.

The Louisville Medical Gazette. Edited by L. J. FRAZEE, M. D. January, February, March, 1859.

The Savannah Journal of Medicine. Edited by J. S. SULLIVAN, M. D., JURIAH HARRISS, and R. D. ARNOLD, M. D. January, March, 1859.

Atlanta Medical and Surgical Journal. Edited by J. P. LOGAN, M. D., and W. F. WESTMORELAND, M. D. Jan., Feb., March, 1859.

Southern Medical and Surgical Journal. Edited by Drs. H. F. and R. CAMPBELL. January, February, March, 1859.

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OF THE MEDICAL SCIENCES

FOR APRIL 1859.

ART. I.—*Contributions to the Literature of Carcinoma.* I. *On Transition Forms between Cancer and Innocent Growths.* II. *Anatomy of a Case of Retrograde Cancer.* By J. J. WOODWARD, M. D., of Philadelphia.

I. *On Transition Forms between Cancer and Innocent Growths.*—In a paper on the "Anatomical Diagnosis of Cancer," published in the *American Journal of the Medical Sciences* for January, 1859, in speaking of the distinctions between cancer and innocent connective tissue formations, I made use of the following language: "At the same time, I am disposed to admit the existence of transition forms, of which, standing as they do upon the boundary-line, no dogmatic opinion can be given." Since the publication of that paper, I have had, through the kindness of Professor Henry H. Smith, an opportunity of studying the minute anatomy of a tumour removed by him at the surgical clinic of the University of Pennsylvania. This tumour appeared to me, after a careful investigation, to deserve a place in the category of transition forms. Its study has been suggestive of a few thoughts, which will form the basis of the present article.

It will be remembered that the fundamental doctrine underlying the paper to which I have just referred was an utter rejection of the motion that cancer is a *heterologous* growth, in any proper sense of the word. On the contrary, the opinion was expressed, that, however obscured by imperfect development, by disordered growth, and by premature involution, it was quite possible to recognize the homology of cancer with ordinary developmental connective tissue, such as can be observed in granulations, in organizing lymph, in imperfectly developed fibrous tumours, or in the

normal developing connective tissue of the embryo. If this opinion be correct—if cancer formation be less a special and specific process than a modification of processes which ordinarily are much less terrible in their consequences—it might be anticipated, *a priori*, that instances would constantly occur in which, though the processes were disordered *in the cancerous direction*, the modification would not be sufficiently decided to enable us authoritatively to denominate the growth *a cancer*. Accordingly, the doctrine of homology above propounded receives fresh confirmation in the fact that the whole group of semi-malignant tumours, as they have with *some* propriety been called, are readily recognized by their minute anatomy to be in fact *new formations of connective tissue.*[1] Under this category must, indeed, be placed the *recurring fibroid* and *malignant fibroid* tumours of Paget, the *fibro-nucleated* tumour of Bennet, such cases as those described by Laurence (*Surgical Diagnosis of Cancer*, 2d edit.) as *malignant fibro-plastic* tumours, with lupus, keloid, and some others of the semi-malignant affections.

Viewed from this stand-point, every case belonging properly to the category of transition forms assumes a high degree of interest, and deserves the special study of the earnest pathologist. Above all, investigation should be directed to determine how far the deviation of the growth in one direction or another from the healthy, unmodified plan of connective tissue development is associated with its approximation to cancer in clinical history. Our present knowledge of this group of growths is so limited that we are not now in possession of facts which will justify us to attempt an answer to such a question, and, with some few exceptions, the histories of those cases of this kind which have been put upon record are not sufficiently minute in their account of the *anatomy* of the new formation to enable us to make use of them in this connection.

If cancer be an imperfectly developed new formation of connective tissue, disordered in its evolution and involution, as Wedl has taught, and as my own studies incline me to believe, and if the essence of the modification which occurs is to be looked for (see *Am. Journ., loc. cit.*) in the arrested development of many of the elementary forms, in their misshapen distension, and their premature fatty degeneration, it follows that in the study of transition forms special attention is to be devoted to these three categories, with the intention of observing how far the degree of anatomical disturbance corresponds to the peculiar clinical history of any given case.

In a word, the ultimate result of such investigations will be to decide whether innocence or malignancy are accidents which may occur in connection with any growth, so that *no* anatomical structure will insure that the

[1] In reply to several queries, it may be stated that I do not use the words "connective tissue" synonymously with white fibrous tissue, but apply them to *that mixture of white fibrous with yellow elastic tissue* which forms the bond of connection between the several tissues and organs of the body.

case will or will not pursue a cancerous history, or whether, on the other hand, each special peculiarity of history impresses upon the local formation a modified anatomy.

With these introductory remarks I proceed to the consideration of the case I have mentioned.

CASE. *Tumour of the left Breast in a Negro Girl* 15 *years old. History.*—The history of this patient, prior to the operation, was as follows: About a year before, while reaching over an ash-box, the pressure made upon one side of the box caused the opposite corner to fly up, giving her a violent blow upon the left breast. This was followed by a moderate degree of tenderness, swelling, and heat in the part. No treatment was employed, and the symptoms gradually subsided. After the swelling had quite left the breast, however, the patient noticed a small hard lump in the gland, which, though it made no prominence externally, could be distinctly recognized by the touch. The lump gradually increased in size until the time of the operation.

At that period the patient presented the following conditions: Her general health was good, she was plump, but not over fleshy, and menstruated regularly. The left breast formed a pendulous tumour, which was not adherent to the deeper parts, though closely attached to the skin. It was about twice the size of the opposite breast, was hard, smooth, very heavy, and evidently involved the whole mammary gland. There was no retraction of the nipple, which, however, was flattened down to the level of the areola.

The growth was the seat of uneasy sensations, compared by the patient to the pricking of pins, but there had never been severe pain in it at any time. At each menstrual period it enlarged, and subsided after the flow had ceased. The lymphatic glands of the neck and axilla were not affected.

As the tumour was rapidly increasing in size, Professor Smith determined to remove it, and it was accordingly amputated by him, February 2, 1859, at the surgical clinic of the University of Pennsylvania. The patient rapidly recovered from the effects of the operation, and was discharged February 14, the wound being then almost completely cicatrized. No tumours have occurred at any time, so far as known, in members of this family, the father, mother, and other relatives being healthy.

Remarks upon the History of this Case.—From the age of the patient, taken in connection with the seat of the growth, the probabilities *against* the cancerous character of the growth are very strong. Mr. Paget, whose ample opportunities entitle him to respect, says, in his work on surgical pathology (p. 512, Am. edit.), that scirrhous cancer of the breast "is extremely rare at any age under 25." While as to medullary cancer he remarks (p. 546): "The *breast* is among the parts which are most rarely the seats of medullary cancer."

Mr. Laurence, in the second edition of his work on the *Diagnosis of Surgical Cancer*, makes use of the following language; the Italics are his

own (Laurence, *loc. cit.*, p. 51): "Supposing we are told that a female has a *tumour in the breast*, and nothing more: from this we can infer nothing. Supposing, again, we are told a female with a tumour *somewhere* is aged fourteen: from this we can infer nothing. But let us be told that that 'somewhere' is in the *breast*, and we at once, *cæteris paribus*, derive a most important conclusion, viz., that the chances of the tumour being of a cancerous nature are infinitely small."

But although the *probabilities* are therefore against the cancerous nature of the growth, the *possibility* of this event cannot be overlooked. It must also be conceded that the uniform shape of the growth, the absence of pain, and the fact that it was not at all adherent to the pectoral muscle, although it had attained such marked size, are all against the idea of its cancerous nature. The only points suggestive of cancer, from the history and external characters of the growth, were its rapid increase in size, its great weight, and its extreme hardness.

Anatomy of the Growth.—The morbid mass, as handed to me, consisted of an elliptical portion of skin in the centre of which was the nipple, of the subcutaneous fat in which the gland was imbedded, and of the mammary gland. Each of these parts presented points of interest, which may be separately noted.

The *skin*, in other respects normal, was unusually adherent to the subcutaneous layer of adipose tissue. The nipple was flattened, and the milk-ducts passing from the nipple to the gland almost entirely obliterated.

The subcutaneous adipose layer was unusually thick for a virgin breast, a thickening not due to an increase of the proper adipose tissue, but to an excess of the connective tissue by which the groups of adipose cells are held together. To the unaided eye the adipose layer, therefore, presented an unusual appearance, being preternaturally firm, and instead of the ordinary yellow lobulated aspect of this structure, exhibiting a dense, pinkish-cream coloured texture. Thin sections, cut with the Valentin's knife, exhibited the following conditions: The groups of adipose cells were quite normal, and lay as usual in the areolæ of a framework of connective tissue. This connective tissue, however, was preternaturally abundant and for the most part fully developed, though some few spindle-shaped nucleated cells, evidently on the road to fibre production (embryonic connective tissue elements), were observed. Especially here would I remark upon the predominance of the well-developed *bundles* of white fibrous tissue, over the yellow elastic fibres on the one hand, and the spindle-shaped cells on the other. The doctrinal suggestions of this observation will hereafter be commented upon.

The mammary gland, however, is, in this case, the point of highest interest. Nearly twice its natural size, and of preternatural density and weight, it was yet nevertheless readily isolable both from the pectoral muscle below and from the superficial adipose tissue above and laterally. It was especially to be observed that whatever the pathological lesion might

be, *it affected the whole gland uniformly.* Its cut surface was homogeneous, and may be described as being less pink and more translucent than the normal gland tissue. The gland was succulent with a dense, transparent, albuminous fluid, which readily exuded on pressure; and when the cut surface was scraped with a knife, this fluid, rendered more or less turbid by the presence of blood-globules and of the anatomical elements of the growth, adhered to the blade.

The turbid bloody fluid thus obtained in no wise resembled, to the naked eye, the so-called cancer juice. The microscope revealed in it (besides blood-corpuscles) the following anatomical elements :—

(*a.*) Innumerable clear, oval, or rounded-oblong nuclei. These on the average were between $\frac{1}{2000}$ and $\frac{1}{3000}$ of an inch in their long diameter. Their contents were transparent and non-granular, and each contained one, or more rarely two, and in some instances even three, small, distinct, shining, spherical nucleoli. These nuclei only appeared as above described when viewed in the juice obtained by scraping, without reagents. The addition of water rendered them slightly granular, and acetic acid made their contours more distinct, at the same time causing them to become so granular as to obscure the nucleoli. After the most severe and impartial study, I am obliged to state that these bodies presented no points by which they could be distinguished from the nuclei of healthy developing connective tissue cells (such for example as those observable in the skin of an embryo of 4 months), except the fact that they were not contained in cells.

(*b.*) Accordingly, I was not surprised to observe also elongated, spindle-shaped cells, isomorphous with those of healthy, developing, connective tissue wherever found. These, however, were comparatively few in number; their nuclei were in all respects identical with those above described.

(*c.*) A few pale, oval cells, extremely delicate, with transparent contents, were also observed.

(*d.*) A few granules, and fat-globules (Aschersonian vesicles), were also observed, but they were not more abundant than in any normal tissue.

Numerous thin sections were cut with the Valentin's knife from various parts of the growth. These everywhere revealed essentially the following conditions :—

Groups of the nuclei above described, and occasionally groups of the cells described at (*c*), lay in the meshes of a stroma of well-developed connective tissue. This stroma was composed of exceedingly handsome, well-developed bundles of white fibrous tissue, with which but a moderate quantity of yellow elastic tissue was mixed. I should say, speaking approximatively, that the absolute quantity of well-developed, normal, white fibrous tissue was between twice and thrice that in a normal gland. The nuclei often lay in little groups of two or three, in the angles of the fibres, and sometimes in larger masses which could in nowise be interpreted as representing the gland-vesicles or the milk-ducts, while the cells never

lying in spaces so shaped that they might not represent modifications of these normal textures, were sometimes also contained in cavities the shape and relations of which imperatively required them to be regarded as the terminal cæca of the lactiferous tubes.

The thin sections were beautifully transparent, and nowhere presented any evidences of fatty degeneration.

Remarks upon the Anatomy of this Growth.—The above details present the prominent points of the anatomy of this tumour. I have presented them nakedly that others may use them as they please, and may separate at a glance the data observed, from the deductions based upon them. From these data, however, the following remarks would appear legitimately to spring:—

1. With regard to the excessive quantity of well-developed white fibrous tissue observable both in this mammary gland and in the subcutaneous adipose tissue removed with it, it is especially to be observed that it is not only wholly disproportioned in quantity, as compared with a normal gland, but it is also excessive as compared with the quantity of yellow elastic tissue found. And here also the extremely small number of healthy spindle-shaped cells (young connective tissue elements) observed both in the superficial adipose tissue, and in the gland itself, becomes significant.

Before, however, proceeding in the attempt to interpret these phenomena, it will be necessary to make a remark on the normal process of growth in the connective tissue of the economy.

Up to a certain period in the development of the embryo, the increase in quantity of the connective tissue elements probably depends almost exclusively on the development of new cells, and their gradual transformation into white fibrous bundles, and yellow elastic fibres. An increase due to the same conditions probably continues to birth, and also, but with gradual diminution in amount, until a period in childhood which has not been definitely determined. (See Kölliker, Am. ed., p. 135.) But the increase is also due, from an early period in embryonic life, to an actual *growth* in length and thickness of the individual white fibrous bundles, and yellow elastic fibres, which become simultaneously both longer and thicker. In the case of the white fibrous bundles the number of fibrilli is apparently increased as the bundle increases in thickness, an increase due to the direct appropriation by the growing bundle of the requisite material from that nutritive plasma, in which, during healthy life all the tissues are bathed. To this process is due all the increase of the connective tissue of the economy which occurs from the cessation of the development of fibre-cells in childhood to fully developed adult life.

It must also be admitted that this growth in the size of the individual elements may go on in any part chiefly in the white fibrous bundles, or, on the other hand, chiefly in the yellow elastic fibres, otherwise how shall we account for the fact that in some parts (*e. g.*, the ligamentum nuchæ) the

elastic fibres are not only more numerous, which can be accounted for by supposing a greater proportional number of formative cells, but also the individual fibres of much greater diameter than those of some other parts of the body.

The above remarks will at once make intelligible what many pathological observations have rendered probable to me, that there are two kinds of *pathological increase in the quantity of connective tissue in any part.* One, which is best described as *a new formation of connective tissue,* which imitates most closely the embryonic process; cells arising in obedience to the law of free cell-development, which are transformed, some into white fibrous bundles, some into yellow elastic fibres. This is the process which goes on in the formation of cicatrices, and in the development of inflammatory exudations generally, as well as in many new formations the initial step of which cannot be positively affirmed to be circulatory disturbance. The second process will be best described as *true hypertrophy* of the connective tissue of a part, and especially of its white fibrous element. As the former process repeats essentially the normal *embryonic development* of connective tissue, so this repeats essentially the ordinary process of *growth* which has been shown to obtain to a great extent in infancy, and almost exclusively in the increase of this tissue during the time of transition between childhood and adult life. In *true hypertrophy,* in the sense in which I here use the term, no embryonic elements are formed, the bundles grow in thickness, and perhaps also in length, by the direct assimilation to themselves of material from the blood.

At the same time, however, that I discriminate between these two separate processes, I am free to admit that in very many instances they occur simultaneously, and that there is at once a *true hypertrophy* and a *new formation* of connective tissue. This appears to have been the case in the tumour under consideration. Without wishing to be dogmatic on the point, the absence of transition forms between the spindle-shaped cells, and the firm, old, fully-developed bundles, inclines me to believe that the mass of increase in the connective tissue both of the gland and the fat about it is due to such a growth in the normal white fibrous bundles of the part as would come under the head of what I have defined as true hypertrophy. At the same time, with the hypertrophy, the existence of the embryonic elements demonstrates a certain amount of new formation.

2. The consideration of the *free nuclei* above described is not without its difficulties, but the fact that they are isomorphous with the nuclei of the spindle-shaped cells, also observed, would seem to point out a genetic relation between the two. I regard them as the nuclei of formative connective tissue cells, the development of which has not yet proceeded so far as to the formation of a cell-wall.

3. More difficult is the interpretation of the cells described above at (*c*). Upon the whole, I regard them as probably the epithelial cells of the smaller milk-ducts, and of their terminal follicles.

It would appear, then, from the considerations adduced, that at least two distinct and simultaneous processes existed in the breast under study. 1. True hypertrophy of the connective tissue of the parts in the manner above laid down. 2. A new formation of connective tissue, which was represented in the case of the superficial adipose layer by a few spindle-shaped cells, in the case of the gland by similar cells and the abundant formation of nuclei. This point in the investigation attained, the question at once arises, Is this an *innocent* or *malignant* new formation of connective tissue? Have we here any evidences that the new formation is not pursuing the usual history of connective tissue development? And if so, is the disorder and distortion of the process such as to justify the designation *cancerous?*

Our attempt to answer this question must be based exclusively upon the anatomical data afforded by the study of the new-formed elements. The coexistence of what I have called *true hypertrophy* of the connective tissue involved, however interesting it may be in a general point of view, is of no value in diagnosis. Such a hypertrophy exists not only in benignant conditions, *but also in cancer.* The careful study of a series of hard cancers of the breast will convince any unprejudiced observer of this fact.

In the study of the new-formed elements, then, confining ourselves to them alone, we cannot fail to note the great numerical predominance of nuclei over more highly developed forms; and, when the age of the disease is taken into consideration, it will hardly be disputed that we are to regard their development as preternaturally retarded. If we compare the innumerable free nuclei with the few spindle-shaped cells, we recognize, to a certain extent at least, the same inequality in the degree of development of the several elements which is to be observed in cancer. But while this is admitted, it must also be confessed that we fail to recognize the misshapen distension and the fatty degeneration of cancerous elements.

Our position, then, in regard to this breast, may be thus stated: the cancerous modification of pathological connective tissue growths lies essentially in three categories—the disorder in the degree of development, the misshapen distension, and the fatty degeneration of the elements. In the growth in question the disordered development occurs, but there is no misshapen distension, no fatty degeneration. Can we, in the absence of the latter two conditions, affirm the growth to be cancer? In the present state of my knowledge on this subject, I would answer, certainly not. Yet, as the disordered development is a deviation in the cancerous direction, I cannot positively pronounce the growth to be innocent; for the possibility must not be overlooked, that, if extirpation had not interfered with the progress of the affection, distension and degeneration of the elements might have ultimately occurred.

I therefore place the case upon record as belonging to the category of transition forms, and shall watch with interest its clinical history.

II. *Anatomy of a Case of Retrograde Cancer.*—The purpose of this article is briefly to record certain anatomical peculiarities in a scirrhous tumour of the breast, which was removed at the surgical clinic of the University of Pennsylvania, and which I was permitted to study, through the politeness of Professor Henry H. Smith. To some it may appear a useless task to add to the details we already possess connected with the minute anatomy of malignant tumours; but, properly regarded, each additional fact is a step, not towards complexity, but towards simplicity, and the scientifically directed attempt to account for the protean forms which new formations assume must ultimately lead to the determination of those guiding laws which lie at the bottom of them all.

History of the Case.—An old Irish woman, more than sixty years of age, presented a tumour, rather larger than a hen's egg, in the left breast. The tumour was ulcerated at several points, from which florid granulations sprouted. The skin surrounding these points was livid, and firmly adherent to the subjacent growth; the nipple was retracted, but the morbid structures were not adherent to the pectoral muscle, nor was there any enlargement of the cervical or axillary lymphatic glands. This tumour had been growing between six and seven years; it first ulcerated about two years ago. During the whole period of its growth it has been the seat of paroxysms of severe lancinating pains.

The bodily health of the patient is good, but her friends state her to be subject to occasional periods of mental aberration.

The opposite breast is in the usual atrophied condition of the patient's time of life.

The breast was removed at the Clinic of the University of Pennsylvania, Saturday, February 5, 1859.

Remarks upon the History of this Case.—The general appearance and history of this case were probably quite sufficient for a diagnosis without any inquiry into the minute anatomy of the tumour.

Anatomy of the Tumour.—The morbid mass, as it came into my hands for study, consisted of an elliptical piece of skin, in which the nipple occupied one focus of the ellipse, of the whole mammary gland, and of some of the surrounding adipose tissue. The nipple was retracted, the adipose layer *over* the tumour had disappeared, and the diseased gland came into immediate contact with the thin, atrophied, livid skin, to which it was adherent. There were several ulcerated points on the projecting portion of the mass from which scanty vascular granulations protruded.

The tumour being laid open from behind by a longitudinal incision was found to present an irregular central cavity two-thirds filled with a yellowish, friable, solid mass, which was adherent at several points to the walls of the cavity.

All of the cavity not occupied by the central mass was filled with a yellowish creamy fluid in quantity rather more than half a fluidounce, which,

to the naked eye, much resembled pus. The odour of this fluid as well as of the friable mass was exceedingly offensive.

The cavity was bounded by a wall or crust one-half to three-fourths of an inch in diameter, which represented the remainder of the diseased gland, and ran out irregularly into the surrounding textures. Sections of this portion of the growth presented, on the surface, a pinkish cream colour, over which radiated yellow reticulated lines.

Each of the above portions were separately studied.

The purulent looking fluid contained in the central cavity presented the following elements:—

(*a.*) Innumerable granules, the majority of which were Aschersonian vesicles of the most variable sizes.

(*b.*) A few angular and granular *nuclei* which were utterly indistinguishable from those of softened *tubercle.*

(*c.*) Cholesterin plates.

No bodies resembling pus-corpuscles were observed.

The elements of the central friable mass were identical with these, but aggregated together so as to form a tolerably solid substance.

The outer crust or shell yielded on scraping a moderately opaque cream-like "cancer-juice," in which the following elements were observed:—

(*d.*) *Nuclei;* large, clear, oval or rounded oblong, with single, double, or triple, large, shining vesicular nucleoli. These nuclei had transparent or dimly granular contents, and varied in length from $\frac{1}{1200}$ to $\frac{1}{3500}$ of an inch. The great size and irregular shape of many of the nucleoli was noteworthy. The nuclei became more distinct and their contents granular on the addition of water, and yet more so on the addition of acetic acid. Besides the nuclei above described there were also a few similar nuclei in which a certain amount of fatty degeneration had occurred, as evidenced by the presence of numerous Aschersonian vesicles.

(*e.*) *A few spindle-shaped cells*, the ordinary normal young connective tissue elements.

(*f.*) A small number (that is, few as compared with the number of the nuclei) of *distorted* young connective tissue elements, huge misshapen cells of the most varied shape with one, two or more nuclei, identical in appearance with the nuclei above described. These answered well to the characters of the so called "cancer cells."

(*g.*) A few of those aggregated heaps of granules, usually known as compound granular cells.

(*h.*) Innumerable granules, chiefly Aschersonian vesicles.

Numerous thin sections of this part of the morbid mass were cut with the Valentin's knife, and the usual arrangement of similar growths was found to obtain. The elementary forms above described were aggregated together into larger or smaller groups, and lay in the meshes of a connective tissue stroma, which doubtless represented the primitive connective tissue of

the gland, *plus* a certain increase due to true hypertrophy in the sense laid down in the first of these papers (vide *supra*), as well as to new formation.

The numerical relations of the cells to the nuclei, as thus studied, was not so disproportionately small as might have been supposed from the study of the juice; yet a vast predominance in the number of the nuclei was still observable.

Perhaps the most noteworthy fact in such sections, when studied with a high power, was the degree of fatty degeneration, as evidenced by the innumerable Aschersonian vesicles, not only in the cells and nuclei, but also in the fibrous stroma of the growth. In some points, indeed, the degree of fatty metamorphosis was such as to render the preparation too opaque to be satisfactorily studied.

When such sections were studied with lower powers, it was observed that these opaque parts were distributed with a certain degree of order, appearing with a two inch object-glass as a brownish-black (or with reflected light as a yellowish-white) branching network, which was to a certain imperfect degree recognizable to the naked eye. It is, as is well known, to such a branching network that the designation *reticulum* is given; and Johannes Müller, who did not know that this yellow reticulum was only the optical expression of the excessive fatty degeneration of certain parts, made a special variety of cancerous growths, "reticular cancer," grouping under this head all cancers which exhibited a reticulum.

The *granulations* above described, as sprouting from various points of the tumour, presented the following conditions: In the more superficial projections the only elements observed were innumerable nuclei identical with those described above at (*d*). These were piled irregularly together, no definite order being observed.

In the deeper parts, as shown by thin sections, similar nuclei lay in groups in the meshes of a vascular stroma, composed chiefly of ordinary young connective tissue cells.

Remarks upon the Anatomy of this Growth.—This tumour would appear to belong to the category of retrograde cancer. I interpret the purulent fluid occupying the central cavity as *softened cancer.* It is not pus in any proper sense of the word. I regard it as the ultimate result of that destructive fatty degeneration, which, in one degree or another, is characteristic of cancer. The offensive odour of this fluid is simply the expression of the putrefactive processes commenced in a substance whose vitality is lost.

The central friable mass is simply to be regarded as a slough. It exhibits well the condition of a cancer to which certain continental writers have applied the term *necrosis*, a term usually limited in this country and England to gangrene as it occurs in bones.

Some of the earlier writers supposed that the softening and breaking down of a cancerous tumour invariably began in the central portions, as

in this case. The analogy of phlegmons and of softening tubercle was in favour of this idea, which, however, has been disproved by numerous observations which have shown that the softening and sloughing of the cancerous growth may begin in *any part of its substance.*

Various theories have been propounded to account for the tendency of cancerous growths to ulcerate and slough. The idea, often promulgated, that the part of the cancerous mass farthest removed from the vital influence of healthy tissues, generally sloughs, and that on account of its distance from them, is met by the broad fact that whereas some cancerous tumours ulcerate and slough before they have attained the size of a hen's egg, others acquire a prodigious bulk before they undergo this change. For myself the anatomical conditions of this case, and of every other sloughing or ulcerating cancer it has been my fortune to study, compel me to conclude that the ulceration and sloughing of these growths is the inevitable result of an exquisite degree of that fatty degeneration which is characteristic of cancer. The unknown momentum which determines the lesser degree of this process determines also the higher.

The study of this case, moreover, suggests an anatomical reason for the fact that cancerous ulcers do not generally cicatrize. It is simply due to the circumstance (which I have noticed also in several other cancerous sores) that the granulations sprouting up in such a sore, whatever their external aspect, are in fact not ordinary granulations but *cancer substance,* and they therefore pursue the history of cancer, instead of undergoing those transformations which occur in healthy granulations.

At the same time, the fact must not be forgotten that the cancerous ulcer *does sometimes,* though rarely, cicatrize. I have never had the opportunity of observing, and I know of no recorded observations which show, whether in such a case the alteration in the usual history is due to any prior change in the anatomy of the granulations.

Art. II.—*Case of Carbuncular Inflammation of the Lip; with some remarks on this anomalous form of inflammation.* By Frederic D. Lente, M. D., Surgeon to "West Point Foundry."

Feb. 19*th,* 1858. Lieut. M., U. S. Navy, a stout healthy looking man, about 40, had been rather "under the weather" for two weeks, with gastric derangement, &c. He had left the South a few days ago, just before a sudden cold change in the weather, and, in coming up Chesapeake Bay, exposed himself on deck at night. He was soon after attacked with shiverings, and pains in his muscles and bones, with headache. These symptoms he still complains of. Some days ago, he noticed a small pimple

on the left side of the upper lip, which he subsequently scratched, and thus formed into an open sore. Two days ago, the lip commenced swelling, and became painful; the pain and tumefaction had both increased rapidly, causing now entire loss of sleep. The lip, on the left side, is swollen, hard, livid, and everted to some extent, and just at the junction of the vermilion border with the skin, is a small ulcerated spot. The tongue is coated with a whitish fur, pulse rapid, and rather feeble, breath offensive, bowels costive. Ordered pulv. Seidlitz every four hours, and to apply an emollient poultice to the part.

20*th*. The pain became so severe last night as to render a large anodyne necessary. Has had no sleep. The tumefaction has increased and extended beyond the median line to the right, and also upwards towards the eye. Made a deep incision through the ulcer. No discharge of pus, but the cut tissues could be seen infiltrated with points of thick curdy matter. Bowels have not moved. Ordered ℞.—Hydr. sub. mur. gr. x; pulv. jalapæ gr. xij.—M. Continue the powders.

21*st*. Pain somewhat less severe. Bowels have been moved once; tumefaction full as great, edges of incision gaping widely, and slightly everted. Continue poultice.

22*d*. Last night was summoned, and found the patient suffering excessive pain; tumefaction and constitutional disturbance increasing. The vermilion border, on both sides of the incision, is more swollen and hard; small points of matter can be seen at various points through the livid epithelium. A longitudinal incision through the vermilion border, on either side of the first, was accordingly made, giving vent to a little thick pus; a large anodyne was administered, and the poultice continued.

23*d*. Pain somewhat relieved; slept three or four hours; tumefaction the same; edges of first incision gape more widely, and still everted. Pulse feeble. Directed him to anoint the lip freely with the following ointment, and to keep the same constantly applied on lint covered with oiled silk. ℞.—Pulv. opii ʒj; glycerin ʒiij; ung. hydrarg. ℥j, M., and to take one of the following pills every three hours: ℞.—Pil. hydr., quin. sulphat., ext. conii mac., āā gr. xij; ferri sulph. exsic. gr. vj.—M. Divid. in pil. No. xij. A Seidlitz powder occasionally.

24*th*. Better. The lip is softer, and a little less tumefied; the edges of the first incision have softened and fallen in, and there is some discharge of pus from the wound. The lip is almost free from pain. But the inflammation towards the eye has rather increased; appetite fair, but can only take soft food.

26*th*. Has been gradually improving; the softening going on; matter is oozing through several small points near the edges of the incision. An abscess, which has been forming between the first incision and the eye, has now burst into the former, and discharges a dark, unhealthy pus. The trouble is now evidently over. Convalescence was rapid.

At the time of the occurrence of the above case, it had escaped my recollection that I had ever seen any account of a similar affection. I therefore termed it *carbuncular* inflammation, as conveying a more accurate idea of the peculiar pathological condition of the part than any other; but still it is not altogether a satisfactory term, as there is no sloughing, and it does not convey any idea of the malignancy and fatality which often attend this affection. I find that, for four or five years past, a disease very similar to this, if not absolutely identical, has been noticed by two or three physicians in different parts of this country. In a report of the proceedings of the New York Medical and Surgical Society, published in the January number (1859) of the *New York Journal of Medicine* (p. 18), Dr. John Watson notices a case of this disease under the name of *virulent pustule,* which bears so close a resemblance to the one just described, that I take the liberty of transcribing his brief account.

The patient was an Englishwoman, who had noticed "a small and slightly painful pimple upon the upper lip, which she had pricked with a needle. This seemed to aggravate it, and when the part was somewhat swollen, she had gone to a neighbouring apothecary, who, with a lancet, had scarified the mucous membrane of the lip. When Dr. Watson saw her, there was considerable tumefaction about the part, extending to the angle of the lip, and upwards towards the eye, for two or three inches. The mucous membrane was swollen, corrugated, and nodulated, the pulse 120 to the minute, the tongue coated, and the swollen part quite painful. He now thought of making a free incision into it; but, as it appeared to him that no sufficient indication existed for so doing, he ordered an evaporating lotion for the cheek, and gave internally spirits mindereri, and solution of morphia.

"This treatment was kept up for two days, with the result of producing diaphoresis, and some sleep. At the end of forty-eight hours, during which little change had occurred in the inflamed spot, he found, on everting the lip, points like detached pustules, similar to those showing themselves in anthrax, when about to break. He now made a free incision into the lip, and gave exit to blood and matter. The next day the pulse was higher, but she was evidently better, and, in a week, convalescence was fully established. The incision was undoubtedly of great benefit to her.

"The disease, he remarked, has not as yet been fully and accurately described; resembling anthrax in many respects, and the real pustule maligne in others, it differs in many particulars from both, and should not be confounded with them in description. The disease generally appears about the face near the mouth, and is often fatal, and had been so in three cases which he had seen.

"Dr. Parker agreed with Dr. Watson as to the essential difference between this species of virulent pustule, and the pustule maligne of the French writers. He had lost a case very similar to that mentioned by Dr. Watson."

"Prof. Post regarded the disease as very similar to carbuncle. Prof. Parker remarked that anthrax presupposes always more or less sloughing of the surrounding tissues; this disease is not accompanied by it, and resembles mere furuncle."

"Dr. Van Buren had seen a case in a young married lady, which, commencing in a small pimple on the lower lip, ran a very rapid course, and

destroyed life in forty-eight hours after he saw her. Dr. Buck mentions four cases of this affection, of which three died."

In the 12th vol. of the *New York Journal of Medicine*, 1854, page 368, Prof. Parker has an article on "*A Peculiar Form of Malignant Inflammation of the Lips and Face, resembling Malignant Pustule*," including an account of four cases, three of which proved fatal within a very short time; in the case that recovered, portions of the lip sloughed. Prof. P. remarks that, in all the cases which had fallen under his notice, the disease has been seated on the *lower* lip. "The disease," he goes on to remark, "would seem to be peculiar, having many points of resemblance to other similar affections, but still, not so closely allied to any one as to warrant its classification under the same head." Dr. Parker also notices an article by Dr. Peirson, of Salem, Mass., who reported several cases in the *Boston Med. and Surg. Journ.*, 1852, which he considered malignant pustule. "With but one or two exceptions, the disease appeared in curriers, and hence Dr. Peirson attributes the disease to inoculation by dead animal matter." The disease, as a very general rule, attacked the lips. In vol. 13 *New York Journ. of Med.*, 1854, Dr. Ayers, of East Creek, N. Y., notices two cases of this disease, both occurring on the lower lip. The treatment was incision, tonics, stimulants, and *mercurial*. Both cases recovered, although the symptoms were very alarming.

Two diseases very closely resembling this affection are very prevalent in France, and very fatal, though very rare in this country, Pustule maligne, and charbon malin, and it is probable that all three originate in the same manner. Carbuncular inflammation of the lip seems to bear a closer affinity to the former than the latter. Thus malignant pustule, as described by the authors of the *Compendium de Chirurg. Pratique*, like this disease, marches from without to within, while malignant carbuncle first manifests itself in constitutional disturbance. P. maligne only attacks those parts of the body habitually uncovered, C. malin all parts. P. maligne commences by a small, and apparently unimportant point of redness and swelling; the C. malin by a swelling of considerable size, and less circumscribed. Lastly, the pustule, according to the best French writers, always results from external action, and, often, passing through two or more stages, before any disorder of the general health is observed. But, after all, these two diseases, though differing so widely in many respects, are in their essence identical. This appears to be the conclusion of those writers by whom they have been most closely observed. That is, they both result from an animal poison introduced by some means into the system; in the case of malignant pustule, through the skin; in the case of malignant carbuncle, through the air or alimentary passages.

In the *Compendium de Chirurg. Pratique*, art. "Maladies Charbonneuses," is an account of an epidemic of malignant pustule, occurring in

the department of the Basses-Alpes, which Bayle, who describes it, considers of spontaneous origin, developed without contagion. But it appears that, simultaneously with this epidemic, there was also one among animals in the same localities, by which great numbers were perishing. This, coupled with the fact of the extremely contagious nature of the epizootic, its facility of admission into the human system by so many inlets, might well lead the authors of the compendium to characterize the alleged spontaneity of the epidemic as *une chose très incertaine.*

Considering the fact then established for the present that the charbon malin, or the pustule maligne, always results from an animal poison introduced from without, we inquire, What is the origin of this carbuncular inflammation or virulent pustule, or by whatever name it may be designated? It is certainly very nearly allied to these diseases, not differing more from one of them than they do from each other, although, as we have seen, identical. The individuals attacked are, as a general rule, not in any peculiar pathological condition as to their general health. It would seem then probable that this disease, like pustule maligne, is a local affection, but whether produced by the same or some analogous cause, it is impossible at present to determine; such appears, however, not improbable when we consider the extreme facility with which these diseases are spread by contagion. Thus, Bidault de Villiers states that a number of persons contracted malignant pustule by merely handling the dead body of an over-driven ox (un bœuf surmené). Fournier states that those engaged in washing and carding the wool of infected animals contracted the disease; also in tanning the skins. In Bourgogne, in France, where the disease is unusually prevalent and fatal, the peasantry often line their sabots with sheep-skin, and thus often contract pustules. Nor is it necessary that there should be any abrasion or wound of the cuticle; for it is stated that the shepherds, who carry their sick sheep on their shoulders, have pustules on the back of the neck; and Chaussier reports the case of a shepherd who, having carried the bleeding body of a dead sheep on his shoulders, and the blood having penetrated his clothes and rubbed against his loins, had a pustule maligne at this point. Nor is it necessary even that contact should take place between the diseased body of an animal and the human body; Fournier, Chaussier, and others, relate instances of the production of these pustular and carbuncular diseases through the respiratory and digestive passages.[1] They may also be produced by the bite of an insect. It is related that Maret, a celebrated physician of Dijon, believed that a particular insect had the power of producing pustule without having been previously contaminated by contact with a diseased animal. But he appears to be alone in this belief, and the general opinion seems to be that it always results from an insect "*qui s'est nourri de débris d'animaux, ou qui a sucé les liquides*

[1] Compend. de Chirurg. Pratique, p. 279.

fournis par des plaies charbonneuses." "All species of flies, but especially the carnivorous wasp (quêpe carnassière), are the agents by which the virus is most frequently conveyed." Thomassin has observed that even the bee is capable of conveying the contagious principle. With these facts before us, and calling to mind the numerous instances of the sale of diseased meat which are made public from time to time, and the still more numerous instances which never come to light, and also the many diseased living animals that are constantly to be found in our larger cities, it is not difficult to believe in the contagious nature of this peculiar disease, which is becoming so prevalent of late in this country. The fact that so many partake equally of the diseased meat, while so few contract the disease, does not militate against this supposition; for we well know that, among the great number of dissection-wounds received yearly by the hundreds of medical students, and by physicians engaged in making post-mortem investigations, very few result in serious trouble. It is stated by de Villiers that of two or three hundred who ate of diseased meat, only fifteen were affected with pustule maligne. Many of those affected with true malignant pustule in France were positive that they had neither eaten of diseased meat nor been exposed to contagion. There is but little doubt that Dr. Peirson's cases, referred to in Dr. Parker's article, originated from contagion, as they nearly all occurred among curriers.

The increase of this disease in the northern part of this country, the little attention it has received in the medical periodicals, the great obscurity which envelops its history, the ill success of treatment, and its consequent fatality, we hope will excuse the length of this article, which, although it throws little additional light on the subject, yet may attract the attention of other observers who may have more extended opportunities for its study. A few remarks on the treatment of carbuncular inflammation will conclude this paper.

Lieut. M.'s case differs in two particulars from most of those heretofore published. The severe local symptoms were preceded by marked constitutional disturbance, thus resembling more the development of the charbon malin than the pustule maligne; the treatment, also, which seemed to afford any decided relief was also peculiar—the *mercurial*, externally and internally, in combination with anodyne and tonic. Allusion has already been made in this paper to two severe cases treated successfully by Dr. Ayres by mercurial, combined with tonic and stimulant treatment. He only used the mercurial internally. We have only met with these three instances in which any form of mercurial has been tried. Deep incisions are dwelt upon by most of those who have reported on the subject as a *sine qua non* in the treatment, and the appearance of the local affection is certainly suggestive of this procedure; but, in very few instances, have they given that prompt and decided relief which they always do in other similar diseases, with one exception—malignant pustule; in this disease, French

surgeons have found *deep* incisions very dangerous, and many have discarded them entirely. May they not also be prejudicial in this disease? We are certainly in need of some more prompt and certain remedy than this has heretofore proved to be. The actual cautery has been much employed by some French surgeons of eminence, and even in some apparently desperate cases has proved successful when used freely and fearlessly. It would be a very serious case that would warrant the application of this agent to the seat of this peculiar inflammation, which is almost invariably on the lips; but when the mortality is from fifty to seventy-five per cent., its application is certainly not unwarrantable, whatever the resulting deformity. In all cases, whatever other treatment may be employed, tonics, nourishment, and stimulants, according to the urgency of the typhoid symptoms, which are nearly always present, will be imperatively demanded.

COLD SPRING, February 26, 1859.

ART. III.—*Cases Illustrative of Criminal Abortion.* By HORATIO R. STORER, M. D., of Boston. (Read before the Boston Society for Medical Observation, Feb. 7, 1859.)

CASE I. Mrs. H—, of Roxbury, applied to me for treatment, on the 14th of October last. Patient, an American by birth, though the wife of a German, is a well-formed, healthy looking woman, some twenty years of age, and was then five and a half months gone with her first child. Her general health had been good, and till the present she had never suffered from any form of neuralgic pain.

She reported excessive toothache, of nearly two months' standing; that it commenced on the left of the lower maxilla, but then affected both sides of both jaws; that during the whole period she had been under the charge of a physician, and had been thoroughly and actively treated, by anodynes, local and general, by antispasmodics, purgatives, fomentations, counter-irritants; that a tooth, apparently the only carious one she had, had been extracted ten days previously, and that it had been proposed to remove others, to which, however, she would not consent—all without the slightest relief.

She alleged, and showed, loss of sleep and of appetite, great general prostration, excessive despondency of mind. After the extraction of the tooth, abortion had threatened—and she now begged that it might be brought on; declaring, if refused, that she would induce it upon herself, rather than endure further pain.

She was ordered a fragment of pellitory root, pyrethrum, as a direct

gingival stimulant, though horseradish would probably have answered the purpose, and on the second day presented herself cured. There has been no return of the malady, save a slight attack on January 19th, which was readily relieved by the same treatment. Patient was confined on February 3d; and is doing well.

I report this case for two reasons. In the first place, as an instance of the frequent success of simple and apparently trivial remedies after severe ones have failed. The affection seems to have been entirely neuralgic in its character, reflex, the result of the uterine irritation. All other causes mentioned by writers as liable to produce it were absent; there was no local inflammation, no general catarrhal affection; the disorder did not commence at, and apparently was in no way dependent upon, the carious tooth, at least it was not relieved by its removal, nor by the local blood-letting then occasioned.

I am aware that sialagogues, according to Gardien, and he is apparently indorsed by Churchill, are supposed indicated only in those instances where the toothache is in consequence of a general catarrhal affection, which did not here exist; but on the other hand their use would seem, on the simplest theoretical grounds, among the first procedures that would occur to the mind.

The second of the reasons referred to is the following: that I may express my strong disapproval of the practice, still extensively obtaining among physicians and dentists, of subjecting patients to the risk of miscarriage, which must be confessed excessive, by the extraction of teeth during pregnancy. This procedure should in no instance be resorted to till every other measure which affords any prospect of relief has been faithfully employed. In the history reported, it is seen that such was not the case.

Extraction has been recommended by authorities who are respected, by Campbell, Gardien, Capuron, and others, on the supposition that there is a greater likelihood of abortion from the continued pain; but against this argument I place the facts that after resisting many remedies, the pain often disappears spontaneously—as is indeed allowed by one of the writers instanced, Capuron—and that in more plausible measures, tried and untried, readily occurring upon reflection to all who do not blindly follow the books, there is, I think, a greater probability of success. Anæsthesia, local and general, have both been found to avail. I would suggest, as worthy attention, a modification of the process of subcutaneous injection, proposed by Alexander Wood, of Edinburgh, and so successfully employed; merely here introducing the opiate beneath the mucous membrane, which I am not aware to have yet been done.[1] Should this also fail, a direct topical appli-

[1] After this paper was in type, I find, by the Edinburgh Medical Journal for Nov. 1858, p 424, that the above suggestion has been partially anticipated; a dentist,

cation, either of a local anæsthetic or a gentle stimulant, might be made to the cervix uteri; but as this latter procedure, though at times successful in the obstinate vomiting of pregnancy, cannot be used with too much caution during gestation, it would, therefore, be seldom justifiable; and only in the cases where the extraction of teeth in pregnant women can ever be defended, those, namely, where abortion is actually threatening and apparently at hand.

It might be asked how the case now reported can fairly and with justice be considered as in any way illustrative of the subject of criminal abortion, because, though menaced and solicited, this did not actually take place. It is an acknowledged principle of jurisprudence that a person must be presumed to intend all the natural, probable, and usual consequences of his own act.[1] Whenever, by any operative or other procedure, a physician directly produces abortion, unintentional though this may be, if in the absence of any precaution that might have been taken, he must be considered, to the extent evidenced by the history of the patient, responsible therefor; and the class of cases to which that now reported belongs, is accordingly open to as legitimate a question of obstetric morality and of criminal responsibility as that other series of late so ably discussed by Dr. Churchill, of Dublin.[2]

CASE II. I was called to attend Mrs. S—, of Plympton Court, on the afternoon of January 2d, 1859, and found her flowing profusely, this having continued for many hours; the attack at first being considered menorrhagic, as she was still suckling, till an abundant escape of coagula had rendered probable its true character.

Upon examination, the uterus proved somewhat enlarged and slightly retroverted, the os, with difficulty reached, sufficiently patulous to admit the tip of a single finger, but its outline laterally and symmetrically fissured, so that the cervix presented to the touch an apparently deep and incised transverse wound completely through its substance, with irregular edges and indentations simulating artificial punctures. These irregularities and depressions were not like those presented by malignant disease; there was no fetor or other usual or plausible symptom of such, save the hemorrhage. Within the os a mass was felt presenting, which, from the absence of other signs of polypus, the sudden and profuse sanguineous discharge, and the faint uterine contractions that from time to time occurred, conjoined as these were with a contained body of hardly sufficient size, if a polypus, to have

Dr. Smith, having had recourse to the measure, for the painless extraction of teeth. The experiment does not yet seem to have been tried, however, as here proposed, with a view to prevent that operation.

[1] DAVIS, Crim. Justice, 483.

[2] Dublin Quarterly Journal of Med. Science, August and November, 1858.

itself excited them, I did not hesitate to pronounce a partially detached ovum.

My first impression from the physical examination was of course that the abortion must have been owing to direct instrumental or other violence, which, however, the patient persistingly denied. Upon learning that she was of the Catholic faith, these suspicions were somewhat allayed; for reasons I shall elsewhere fully set forth, and which are borne out by statistics so far as existing, and by general experience.

I was the further convinced of my error upon hearing the past history of the case. The patient, as already remarked, was nursing—sixteen months having elapsed since her last confinement. The catamenia had not returned, and the only possible cause she had had for supposing she might be pregnant, beyond the ordinary exposure of married women, was an apparent slight decrease in the amount of milk secreted; of the existence of this decrease, however, she was by no means certain, and had attributed to it no importance. She had strained herself within a day or two, by reaching to a shelf.

Mrs. S— had been confined four times; in every instance labour being exceedingly tedious, and delivery accomplished by instrumental aid. Though the children were all born living—one, however, the first, dying shortly after birth—their extraction was effected with much difficulty, and the application of considerable force. I could not doubt, therefore, that laceration of the cervix had occurred at one or more of the times referred to, sufficient to account for all the appearances that were now observed; an accident by no means incompatible with the exercise on those occasions of all needful caution and skill, for it is notorious that lesions, to a greater or less extent, of the margin of the os are extremely common, even in natural labours.

In the treatment of the present case there was nothing unusual. It seemed impossible to check the abortive process, and there being little danger from confined internal hemorrhage in early miscarriage, when the uterine cavity is but slightly enlarged or dilatable, the vagina was plugged; strips of cotton being used, inserted separately and tightly packed, like the foil in the process of tooth-filling. Upon their removal, three hours after, the os was found widely patulous, proper forceps were introduced, and an embryo, some six weeks advanced, was removed; upon which the hemorrhage at once entirely ceased. The patient has made a good recovery.

January 28th, four weeks subsequent to the miscarriage, the physical signs presented by the uterus were still persistent, as described above.

Upon reviewing this case, I think it important to dwell upon the diagnostic peculiarities it presents, unadverted to, so far as I am aware, by any obstetric writer. We are all familiar with the various permanently disastrous results at times following instrumental or otherwise difficult labour,

the adhesions, bands, and fistulæ that not unfrequently come under medical observation; but to find such, in the absence of all signs of concomitant or consequent malignant disease, and accompanied by profuse hemorrhage —their edges, the cicatrices themselves, and the depressions between them, obscured and filled by coagula, and at the same time, and in the midst of these physical anomalies, the presentation of an aborting ovum—would raise, I may surely say, in almost every mind the suspicion of foul and criminal interference. Were death to occur under such circumstances, the result at an inquest could hardly be doubted, unless unusual care were observed at the autopsy to remove by ablution all clots obscuring the age of the existing lesions; a precaution that in most instances would hardly be observed, for fear of disturbing any attachments of the ovum—so often in these cases preserved for the cabinet—that might still obtain.

It is my duty to lay the more stress upon this case and its several suggestions, lest at other times, and in another connection,[1] any remarks of my own might seem unjust. But, on the other hand, I am thus strengthened in my belief that many of the medico-legal relations of criminal abortion are as yet uninvestigated or unfound, and that with every step towards their elucidation an advance is made towards the ultimate suppression of the crime.

ART. IV.—*Two cases of Encysted Tumour of the Ovary successfully treated by excision, and an attempt to justify the operation of Ovariotomy under certain circumstances.* Read before the College of Physicians and Surgeons, of Louisville. By HENRY MILLER, M. D., President of the College.

IN the spring of 1848 I performed, for the first time, the operation of ovariotomy on a poor woman, from the State of Indiana. A history of the case, and the favourable issue of the operation undertaken for its relief, were published in the July number of the *Western Journal of Medicine and Surgery*, for the same year. In the fall of 1857, a little upwards of a year ago, I performed the same operation, for the second and only other time, in the case of a woman coming to me from Ohio County, Ky.

I propose now to give some account of my second operation, having purposely deferred doing so until sufficient time should have elapsed to test the validity of its results, and then to present an abstract of the first case and trace the further history of the patient. In this way I hope to construct a platform of personal experience, from which, limited though it be, I may presume to speak with greater confidence of being attentively heard,

[1] Essay on Criminal Abortion. North Amer. Med.-Chir. Review, May, 1858.

when I come to express my views concerning the treatment of advanced cystiform degeneration of the ovary.

CASE II. Mrs. Ashby, æt. 30, came to the city for the purpose of consulting me on account of a large abdominal tumour, which was first noticed by her fifteen months previously, being then a small lump in the right iliac region. Since that time it has grown to such magnitude as to fill and distend the cavity of the abdomen, its superior boundary reaching to the epigastric region. She states that it is painful at times, especially after any considerable exertion, and that it has occasionally been the seat of very acute pain. She had borne but two children, the first being twelve and the second five or six years old. Since the last birth she has not conceived, nor did the menses return until twelve months subsequently to parturition, when they were too frequent as well as profuse for a while, but became scanty and protracted afterwards, as they are at present. Her appetite and digestion are impaired; there is marked emaciation, with pinching of the features and an expression of anxiety and suffering. The pulse is permanently accelerated.

Examination of the Tumour.—On making bare the abdomen, the unequal surface and irregular limits of the tumour are quite obvious. The density of its several portions, as tested by the touch, is various and at its upper part, where it is most yielding, distinct fluctuation is perceivable. Below the umbilicus, fluctuation is very obscure, if it can be detected at all. Examined per vaginam, the uterus appears to be of natural size, but is pushed over towards the left ilium, and on introducing the sound into its cavity, the point of the instrument can be felt through the integuments above the horizontal branch of the left os pubis. The inferior part of the tumour encroaches on the pelvic cavity, and can be easily felt through the walls of the upper portion of the vagina.

Diagnosis.—From the particulars, above recited, it was judged that the patient was the subject of encysted dropsy of the *right* ovary, of the multilocular kind, and furthermore that she could not much longer survive the inroads which it was so evidently making on her constitution. It was, therefore, deemed expedient to propose for her consideration the only remedy which appeared adequate to the crisis, viz., the complete extirpation of the offending growth. The formidable nature of the operation and the risk of its fatal issue were fully explained to the patient and her husband, and they took a week to consider whether it should be accepted or declined. Their decision being thus deliberately in its favour, preparation was made for its performance.

Operation.—On the 30th of September, 1857, the operation was performed at St. Joseph's Infirmary, in the presence and with the kind assistance of Drs. Flint, Bayless, Bell, Rogers, Palmer, and Donne.

The patient being placed upon a table in the usual position, anæsthesia was induced by the administration of chloroform in the hands of Dr. Bell, and the operation commenced by making an incision about two inches in length through the linea alba just below the umbilicus. A quantity of puriform and viscid fluid flowed from the incision, but whether it escaped from the cavity of a cyst, unintentionally opened, or from the cavity of the peritoneum into which it had found its way through an ulcerative perforation of the cystic coats, could not be certainly determined. At all events, a trocar was now thrust into the cyst, exposed by the incision, to evacuate

its contents and diminish the size of the tumour. A part of the fluid flowed into the peritoneal cavity.

Two fingers were next introduced between the surface of the tumour and the peritoneum lining the walls of the abdomen, in order to ascertain whether any adhesions had been formed, and to break them up should any be discovered. But the tumour was found free as far as the fingers could reach, and perfectly movable in the abdominal cavity. The external incision was now extended nearly to the symphysis pubis by the bistoury, guided by the forefinger of the left hand, dividing in its course the peritoneum, fascia abdominalis, and common integument, at the same time, and bringing more fully to view the tumour, which lay in front of the omentum. It was then evident that the tumour was too large to be extracted through the incision that had been made, and that consequently it could not be liberated except by extending the incision considerably above the umbilicus, or by still further reducing the volume of the tumour by puncturing other cysts. The latter alternative was adopted, and by this means there was additional effusion of cystic fluid into the cavity of the peritoneum.

The entire hand was next introduced into the abdomen, and carried behind the tumour to push it through the incision; but resistance was encountered, and in the effort necessary to overcome it, the posterior face of the tumour was lacerated by the points of the fingers, opening other cysts, whose contents escaped, in large part, into the peritoneal cavity. The mutilated tumour was, however, delivered at the expense of considerable stretching of the aperture which had been made for its release, and committed to the hands of an assistant, who raised it sufficiently to bring its pedicle to view. The pedicle was then punctured about an inch from its attachment to the tumour, and an aneurism-needle, armed with a strong double ligature of twine, was passed through it and securely tied. This being done, the pedicle was cut across close to the tumour, and there was no bleeding whatever from the cut.

The fluid of the cysts mingled with the blood effused from their torn and punctured walls, was carefully removed from the peritoneal cavity by sponging, and the left ovary subjected to examination. It presented a somewhat vesicular appearance, insomuch that it was thought to be in a diseased state by one of the medical gentlemen present, who suggested the propriety of its removal. But being unwilling to unsex my patient, without indubitable necessity, I suffered it to remain, and it has since done some service to the State, as will appear in the sequel.

The wound was closed with deep sutures, about an inch from the incised edges, through the parietes of the abdomen, avoiding the peritoneum, and about three-quarters of an inch apart. The edges of the wound were then brought more exactly in apposition by smaller interrupted sutures, passing through the skin only in the intermediate spaces between the deep ones. The ligatures upon the pedicle, which had been brought out at the inferior part of the wound, were tied to a piece of bougie to guard against their retraction into the abdomen, as well as to draw the end of the pedicle as near the surface as possible, and so avoid irritating the bowels by their contact. Long strips of adhesive plaster were applied over the superficial sutures to cover the wound and support the abdomen: compresses were placed vertically on either side of the wound, and a many-tailed bandage of flannel was pinned over the whole to complete the dressing.

The patient was greatly exhausted when the operation was finished; appeared restless, and vomited once. She was put to bed, and made as

dry and comfortable as possible; 40 drops of laudanum were administered, and she took brandy and water freely; a large sinapism was applied over the stomach, and bottles of hot water placed near the extremities. The nurse was directed to repeat 20 drops of laudanum every hour until composure ensued.

Night (nine hours after the operation). She had taken seven doses of 20 drops of laudanum; had not vomited any more; pulse good; extremities warm, except the toes. The urine was drawn off with the catheter. Two or three spoonfuls of chicken-soup every hour was ordered for nourishment, and the only medicine directed was laudanum, provided she complained of pain or restlessness.

October 1, 9 o'clock A. M. She had a pretty comfortable night, sleeping by naps; took laudanum once only; pulse 104; temperature natural; complains only of the wound. Catheter used, discharging half a pint of high-coloured urine. Prescription: To have soup more freely, and milk-toast. At 9 P. M., pulse 120; no pain. Catheter drew off, as in the morning, half a pint of high-coloured urine. Continue nourishment, and laudanum to keep down pain.

2*d.* Morning visit. She had rested well during the night; took small doses of laudanum occasionally; no pain, or abdominal tenderness or distension; catheterized; everlasting half pint of high-coloured urine. Prescription: Laudanum to quell pain, and for nourishment corn-bread, which she much craved, in addition to her soup. Evening visit. Pulse 104; no abdominal pain or tension; catheter; laudanum *pro re nata.*

3*d.* Morning. Complains of vesical uneasiness, and on introducing catheter found a larger quantity of urine not so high-coloured; dressed the wound, which looks well, and appears to be uniting by the first intention. Evening. Complains of considerable uneasiness from distention of the bladder; catheter; prescribed 40 drops of laudanum, and 20 drops to be repeated twice.

4*th.* Morning. Rested well last night; directed nurse to draw off the urine with catheter every six hours.

5*th.* The bowels not having been moved since the operation, a teaspoonful of sulph. magnesia was given, and followed by an enema in six hours, which procured two free evacuations. Doing well.

6*th.* The wound was again dressed; uniting; no suppuration; no tenderness, distension, or pain of abdomen; has a good appetite, and passes urine without the catheter.

From the last date to the 10th, nothing occurred worthy of special notice: with the aid of laudanum she rested well; the bowels were kept open with small doses of Epsom salts and enemata, and the wound firmly united, except at its inferior angle, where the ligature of the pedicle kept it open; her appetite continued good, and she complained only of a little dysuria. On the 10th of the month, which was also the tenth day after the operation, all the sutures were removed, and there was some suppuration about the ligatures of the pedicle. They were slightly pulled, but found not to have become detached. From this time to the 17th, the patient went on well, and on this day one of the pedicular ligatures yielded easily to traction, and was removed: the wound here was granulating. On the 19th, the other ligature came away, and the small wound, which was kept open by the ligatures, soon closed and cicatrized. Previously to this, the patient had been sitting up during a part of the day: henceforward,

until her husband came for her, on the 12th of November, she improved in flesh and strength; her countenance became more animated, colour revisited her cheeks, and she impatiently expected the return of her husband.

The remnant of the tumour, which was extracted in this case, weighed upwards of *eleven* pounds. It was multilocular in its character, as you will perceive from an inspection of it as it lies on the table before you. In its recent state, it bore unmistakable marks of having been the seat of acute inflammation, viz., its external surface presented, at various points, areas of considerable size, of a whitish colour, caused by deposits of lymph, and the internal membrane of the cysts that were opened was extremely red and highly vascular. This was especially the case with the largest cyst, in which fluctuation existed, and the fluid it contained appeared to be an exudate, most completely transformed into pus.

Remarks.—Mrs. Ashby's husband returned to the city, as already stated, about the middle of November, to take her home. I received a letter from him, in the course of a few weeks, informing me that her menses had not returned, and that she was complaining of nausea and occasional vomiting. Suspecting that she might have become *enceinte*, I wrote to Dr. Hale, of Hartford, the shire-town of Ohio County, early in the ensuing summer, and requested him to inform me of my patient's condition. The doctor very obligingly paid her a visit, and reported that her health was good, and that she was about seven months advanced in pregnancy. She has since been delivered of a living child, as I learn from the following letter from the doctor, which lays me under renewed obligation:—

HARTFORD, August 23, 1858.

DEAR SIR: I presume you would like to have some further and more definite information in regard to the case of Mrs. Ashby. She was delivered on the 16th instant of a living and well-developed male child, weighing seven pounds. I was called to her assistance, but arrived about two hours after her delivery. She had no help save that of two of the neighbouring women, who possessed no skill in that capacity. I was informed that she had an easier and more speedy labour than she had experienced in her former confinements. There are no signs of a return of the disease for which she underwent the surgical operation, which you performed. I heard from her yesterday; she and the child were both doing well. Yours respectfully, J. HALE.

From the dates, above referred to, it will be observed that the aptitude for procreation returned very early after my patient was freed from her ovarian burden. In fact, conception must have taken place either before she left the city, or immediately after she reached home.

I will now compile an account of my first case from the *Western Journal of Medicine and Surgery*, in which it was originally published:—

CASE I. Mrs. McLaughlin, widow, æt. 37, residing in the State of Indiana, came to the city, in the winter of 1847, to consult me in relation to a tumour in the abdomen, which was a source of no little annoyance to her on account of its size. On inquiry I learned that she had first observed a tumour, of small size, in the right iliac region, some time during the previous fall, which increased rapidly until it acquired its present magnitude. The menses had only appeared once or twice, and that in small quantity

and of short continuance, since the tumour first attracted her attention. She had felt no acute pain in the tumour at any time, and her general health did not appear to have suffered much, her appetite and digestion being pretty good.

On examination I discovered a large tumour in the abdomen, extending considerably above the umbilicus, and mostly occupying the right side, but stretching across the linea alba into the left side. Its shape was irregularly globular, and its consistency variable, being at some points much firmer than at others, and in portions of it fluctuation could be distinguished. Pressure upon it, even rough handling, gave no pain; and it appeared to be, in some degree, movable in the abdominal cavity. Examined *per vaginam*, the tumour could be felt within the pelvis, and the uterus was found in its normal condition.

The history of the case, taken in connection with the disclosures made by the examination, authorized the inference that the patient had multilocular dropsy of the right ovary. Upon being told that excision of the tumour afforded the only hope of a radical cure, the patient urgently demanded the operation, notwithstanding that she was made fully acquainted with its dangerous character. Indeed, its dangers were portrayed in more sombre colours than statistics strictly justified, for I would gladly have gotten rid of the case. I managed, therefore, to put her off on the plea of the unfavourableness of the weather—it being winter—and induced her to return home, taking with her some prescriptions which I hoped might benefit her.

In the first spring month following, namely, in March, 1848, Mrs. McLaughlin, true to her purpose, made her second appearance and avowed her determination to have the tumour removed by the knife. In sooth, she was not so well now as she was when she first came under my observation: the tumour had not, perhaps, sensibly increased in size, but she had perceptibly emaciated, and there was a frequency and quickness of pulse that betokened hectic irritation of her system. Her appetite had become poor, her bowels were irregular, and the tongue red and sore. It seemed, therefore, proper to get her system into a better state before resorting to an operation, so likely to test its powers of endurance; and, accordingly, she got mild alterative and aperient medicines every day until the secretions became healthy, the tongue more natural, and the pulse less accelerated.

On the 29th of March, 1848, a council was convened, consisting of Prof. Gross, and Drs. Bayless, Colescott, and T. L. Caldwell, at which it was judged advisable, at that time, merely to puncture the tumour, where fluctuation was most evident, with the view of diminishing its size and of allowing further time to improve the general health. I accordingly punctured it with a trocar, in the linea alba, about three inches below the umbilicus, and discharged *twelve pints* of a whitish, ropy, albuminous-looking fluid. When the fluid ceased to flow, the abdomen was flaccid to the left of the linea alba, and also a short distance to the right; but in the right iliac region, reaching up to the ribs of the same side, it still felt hard and unaltered in shape. No unpleasant symptoms followed the operation, and the puncture soon healed.

Operation.—On the 6th of April, 1848, the operation of extirpating the encysted tumour of the ovary was performed, with the valuable aid of Prof. Gross, and Drs. Bayless, Colescott, W. B. Caldwell, and T. G. Richardson. The patient being placed upon a table, on her back, her feet resting on a chair, and chloroform having been administered, an incision was made

through the integuments over the linea alba, from the umbilicus nearly to the pubes. The fascia and peritoneum were then divided, by several strokes of the scalpel, at the superior part of the incision, and a finger introduced to guide the probe-pointed bistoury, with which an incision was made through these tissues corresponding to that of the integuments. The tumour was now brought to view and found adherent, though not very firmly, to the omentum and parietes of the abdomen. To break up these adhesions, obstetrician-like, I passed my hand into the abdominal cavity and around the tumour, and had no difficulty in accomplishing the object. It being now ascertained that the tumour was too large to be brought through the opening that had been made, two of the cysts were punctured and a quantity of fluid discharged, resembling that which had been drawn off by tapping, a week previously; the quantity could not be estimated, as it was suffered to run upon the table, whence it flowed on the floor and across the room, there being no vessels at hand to receive it. By getting my hand posterior to it, I now drew the tumour through the incision and supported it while Dr. Gross secured its pedicle by a strong ligature, passed through the broad ligament and tied round the Fallopian tube and ligament of the ovary: its connections were then severed by cutting across the broad ligament near the inferior portion of the tumour. Finding that the vessels of the outer extremity of the broad ligament bled considerably, they were secured by another ligature which included the whole of its margin.

The tumour being removed, the cystic fluid which had been effused, in considerable quantity, into the abdominal cavity, and likewise the blood, were carefully sponged out, when the wound was closed by the interrupted suture, adhesive strips, compresses, and bandage—the ligatures having been brought out at the inferior part of the incision. The operation was commenced at 1 o'clock P. M., and the patient was put to bed in less than an hour, having borne it remarkably well; in fact, she felt, as she declared, very little pain, being all the while under the influence of chloroform.

5 o'clock P. M. same day. The patient is comfortable; pulse 68, and of good volume; she has vomited once, and complains still of occasional nausea. Gave 30 drops of laudanum, which is to be repeated in an hour, if not relieved. At 10 o'clock P. M. she was alarmed by discharge from the wound, following vomiting, and I was sent for. Notwithstanding she appeared fully as well as at previous visit, yet to allay her fears, the dressings were removed, when it was discovered that there had been a free escape of bloody serum, most probably a remnant of what had been effused during the operation.

April 7. Morning. Found my patient doing well; no pain, or sickness, or tenderness of the abdomen; but she had slept little or none; had not urinated, and said she could not in recumbent position to which she was confined; catheterized; ordered to be kept quiet, and to have no nourishment but rice or weak tea and toast. Evening. Slight febrile excitement; pulse 72; skin warm; had slept some, and passed urine.

8*th*. Morning. Passed a good night; complains only of weariness of confinement to back; appetite; has urinated; pulse 68. Evening. Pulse 68; nothing worthy of notice has occurred.

9*th*. Morning. Had slept well; cheerful; pulse 68; no pain; tongue slightly furred; bowels not having been open since the operation, attempted to relieve them by a large warm-water enema, but very little feces were brought away by it. Evening. Has had some pain in bowels during the

day; tongue not more coated than in the morning; a little fulness in epigastrium; prescribed a purgative of blue mass, rhubarb, aloes, and soap.

10*th*. Morning. The medicine not having operated, ordered the syringe again, but without effect; no pain or swelling of the abdomen; rested better last night than since the operation; pulse 68. Evening. Bowels not having acted, ordered a tablespoonful of castor oil, to be repeated, if necessary.

11*th*. Morning. The oil had purged her freely several times in the course of the night; no complaint. Evening. Nothing to note.

Henceforward a diary would not be interesting. Suffice it to say that no unfavourable symptoms occurred at any time. Her appetite continued to improve, and the bowels acted naturally, or, at most, she took a little oil once or twice. The wound was examined on the 16th of the month, but finding that it was not yet firmly united, the sutures were not cut, nor were the ligatures of the pedicle pulled. On the 19th (thirteenth day after the operation), there being some suppuration about the sutures, they were removed, and the adhesive strips, compress, and bandage replaced; the ligatures were not touched. On the 25th (nineteenth day after the operation), one of the ligatures yielded to gentle traction and was removed; but the other, though tried at every dressing, did not yield until the 7th of May (thirty-first day after the operation), at which time the wound was firmly closed, except the small opening through which the ligatures were brought out. The next day she started home, where she arrived without accident of any kind.

The tumour in Mrs. McLaughlin's case, as in Mrs. Ashby's, was multilocular in its structure, and weighed, notwithstanding its copious depletion by tapping, *nine and a quarter pounds*. A preparation was made of it, which was destroyed by the burning of the Medical College, on the last day of December, 1856.

Through the kindness of Dr. CHARLES L. HOOVER, of New Albany, Ia., I have been enabled to trace the history of Mrs. McLaughlin subsequent to the operation. The doctor is acquainted in her neighbourhood, and did me the favour to write to a medical friend residing there, from whom was obtained the following information, which he obligingly communicated to me by letter, under date of September 26th, 1858.

Mrs. McLaughlin did not marry again, but her health continued good, in every respect, until within a short time previous to her death, which occurred in November, 1855, *seven years and six months* after she returned home. Her catamenial discharges were regular and normal up to the last few months of her life, when they became irregular and profuse, owing, as it was supposed, to her peculiar age. She is stated to have died of a congestive chill, her menses being on her at the time, in inordinate quantity, though they had been scanty at the two menstrual periods immediately preceding. "There had been," the doctor remarks, "no visceral disturbances—no indurations; nor had the other ovary suffered in any way."

Having brought together the two cases in which I deemed it expedient

to extirpate encysted tumours of the ovaries, I shall now state the grounds on which I judged the operation to be not only proper but indispensable. As, however, the nature of the disease itself is largely entitled to our consideration, when we are seeking an appropriate and adequate remedy, I must beg indulgence whilst I endeavour to unfold, very briefly, the pathology of cystic growths of the ovaries. To Dr. Hodgkin we are, as I conceive, chiefly indebted for our knowledge of the subject, and I shall attempt but little more than to present the results of his inquiries, venturing only to throw out a single suggestion, which has been prompted by a careful examination of the specimen before us.

The ovary may be the seat, 1st, of a simple cyst; 2d, of a compound cyst, formed of a capsule containing secondary, and even tertiary cysts. The first of these, we have good authority for affirming,[1] seldom becomes larger than an orange, and is for the most part only detected after death. By Dr. Hodgkin, however, we are told that when these simple cysts are formed in the folds of the broad ligament, or intimately connected with the ovaries, if not imbedded in their substance, they at times acquire a very large size and constitute one of the forms of ovarian dropsy.[2] There can, nevertheless, be no doubt that it is with the compound ovarian cysts we are chiefly concerned when surgical intervention is called for; we shall, therefore, confine our attention to them.

Cysts of this second kind do not acquire their complexity by the aggregation of a number of simple cysts; but they are originally single, and become compound in consequence of the remarkable property which their parietes possess of producing other cysts of a similar character with themselves, on which account they have been denominated by Mr. Paget[3] "proliferous" cysts. These secondary cysts are endogenous in their growth, and encroach upon the cavity of the primitive cyst in proportion to the size they attain. The forms they assume are various: they may be pedunculated, hanging by slender stalks in the cavity of the principal cyst, each having its own peduncle, or a number being suspended by a common peduncle; or they may be broad and flattened—compressed and bound down, as it were, within the parietes of the principal cyst. Dr. Hodgkin commences his description of them with a form intermediate between these two extremes to which our specimen belongs, because it best exhibits the peculiar structure of these growths; for herein they are evidently cysts of like structure with the principal cyst, whose cavity they invade, and may eventually occupy completely, as they do in the tumour before us.

The tissues of which these cysts, whether of the primary, secondary, or

[1] Prof. John Hughes Bennett, of Edinburgh, in his recently published work, Clinical Lectures on the Principles and Practice of Medicine, p. 714.

[2] Lectures on the Morbid Anatomy of the Serous Membranes, p. 147.

[3] Lectures on Surgical Pathology, Phila. ed., 1854.

tertiary order, are composed, are richly supplied with blood, and the lining membranes of all of them pour out abundantly a secretion, which fills and distends them. This secretion, serous in its character in the early period of the disease, becomes albuminous, and sometimes quite tenacious, at a later period, and with it may be mingled pus or blood, imparting to it a yellowish or chocolate colour.

Various changes are liable to occur in the cysts contained within the primary one. Those of the secondary order may be extensively ruptured by the development of the cysts within them, or small apertures may be formed, the result also of distension: in either case, they pour out a part of their contents into the interior of the larger cyst. In both events, but especially in the latter, as Dr. Hodgkin remarks, "the opened cysts bear a considerable resemblance to mucous follicles on a large scale; and appear to be the principal source of the very copious and rapidly produced mucous secretion, which is a characteristic feature, in many cases of ovarian dropsy. This mucus bears a very close resemblance to that of the follicles of Naboth; and is frequently so viscid, that it passes with difficulty through the canals."

Again, the internal cysts, distended with fluid, may press upon each other to such a degree as to arrest further growth, at certain points, and cause the absorption of the intervening tissues, by which a multilocular cyst is formed. By such a process as this, the cavities within the tumour become less numerous but larger, whilst the bands stretching across them are all that remains of the partitions that had originally divided it into many separate compartments. The operation of this principle of change is well illustrated by the largest cyst in the preparation before us—that which occupied the superior part of the tumour, which was punctured during the operation, and from which flowed the puriform fluid. Nor is the external sac exempt from the effects of such an influence: should the pressure act upon it, it may become thinner and thinner until perforations are produced in its coats, through which, as Prof. Bennett has particularly pointed out, the fluid contents of the cyst escape into the abdominal cavity, making it necessary to resort to the operation of paracentesis.

At an advanced period of the disease, inflammation often takes place in the structure of the containing as well as of the contained cysts, but especially of the latter, which, according as its product is of the plastic or inorganizable kind, leads to the formation of adhesions between them, or purulent deposits within their cavities. The fatal progress of the case is greatly accelerated by this event: if the pus escape through perforations in the external sac, peritonitis is excited; if it is retained within the cysts, irritative fever is set up, and the patient sinks exhausted, whether the fluid be evacuated by paracentesis or not.

With respect to the anatomical nature of the tissue composing the parietes of ovarian cysts, pathologists do not appear to be agreed. Dr.

Hodgkin, for example, considering it adventitious serous membrane, whilst it is regarded by Dr. Bennett as fibrous, in the greatest thickness of the cysts, lined by a delicate membrane covered with epithelial cells. An attentive observation of the specimen on the table has led me to the conclusion that, whatever may be the anatomical character of the tissue itself, the principal thickness of the cystic walls is made up of minor cysts, filled with a glutinous fluid. After the preparation had been kept in alcohol for nearly a twelvemonth, it was taken out of the jar by the Curator of the College, Dr. Bayless, and myself, for the purpose of making a dissection of it. We found, upon cutting into the cavity of the cysts, which had not been opened before, that their walls were upwards of an inch thick, and that notwithstanding the cut surfaces were smooth at first, upon macerating them in water and subjecting them to strong pressure with the hand, a great many lumps of a whitish inorganic substance were forced out, bringing to view, as you may perceive, a cellular structure resembling very coarse sponge. The entire thickness of the cyst-walls exhibits this spongy appearance, and there is nothing left but the cellular tela in which the concrete masses of the albuminous secretion, hardened by alcohol, had been imbedded. From these appearances it may, I think, be inferred, so far as a single observation can justify an inference, that even the parietes of these cysts are cystic in their structure, or, in other words, are made up chiefly of an innumerable progeny of minor cysts.

In calling your attention, for a few moments, to the *therapeutics* of encysted tumours of the ovary, I shall confine my remarks to what has been proposed or attempted with a view to the radical cure of the disease. The interest of the subject has been latterly very much enhanced by the supposed discovery of a substitute for excision, which consists in the puncture of the cyst or cysts, and the injection of tincture of iodine into their cavities to destroy their secreting property. This was the principal theme in a late highly interesting discussion of the subject of ovarian cysts in the Imperial Academy of Medicine, at Paris, in which participated such men of renown as MM. Malgaigne, Cruveilhier, Velpeau, Moreau, Cazeaux, Piorry, Jobert (de Lamballe), Trousseau, Cloquet, Robert, Huguier, Gimelle, Guérin, Barth—in a word, the representative men of the several departments of medical science and practice in the French capital. The discussion was continued through several sessions of this learned body, and published in successive numbers of the *Bulletin de l'Académie Impériale de Médecine*, beginning with the number for October, 1856, and concluding in February following.

It is remarkable that in this large and magnificent assembly, only one voice was heard from the tribune in vindication of the extirpation of diseased ovaries, under any circumstances, and that voice, I rejoice to add, was M. Cazeaux's; I rejoice because the laurels, which the illustrious speaker has won, have been gathered from the obstetric field, in which I

am myself an humble labourer, and because, moreover, it is my firm conviction that the closer contact and greater familiarity of all such with the peculiar diseases of the sex, entitles their opinions to the very highest deference. By all the other speakers, who expressed their sentiments on the question, ovariotomy was scouted as a rash and wholly unjustifiable operation. The operation, said M. Malgaigne, appears to me infinitely too radical, and well calculated to secure women too absolutely against the danger of relapse. The statistics which have been alleged, he exclaims, prove nothing; we know the value of statistics, where all the success has been collected, but the reverses are concealed. It is conceded by M. Piorry that when ovarian tumours have acquired a certain magnitude, and are withal movable and unattached, we might rigorously attempt their excision; but, he hastens to add, to do this one must possess American audacity (*une audace Americaine*), and though he admits not the separation between physic and surgery, he avows that he will leave such an operation to other hands than his own. Notwithstanding the judgment pronounced against ovariotomy from the tribune of the Imperial Academy, specimens only of which have been cited, it were easy to show, by the confessions of the judges themselves, that neither iodine injections, nor any other measures that have been proposed, hold out the faintest hope of cure to miserable women, pining under *multilocular* cysts of the ovary, and doomed to inevitable death at no distant period. Hear M. Cruveilhier: after having pointed out the pathological differences among ovarian cystic growths, he divides them into two very distinct categories; the one comprehending the species which are, to use his own language, "marked with the seal of incurability;" the other, comprehending the curable species. The species which are absolutely incurable are:—

1st. Areolar cysts, whose incurability results from their nature, which is in no wise cancerous, but from the viscidity, the non-fluidity of the albuminous or gelatinous matter which fills the cells.

2d. Vesicular cysts, whose cells, whether communicating with each other or not, are equally filled with viscid liquid.

"For these kinds of cysts," he declares, "there is no treatment, either palliative or curative." "No palliative treatment," he continues, "for puncture of the cysts discharges little or nothing; no curative treatment, for there is but one mode of cure, namely, extirpation; and although extirpation of ovarian cysts may have been inspired, in some sort, by the isolation of the cyst, by the perfect integrity of the surrounding organs, by the facility of the operative procedure; although it may have been practised a great many times with success, especially in England and America, I do not think that this bold operation is entitled to be cited in science; rash enterprises are not always justified by their success." From the concluding moral of this quotation we beg to dissent. In a condition of peril, abandoned by the prudent and scientific as utterly hopeless, we hold that any

way of escape that may be opened is justifiable and manifestly better than no way at all. It is surely better *anceps auxilium experiri quam nullum*, if there be any truth in Celsus.

The inapplicability of iodine injections to cases of multilocular cysts of the ovary is one of the therapeutical facts most indubitably established by the discussion we are reviewing. Among all the French practitioners, who have made trial of this method, M. Boinet would appear to have had the largest experience of it, and the results of his practice are fully communicated by M. Cazeaux, in the course of the debate. According to his report, iodine injections were employed by M. Boinet in 44 cases, occurring among women, whose ages varied from fifteen to seventy-eight years, and were equally well borne by all. To judge of the results, the cases are classified according as the cyst was unilocular or multilocular, and also according to the quality of the liquid contained therein: 30 of the cysts were unilocular, of which 21 contained a serous fluid, of a citrine colour, and flowing freely; 2 contained a slightly sanguinolent liquid; 6 a liquid purulent or sero-purulent; 3 an hydatic liquid; 1 was complicated with fibrous tumour. Of the 21 serous unilocular cysts, 19 were cured after one or many punctures and injections; 2 died of peritonitis and purulent absorption. Of the 2 women who had sanguinolent liquid, one was cured; and the other, who had two cystic tumours, was cured of one of them, the other tumour being reproduced—one cure, one failure. Of the 6 purulent cysts, 4 cures, 1 relapse, 1 death. The 3 hydatic cysts were all cured. Thus, out of the 32 cysts, pertaining to 30 patients, the results were 27 cures, 3 relapses, and 2 deaths.

The results of the operations performed on multilocular cysts were much less favourable. Eleven cases of this kind were treated by M. Boinet; 6 died, 5 were not cured and were considered by him incurable! M. Cazeaux cites a number of cases reported by other practitioners, which need not be particularly referred to; they all agree substantially in this, that whereas unilocular cysts may be obliterated by iodine injections, or at least made to cease secreting, multilocular cysts are incontrollable by such appliances. Nor is it difficult to assign a satisfactory reason of the difference; it is not possible, by a single puncture, to make the injection penetrate the numerous separate cells composing such a structure. From the nature of the case, as well as from the testimony elicited by this discussion, we may safely conclude that multilocular cysts of the ovary are wholly beyond the therapeutic powers of iodine injections.

But I have ventured to affirm that compound cysts of the ovary are alike impregnable by all the means that have been brought to bear upon them, short of their complete extirpation. The minor incision, for example, by which only a small opening is made into the cavity of the abdomen to gain access to the ovarian cyst and to excise a portion of it, in order that its secretions may ever afterwards escape into the peritoneal sac where they

may be absorbed, is confessedly applicable only to cysts of the unilocular kind. Of what avail could it be to slice off a portion of one of the many cysts that constitute in the aggregate multilocular tumour of the ovary?

Far be it from me to wish to depreciate the value of iodine injections in unilocular ovarian cysts; but it may be reasonably feared that too sanguine hopes, inspired by the disclosures of the French Academy, may be dashed by future and more extended experience. In so far as the term "unilocular" is restricted to *single* cysts in contradistinction to *compound* cysts, we may indulge the hope that such may be incapacitated for further growth and future mischief by the modifying influence of iodine topically applied. But it is very evident that under this appellation tumours are often included, which are identical with the multilocular. We have seen that the tendency of multilocular cysts is from the complex to the simple, by reason of the absorption of their intervening walls; and this process may be continued until but a single great sac is the product, with only the vestiges of the septa that had originally divided it. Then, again, development may, from the beginning, be concentrated in one only of the numerous cysts of a compound tumour—the rest being kept in abeyance within the parietes of the exuberant one, or else hanging by slender peduncles within its cavity. In either case, though there be one great cavity, there are present all the elements of a compound growth, which may be stayed, for a time, by *iodination,* only to renew their march of development after their leader has been vanquished. In this state of dubious truce, I suppose the disease to exist in all those cases of so-called cure of ovarian cysts by iodine injections, in which it is reported, many months after the operation, that a tumour of considerable size could still be felt in the abdomen. There is reason to suspect this multiple nature of "unilocular" cysts when their walls are thick and their cavities are filled with a glutinous fluid. In such, so far as the rather meagre statistics, furnished by the Academy, can enable us to judge, iodine has had but poor success, and is scarcely less fatal than the excision of the whole of the diseased mass.

On the whole it may be concluded that the treatment of ovarian cysts by iodine injections and the excision of a portion of the cyst has only a limited application, and that where the cysts are compound in their nature, there is and there can be but one method of effecting a radical cure, namely, the total extirpation of a growth, which, though not heterologous, possesses such amazing fecundity of soil, and leads, sooner or later, to dissolution. To me it appears that no indication of treatment, even the best established and the most generally recognized, is clearer than this, or better entitled to universal reception. The brief but cogent reason assigned by Celsus to justify the extirpation of a gangrenous limb, is equally applicable to such an irrepressible growth—*Nihil interest an satis tutum præsidium, quod unicum est.* The only question, in my opinion, of doubtful disputation, is the stage of the malady and the circumstances accompanying it, which

plenarily justify a resort to the operation of ovariotomy. These must, of necessity, be discussed and settled in each particular case, after a full survey of the condition and prospects of the patient. So long as the tumour is of such moderate dimensions as to give but little mechanical inconvenience, and to interfere but little or none with the functions of digestion and nutrition—the patient suffering but little, and being capable of fulfilling many of the duties and enjoying most of the pleasures of life—nothing but an insane *cacoethes secandi* could tempt a humane practitioner to think of subjecting her to the pains and perils of the operation. But when the tumour has acquired such volume as to fill and distend the abdomen, making exercise difficult and all duties a burden; when by its encroachment on the abdominal and thoracic organs, their functions are embarrassed, and increasing debility and emaciation are the consequences; when pain and tenderness of the morbid growth evince the presence of inflammation in one or more of its cysts, and the resulting quickened pace of the circulation is contributing to derange still further the lost balance of all the functions of the economy; these are all significant warnings of approaching danger, and declare audibly that the patient's days are numbered, and that without the decisive interposition of chirurgical art, she must be speedily consigned to the tomb.

Speaking from my own experience, I would say that the disease seldom or never reaches the stage demanding the operation, before the patient's mind is fully made up to brave its hazards, for the sake of the chance it affords, be it ever so small, of throwing off her intolerable burden. There is, consequently, no necessity of persuading her to submit to it; nay, the difficulty not unfrequently is to gain her consent to have it postponed, and this too with the gloomy statistics of the operation staring her in the face. With this intimate sentiment that life to her is not worth possessing, and the abiding conviction that its flame is flickering in the socket and must soon become extinct, it may well be asked, what right has the surgeon to refuse the operation, which she claims at his hands, were it attended with even greater risk than experience has indicated?

Indefensible as is the position of M. Cruveilhier in reference to ovariotomy, he is, nevertheless, strongly fortified in it by surgical authority, for not only in France, but also in Great Britain and the United States, the great surgeons, with hardly an exception, have set their faces against it, and it has yet to find its way into a single systematic treatise, ranked among legitimate surgical operations. Meanwhile, it has been successfully practised in scores of cases, and ably advocated chiefly by surgeon-accoucheurs, and there cannot be a doubt that the reproach of bastardy will be ultimately wiped from its escutcheon.

Mr. Liston, it is true, introduces the subject in his *Lectures on the Operations of Surgery*, but only with the view of condemning the operation, in the most unqualified terms, as "exceedingly unjustifiable," and of

informing us that he "has always set his face against it," and he "thinks always shall." His American annotator, Prof. Mütter, is "truly gratified to find Mr. Liston throwing the weight of his great authority in the balance against the operation of '*ovariotomy.*'" But this is a question which, involving, as it does, the dearest interests of womanhood, is not to be decided by authority, however ponderous; our appeal is to the higher tribunal of reason and experience.

The objections against ovariotomy are many and various; they have been mustered in strongest array by Prof. Mütter, in the work just cited, as follows:—

"1st. The difficulty of arriving at a just diagnosis. 2d. The danger of the operation itself. 3d. The nature of the disease does not sanction so violent a remedy. 4th. It is contended that palliatives will often succeed in making a patient comfortable during a long life. 5th. An operation does not always succeed in relieving a patient radically, even when she escapes the dangers immediately consequent to its performance. 6th. The disease may terminate spontaneously."

Bear with me whilst I attempt, briefly as possible, to inquire into the validity of the specifications adduced to substantiate each of these objections.

1st. *The difficulty of accurate diagnosis.*—I have no wish to disguise the fact, lamented by all practical men, that it is very difficult, sometimes impossible, to diagnosticate the precise nature, seat, and connections of tumours, deeply buried in the cavity of the abdomen. Nor must it be denied that gastrotomy has been performed for the purpose of extracting ovarian tumours, when, as it turned out, no tumour of any kind was found. Mr. Lizars' first operation, in 1823, was an instance of this kind—the operator being deceived by great obesity and distended fulness of the bowels—and five similar cases have been collected by Dr. Washington L. Atlee, of Philadelphia, and published in his *Table of all the known Operations of Ovariotomy*, from 1701 to 1851, which comprises 222 cases. In a still greater number of instances, after the abdomen was laid open, it was discovered that the tumour was uterine instead of ovarian, and the uterus even has been removed.

We may not hope to attain absolute perfection of diagnosis, either in relation to abdominal tumours, or any other malady incidental to our frail human nature—for even surgeons of deserved renown have been known to cut for stone in the bladder without finding one—but yet it is certainly true that the diagnosis of the diseases of the internal organs of generation in females has been vastly improved, since many of the mistakes were made, which are now quoted to disparage ovariotomy. For this improvement we are largely indebted to the uterine sound, the use of which, in connection with a careful digital exploration *per vaginam*, is capable, in many cases, of rendering the diagnosis of ovarian tumours as clear and precise as could be desired, so that not the shadow of a doubt need remain. Certainly this

was true in my own cases; ocular inspection could not more clearly have revealed their true nature. It will not, I hope, be deemed presumptuous in me to intimate here that one reason of the aversion of great surgeons to ovariotomy may be that, in the lack of an experienced vaginal touch, they are compelled to rely mostly on palpation through the walls of the abdomen, in forming their diagnosis of tumours contained within its cavity. If this be so, I do not wonder at the painful incertitude, which must perplex them after the most careful investigation, and indispose them to cut into the abdominal cavity at a venture. It is only a few days since I saw a striking exemplification of the insufficiency of abdominal palpation to elucidate the nature of tumours in this region. I was requested by an eminent practitioner to see a lady patient of his, who, he supposed, had ovarian tumour, and might be a good subject for an operation. Upon examining the abdomen, its great size, unequal distension, and hardness of feel, inclined me strongly to the opinion that the case was one of multilocular cysts of the ovary, and had there been no means of correcting this impression, I should have continued in that mind. But on touching *per vaginam*, I was soon convinced that the tumours were of the uterus, for the os was high up, and the cervix and body merged in the inferior part of the tumour, from which it could not be isolated or distinguished. From all that I have seen I am persuaded that, in the advanced stage of compound cystic tumour of the ovary, when alone an operation would be justifiable, errors of diagnosis need not be more frequently committed than in many other surgical diseases.

2d. *The danger of the operation.*—Under this head Dr. Mütter enumerates the diseases and accidents, not forgetting the *extreme suffering*, which may lead to a fatal termination. The suffering may be averted by chloroform, but no art or skill can prevent the occasional supervention of peritoneal inflammation, hemorrhage, shock, &c., which after this, as after all other great operations of surgery, may prove fatal. But the argument proves too much, for if it is valid against ovariotomy, it is equally so against amputation, the ligature of large arteries, and other capital surgical operations. Nor can it be shown that extirpation of ovarian tumours is attended with greater mortality than many of these, which are the boast of modern surgery. The statistics of ovariotomy establish this fact beyond all doubt, and it is vain for Dr. Mütter to attempt by a mere fling to impeach their truthfulness: no statistics in all surgery are more trustworthy or better authenticated. The rate of mortality of ovariotomy operations, according to Dr. Atlee's table, is $26\frac{1}{2}$ per cent., and it cannot be doubted that this will be very much diminished by confining the operation to cases for which it is suited, which our improved diagnostics will enable us to do, and by improvements in the *methodus operandi* itself.

"But," says Dr. Mütter, "I am not disposed to estimate the merits of this measure by statistics, nor should it be thus contrasted with other capital

operations." Why not? Because it smacks of illegitimacy? Let us hear. A writer in the *Edinburgh Medical and Surgical Journal*, for April, 1844, has, as Dr. Mütter conceives, taken the correct view of the bearing of the whole matter, and as his remarks are brief, the doctor begs leave to introduce them. Leave granted.—"If," he observes, "we look alone to the mortality, independently of all other considerations, and assume the above tables as correct in giving the ratio of mortality for the large abdominal incision, we find that it is not greater than for other great surgical operations. Thus, M. Malgaigne has shown that in all the Parisian Hospitals, from 1836 to 1840, inclusive, 201 amputations of the thigh took place, but of this number 126 died; and the result of amputations of all kinds showed a mortality of 38 in the 100 for *pathological* causes, and 40 in the 100 for *traumatic* causes. M. Textor, on the other hand, in mentioning the statistics of strangulated hernia, treated at Wurtzburg from 1836 to 1842, states that of those subjected to an operation, 32 were cured and 24 died, or 3 out of every 7 cases; while at Paris the mortality was 4 out of 7 cases. All this would seem, therefore, to be a strong proof of the legitimacy of the abdominal section, seeing that the mortality is not so high for it as for those surgical operations. This is quite true, but the difference between the one operation and the other is this, *that the one saves* 3 *out of every* 7 *patients, who could not by possibility survive even a few days, were the operation postponed; and the other sacrifices one unnecessarily to prolong for a few months or years the lives of two, who would, perhaps, after all have lived as long had no operation been performed!* In the one case the amputation, or the operation for hernia, is performed for the legitimate purpose of saving life, which otherwise could not be saved; in the other, or the abdominal section, life is heedlessly sacrificed in the attempt to relieve what, after all, is only a burden, and has never yet been found to shorten the average duration of human life. In the one case the surgeon is acting in conformity with the highest principles of humanity and morality, doing all he can to save the life of a fellow-creature; in the other, while we cannot deny that he may conscientiously believe that he is undertaking what is to save life, we fear he is often influenced more by the eclat of performing a great and dangerous operation."

In the foregoing extract, quoted with unqualified approval by Dr. Mütter, it will be observed that there are two affirmations, both of which must be made good, else the argument falls to the ground by its own weight: 1st. That the patients subjected to operation for strangulated hernia, and to amputation for *pathological* causes, could not possibly survive even for a few days, without the operation. 2d. That women labouring under ovarian tumour will live as long without an operation as with it. In regard to the first affirmation, you hardly need to be reminded that, taking even the most urgent and apparently desperate class of cases, namely, strangulated hernia, spontaneous reduction of the bowel may take place, even after stercoraceous vomiting has set in, or failing this, nature's surgery may rescue the patient from impending destruction by involving all the parts implicated in gangrene, and establishing an artificial anus. With reference to diseases requiring or supposed to require amputation, the affirmation of our author must obviously be received with much allowance for the fallacy of the most

enlightened judgment of the surgeon. Among the most frequent of the pathological causes, for which amputation has been practised, we may, perhaps, assign strumous disease of the bones and joints—the localization of a constitutional malady. It is to be feared that, in not a few instances of this kind, limbs are sacrificed which might have been saved, and in others, no advantage is gained by the operation, the constitutional taint remaining and localizing itself anew. Even when the necessity of amputation, in such cases, is indisputable, it can seldom be affirmed, with truth, that without the operation the patient could survive but a few days, for if danger be so imminent, it must be from extreme exhaustion, which should countermand a resort to the knife.

The second affirmation is still less tenable than the first, or I should rather say that it is wholly gratuitous. The writer, it is true, admits that the ovarian malady is a "burden," but then he ventures to assert that it is light, and "has never yet been found to shorten the average duration of human life!" Could this bold assertion be substantiated, it would be decisive of the question, and all would unite in denouncing ovariotomy as an inhuman operation. But unfortunately experience does not bear it out, for when we come to consult statistics, it is found that the average duration of encysted tumours of the ovary, treated only by medical means, is only two years, and none of the women thus affected lived longer than four years! Such is the teaching of the statistics collected by Mr. Thomas S. Lee, and published in his work on *Tumours of the Uterus and its Appendages*, as quoted by M. Cazeaux, in the debate at the French Academy. M. Cazeaux, after referring to the English author's statistics, remarks that he had hoped to gather a larger mass of evidence on this point by addressing letters of inquiry to numerous practitioners, but that he had been disappointed. His researches, however, so far as they were successful, go to corroborate those of Mr. Lee: for having collected 31 cases, he found that 7 only of the patients lived more than ten years, whilst the remainder lived on an average from two years to two years and a half, dating the disease from the time when the tumour had acquired appreciable volume. M. Cazeaux allows, however, that too much value must not be attached to these statistics, as it is highly probable that fortunate cases, being less interesting, are not published so frequently as the unfortunate ones. Making due allowance for this source of error, he is still of the opinion that when an ovarian cyst manifests a continual tendency to grow, and contains already from four to five *litres*[1] of liquid, the patient cannot on an average count on more than four years of existence. Not even this lease of four years' life is put in jeopardy by the operation I am defending, for it were easy to show, by a reference to cases, that before extirpation was resorted to, the tumour had attained to much greater size than that indicated by M. Ca-

[1] A French *litre* is a little upwards of a quart.

zeaux, and the failing flesh and strength and ill-performed functions of the patient betokened the near approach of a fatal termination, so that, in most, if not in all instances, only a few weeks or months of miserable existence were to be spun before the thread of life would have been snapped—existence, as has already been said, so dreary and so hopeless withal that the patients were most willing to risk it for the chance of relief held out by the operation.

3d. *The nature of the disease does not sanction so violent a remedy.*—To support the flattering prognosis of ovarian tumours, declared under this head, the opinion of the celebrated William Hunter is quoted, who stated "that a patient will have the best chance of living longest under it who does the least to get rid of it! This opinion is fully indorsed by Dr. Mütter, who further asserts "that it has not, as yet, been proved to have materially shortened the life of the patient, most of those who die of it reaching an average age." This objection has been already answered anticipatively; and it must, I think, be admitted that it has *now* been proved, if it had not been when Dr. Mütter wrote, that the disease is not so innocuous as it was formerly regarded.

4th. *It is contended that palliatives will often succeed in making a patient comfortable during a long life.*—In the advanced stages of the disease, when the tumour is large, or inflammation has taken place, it is alleged that rest, counter-irritation, leeches, anodynes, cathartics, low diet, and mechanical support, and when the distension is very great, *tapping*, will, for the most part, be sufficient for the relief of the most urgent symptoms. I will not depreciate the value of some of these means of palliation, but I do of course question their power to make a patient comfortable during a *long* life, for the simple reason that such a life is not allotted her. At the same time I am obliged to confess that these boasted palliatives are often miserable comforters at best. Take *tapping*, for example, which is usually reckoned foremost among them, and it must be allowed that the relief it affords is but momentary, while, if it be often repeated, the equally often repeated refilling of the cyst, by the drafts made upon the system, hurries on the case more rapidly to its mortal catastrophe. Nor must it be forgotten that tapping can, in the nature of the case, give only partial relief in multilocular cysts of the ovary, as only one cyst, though it be the largest, is usually punctured in the operation. Still less must the fact, not, perhaps, familiar to all, be overlooked, that the simple puncture of ovarian cysts is an operation by no means free from danger, as death has not unfrequently resulted from it within a few hours.

The estimate which has been expressed of the palliative tapping of ovarian cysts is fully warranted by statistical evidence, for in a communication addressed to the French Academy, pending their discussion of the subject, Dr. Schnepf presents a *resumé* of a long and laborious article by Dr. Fock, on the *Surgical Treatment of Cysts of the Ovary*, in which the results of

the various methods of treatment are furnished. From this work it appears that palliative puncture was practised by Southam, Lee, and Kiwisch, in 132 cases: of this number of patients, 25 died a few hours or days after the operation, 24 within six months, 22 before the end of one year, 21 within the second year, and 11 within the third year. "From these facts," he observes, "it appears that most often puncture accelerates both the progress and fatal termination of the disease, and that it ought not to be employed, except when a radical cure is out of the question."

5th. *An operation does not always succeed in relieving a patient radically, even when she escapes the dangers consequent to its performance.*— A strange objection indeed! Where is the operation in surgery, even of the purest legitimacy, that can produce such a record and challenge our acceptance by its universal success? The cause of possible failure, specially signalized by Dr. Mütter, is complication of malignant disease, for then there may be "relapses or formations of malignant diseases in other organs, from which the patient ultimately perishes." There is, truly, a possibility of such complication in cases of cystic disease of the ovary, though experience proves that it is rare, and when it exists the operation will serve no other purpose than, perhaps, to prolong somewhat the life of the patient. But this objection comes with an ill grace from legitimate surgery, one of whose practices it is to excise parts affected with cancer, with the hope of prolonging life thereby, though it be infallibly certain that the disease, being constitutional, will recur and put an end to the patient's life, at no distant period. Such is the practice recommended by Mr. Paget, whose authority will not be impeached, and whose surgical orthodoxy will not be questioned. The removal of scirrhous cancer of the breast is inculcated by him, under certain circumstances, though it makes no material difference in the *average* duration of life, because it "seems probable that the course of the more rapid cases is retarded by the operation." In support of this, he adduces two tables, one being made up of cases in which the disease ran its course uninterrupted by operative treatment, and the other of cases in which the cancer was once or more removed by operation: in both the average duration of life was something more than forty-nine months. While the two tables thus closely correspond, "it may yet be noticed," Mr. Paget observes, "that the proportion of those who die within two years is 36 per cent. of those in whom the disease is allowed to run its course, and only 24 per cent. of those from which the growth is once or more removed."[1] To gain an advantage, which is only appreciable by a close inspection and comparison of cases, the removal of the diseased breast is here recommended, though it be admitted by Mr. Paget that the operation is by no means unattended with immediate risk of life, for we are told by him that in 235 operations for cancerous and other diseased breasts, he finds 23 deaths, or a mortality of 10 per cent.

[1] Op. cit.

I am not finding fault with the canon of surgery expounded by Mr. Paget, but I may be allowed to insist that surgeons, to be consistent, must materially modify their views of ovariotomy, if they do not receive it into open communion with other capital operations of surgery. How stands the comparison between the excision of cancerous breasts and cystic degenerations of the ovary? The legitimate operation *appreciably* prolongs life, but it never yet, in a solitary instance, effected a radical cure of the disease for which it was performed; it, therefore, merits the suffrages of the surgical profession. The illegitimate operation *indefinitely* prolongs life, and has accomplished numerous radical cures of the disease which it sought to extirpate; it has, therefore, received the unmeasured denunciation of the surgical fraternity. If the excision of cancerous breasts be justifiable at an expense of 10 per cent. mortality, for the sake of adding a few days to a doomed life, why should not the excision of cystic ovaries be justifiable, at an expense of $26\frac{1}{2}$ per cent., for the sake of giving indefinite extension to life otherwise equally fated? for, according to our statistics, there is a remarkable coincidence between the duration of life in the two classes of cases, when they are permitted to run their courses unmolested.

6th. *The disease may terminate spontaneously.*—Ay, and so may any other surgical disease; yet these instances, in cases of cystic disease of the ovary, are confessedly rare—far too rare to justify any *methodus medendi* founded upon the expectation of their occurrence. As well might the accoucheur defer to deliver by turning in a case of shoulder presentation, because spontaneous evolution may take place, or the surgeon postpone an operation, in strangulated hernia, because spontaneous reduction may happen, as the surgeon-accoucheur refrain from a resort to extirpation of far advanced cystic degeneration of the ovary, because nature may chance to get quit of it by her unaided resources. When such a fortunate termination does occur, it takes place either by the bursting of the cyst, and the escape of its contents into the peritoneal cavity, where it may be absorbed, or by the formation of adhesions to the bowel, uterus, bladder, or parietes of the abdomen, and the establishment of an opening by ulceration through which the cyst may be discharged. But it is obvious that such an effort of the *vis chirurgica* can avail only in cases of unilocular cysts, for the multilocular cannot be evacuated through a single aperture.

I have now finished the defence of ovariotomy against the objections that have been opposed to it; with what success I leave you to judge. I cannot, however, dismiss the subject without entering my reclamation in favour of the late Dr. Ephraim McDowell, of Danville, Ky., as its original inventor, whose acquaintance I had the honour to make, just after my graduation in medicine. This task, exacted alike by justice and a sentiment of pride, kindled by the contemplation of all that is laudable and useful, pertaining to my native State, has been made easy by the labours of my friend and former colleague, Prof. Samuel D. Gross. As Chairman of the

Committee on Improvements in Surgery, appointed by the Kentucky State Medical Society, Dr. Gross presented to the annual meeting, in October, 1852, a report on Kentucky surgery in its entire range, which was published in the Transactions of the society. The nature of the duties assigned him led Dr. Gross to glean from every available source all the information that could be collected touching the acts of Kentucky surgeons, and to mark the improvements and innovations which they were instrumental in introducing. Tracing the history of ovariotomy from the first operation performed within our borders by Dr. Ephraim McDowell, in December, 1809, down to our own day, and then remounting to the primitive case, and searching the records of surgery of other countries, Prof. Gross soon satisfied himself that Dr. McDowell's operation is not only the first of the kind performed in Kentucky, but in all the world besides. Dr. McDowell was not eager to blazon his deeds, and hence it was not until he had performed the operation in three cases that any account of them was published, seven or eight years subsequently to the date of his first operation. Copies of this account in manuscript were sent to a few of his friends, and I well remember that Dr. William Richardson, Professor of Midwifery in Transylvania University, possessed one of them, which he was wont to read to his class. Another copy was forwarded to the celebrated John Bell, of Edinburgh, for whom Dr. McDowell had conceived an ardent admiration, while listening to his eloquent lectures in the famous university of that ancient and renowned seat of learning. The document fell into the hands of Mr. Lizars, on account of Mr. Bell's absence from home. Inspired by Dr. Mc Dowell's novel enterprise and its astonishing success, Mr. Lizars attempted the operation in 1823, but was foiled by the very material fact that after he had made the abdominal section, there was no tumour, ovarian or other, to extirpate. The future diagnoses of this eminent gentleman were more accurate, and his operations more successful.

The single point, however, to which I wish to confine my historical remarks, is that Mr. Lizars is unquestionably the first *transatlantic* surgeon who performed the operation of ovariotomy, in any technical and proper signification of the term. To L'Aumonier, of Rouen, is generally awarded the credit of first extirpating a diseased ovary, and his name is generally placed at the head of the list of operators in tables of ovariotomy. But the French surgeon only opened an abscess of the ovary consequent to parturition, and is no more entitled to the credit of originating ovariotomy than he would have been had he lanced an abscess of the mammary gland. In relation to the two other names that intervene between those of L'Aumonier and Dr. McDowell, in the tables alluded to, namely, Professors Dzondi and Galenzowski, Dr. Gross has shown, ludicrously enough, that the patient of the former was a lad, Christopher Shultz by name, who had a circumscribed tumour as large as his head in the hypogastric region; and a cure was effected by drawing out the cyst piecemeal through an incision

in the wall of the abdomen, with a pair of broad forceps. Galenzowski's was a case of multilocular cysts, but extirpation was not practised: the tumour was opened, the cells torn up, the mass fixed by ligature to the wound, and a perfect cure obtained, according to the synoptical account given by Dr. Atlee in his excellent table; but, according to Dr. Gross, the operation was performed in March, 1827, eighteen years after Dr. McDowell's first case, and four years after Mr. Lizars' first case.

From the facts and dates just referred to, we are not permitted to doubt that to Dr. Ephraim McDowell is justly due the imperishable honour of conceiving, maturing, and triumphantly executing the ablation of diseased ovaria by a bold invasion of their sanctuary, even the cavity of the peritoneum, which had been held inviolable by all former surgeons, and from which so many continue to shrink with dismay. Gastrotomy, in the fashion of the major incision, had never before been ventured upon, except with a view of rescuing the fœtus from entombment in its mother's womb, and the mother herself from the dangers of instrumental delivery *per vias naturales*, in cases of difficult parturition. And if a portion of the immortality of even Cæsar's name is due to its association with obstetric gastrotomy, it is not surely too much to say that the name of McDowell will live as long as ovariotomy is practised, and as there are grateful women to enshrine it in their hearts.

P. S. Since the reading of this paper, I have been informed by Dr. Middleton Goldsmith, Professor of Surgery in the Kentucky School of Medicine, that he has frequently heard his father say that Dr. Ephraim McDowell was in the practice of administering the tincture of opium to his ovariotomy patients, in very large and repeated doses, after the operation. He gave a teaspoonful for a dose, and repeated it at the usual intervals, until narcotism was induced, which was kept up for a considerable time—how long I am not precisely informed. Dr. Goldsmith, Sr., now of New York, himself an accomplished and successful surgeon, formerly practised his profession in Danville, Ky., and during a part of the time he resided there, he was the partner of Dr. McDowell, whom he assisted in some of the operations in question. He had, therefore, full opportunity of becoming acquainted with the minutest details of the practice of the father of ovariotomy, and I have referred to this salient point in the after-treatment of his patients, because it displays, in a remarkable manner, the sound judgment and great practical sagacity of Dr. McDowell. His object must be supposed to have been to repress peritoneal inflammation, and extinguish it in its very incipiency, by these large doses of opium.

It is doubtful whether at that day, now nearly a half century ago, opium had been recommended, or had recourse to, with such a view, by any practitioner, either in Europe or America; but at this time the practice is steadily gaining ground, and is destined ere long to be universal. To the value of opium as a remedy in peritonitis, the attention of the pro-

fession was first drawn, I believe, by Mr. Bates, of Sudbury, in a pamphlet which I have not seen, but which is referred to in *Braithwaite's Retrospect*, No. V. He recommended opium, both by the mouth and rectum, in the treatment of severe peritonitis. The practice was deemed chiefly valuable in cases where, either from ulceration or injury, perforation of the intestine has taken place, and which have been supposed to be almost universally fatal. Dr. W. Stokes has related some very striking cases, strongly confirmatory of the opinions of Mr. Bates, remarking that in many of these cases the patient sinks with awful rapidity, and no time is allowed to pursue the active treatment which is adopted in other cases. Purgatives, he thinks, would in such cases probably increase the evil, by *tearing asunder* recent adhesions, and thereby preventing nature closing the communications between the mucous and peritoneal cavities. Dr. Watson, the accomplished lecturer on Practice of Medicine, King's College, London, after citing cases from Dr. Stokes, illustrative of the efficacy of large doses of opium, where peritoneal inflammation supervenes upon perforation of the intestine, bursting of hepatic abscess into the peritoneal cavity, etc., earnestly recommends the same mode of treatment, as an auxiliary, where the peritonitis does *not* grow out of previous organic disease; in all cases, in short, of mere peritonitis. In our own country, Prof. A. Clark, of New York, has ably advocated the opium treatment of puerperal peritonitis, and in the reports of ovariotomy cases, in our medical journals, it is plainly discoverable that opium is much more freely given now than formerly, as a prominent part of the after-treatment. This is particularly noticeable in Prof. Van Buren's report of a case of ovariotomy in the *New York Journal of Medicine* for March, 1850. I cannot refrain from quoting the very high, but not extravagant, estimate of opium, expressed by Dr. Van Buren, in the report just cited: "When the remedy is administered freely, but always intelligently, the state of the pupils, the respiration, and, above all, its influence in keeping down pain, being closely watched; I believe that it possesses the power of preventing the development of inflammation as fully as chloroform will prevent the shock of an operation."

There can, I think, be no doubt that nothing has contributed to diminish the mortality attendant upon ovariotomy, more than the free use of opium for several days subsequently to the operation. An attentive examination of the history of individual cases will, I believe, bear out this assertion. In my own cases, I administered the remedy largely during this period. Not having kept a strict account of the quantity, I cannot say precisely how much of the tincture (the preparation which I prefer), Mrs. Ashby took, but it could not have been less than an ounce.

Valuable as opium is as a prophylactic of peritonitis and as an antiphlogistic when the disease begins to be developed, it is not, perhaps, less salutary in its effects by securing perfect quiescence of the bowels for five or six days subsequently to the operation of ovariotomy. I can conceive of

nothing more inopportune or noxious than the operation of purgative medicine within the period specified. The exaggerated peristaltic movements of the alimentary canal, which purging implies, are well calculated to awaken inflammation in the peritoneal membrane, to which a strong predisposition already exists, growing out of all the circumstances that so recently preceded, whilst the requisition made upon the abdominal muscles for their active co-operation, must necessarily interfere with the healing of the external wound, and might probably displace the ligature of the pedicle, giving rise to secondary hemorrhage. The recovery of both of my patients without a single untoward symptom, I am disposed to attribute more to the free use of opium, than to any other means that were employed, not excepting the genial and equable temperature which it was sought to maintain in their chambers.

But I have been wandering from the point. Dr. McDowell, as it appears from Dr. Goldsmith's testimony, was in the habit of giving his ovariotomy patients large doses of laudanum, and it is reasonable to suppose that his practice was designed to secure the ends, and ward off the dangers which we have been considering. If this be so, it exhibits him to us as being far in advance of his contemporaries in therapeutics, as well as in surgery, for they, as a general rule, knew of no other preventive of inflammation than low diet and purging, and no other remedy for the disease, when developed, than bloodletting, in copious effusions. Indeed, at that time, bloodletting was most rampant under the teaching of Dr. Rush, whose fame was then in its zenith, and whose eloquent voice and pen invested the lancet with irresistible charms.

ART. V.—*Notes upon the Effect of Alcohol, Glycerine, Water, Gum, Ammonia, and the Vacuum upon the Exposed Hearts of Frogs, Snapping-Turtles, and Sturgeons.* By S. W. MITCHELL, M.D., Lecturer on Physiology in the Philadelphia Medical Association. (Read before the Academy of Natural Sciences, Biological Department, December 20, 1858. Recommended for publication Feb. 22, 1859.)

IT is a matter of some interest as connected with the subject of the cause of the rhythmical action of the heart, to ascertain the effect of a vacuum upon its movements. At one time it was alleged that the contact of the air with the exposed heart was the stimulus which, under these circumstances, sustained its action. The simple experiment of placing the organ in vacuo to test the validity of this opinion was resorted to at various periods by Caldani, Wernlein, Kurschner, and John Reid, all of whom observed that the hearts of animals continued to pulsate in vacuo. On the

other hand, Tiedemann, Fontana, and Dr. T. H. Bache and myself, found that the heart in vacuo always fell off in activity, and, in most cases, when the exhaustion was complete, ceased to pulsate.

In certain experiments made by Dr. Bache and myself we found, as just stated, that the hearts of frogs and snappers always became less active in vacuo. Not all of these hearts, however, could be made to stop suddenly when a rapid vacuum was effected, and some continued to pulsate in vacuo for upwards of half an hour. Since, however, the frog heart will pulsate in the air for many hours when suspended, and not disturbed, the effect of the vacuum becomes in all cases sufficiently plain. Thus, when the heart of a snapper, beating in the air fourteen times per minute, was suspended in vacuo, the pulse fell to four per minute, and so continued during twelve successive minutes, acting, however, very laboriously, a single pulsation occupying as much as ten seconds.

On readmitting the air, the pulsations rose at once to nine per minute, and on applying moisture in the form of blood, to eleven per minute. This heart remaining in the air ceased to pulsate at the end of an hour and eight minutes. The single experiment just stated illustrates well enough what usually took place when either frog or turtle hearts were thus treated. The number of pulses always lessened. Sometimes the heart stopped suddenly, beating anew when the air was let in on it, and in all cases exposure in vacuo, even for a time, shortened the length of the period during which the heart ordinarily continues to pulsate. I repeated these experiments in 1854, with like results, and was disposed to conclude that the mere absence of air certainly lessened the activity of the exposed heart. Observing, however, the laboured nature of each pulsation, as well as the swollen and distorted appearance of the heart in vacuo, it appeared to me that other elements were affecting the heart besides the mere absence of aërial stimulus. In despite of every precaution the exhaustion causes bubbles of air to appear under the pericardial surface, and evidently has a great mechanical effect upon the organ, producing blotches and dark stains, as of effused blood, upon the surface of the heart.

To learn, therefore, whether the effect is due only to the absence of atmospheric air, or in part to other causes, I suspended the hearts of several frogs and snappers in bland fluids, as oil, glycerine, and water at various temperatures.

Experiment.—The heart of a large turtle (wt. 12 lbs.) was removed with care. Suspended in air it beat, during four consecutive minutes, thus, $5\frac{1}{2}$, 8, 8, 8. In vacuo, as complete as it could be made with a not very perfect pump which had no gauge, it beat, per minute, 3, 2, 3, 2, 2, 2, 1, 1, 1. The air being let in it rose to 3, 6, $7\frac{1}{2}$, 7, 7, 7. Without moving the heart, a small glass of water, at 66°, was carried up from below, so that the suspended heart rested in it; or, as in this case, being weighed down by a half bullet, sunk below the surface. The heart instantly began to beat, strongly at first, and then feebly, its movements becoming more numerous, as thus:

1st m. 9; 2d m. 11; 3d m. 14; 4th m. 14; 5th m. 21, and very feeble. Two minutes elapsed without further movements, until being removed to the air it beat 11 the 1st minute, when the heart was placed in water at 113° F. Its pulse rose at once to 44, 1st minute, and 60, the 2d minute; then beat convulsively, and suddenly ceased. In air it again pulsated after several minutes, beating 5 regularly. The water at 113° F. again partially renewed its motions, which, however, were only 8 per minute, and altogether auricular, continuing through but four successive minutes.

Similar results attended nine experiments upon the effect of water at the temperature of the air, and at 113° F. In every case the water at aërial temperature quickened, and at last enfeebled and checked the heart's action. Water at 113° F. enormously accelerated the heart's movement, instantly sending it up to 20, 30, or even 88 to 120 pulses per minute, and always more or less convulsing its action. It is worthy of notice that, whether the heart be checked by hot or cold water, it is still susceptible of being restimulated by suspension in alcohol or by mechanical irritants.

I have made no similar experiments with mammalian hearts, but I have obtained precisely the same results with the large heart of the sturgeon, as is illustrated in the following experiment:—

Experiment.—The heart of a sturgeon, nearly five feet long, was removed as rapidly as possible, and allowed to expel all the blood from its cavities. It was then suspended in water at 70° F., the temperature of the air. After extraction it beat 33 per m. The water almost immediately accelerated its rate to 60 in the half minute, when it suddenly ceased to beat. Removed from the water and inflated with air it as suddenly began to act again at the close of twenty minutes. The air was then let out, and the heart plunged anew in water at 113° F. Several rapid pulses took place, and it ceased instantly. The effect of alcohol was not essayed.

From these experiments the power of water to stimulate the heart in certain animals is very well seen. The practical physician will at once be reminded of the stimulant effects of a too watery blood upon the human organ.

The action of an elevated temperature upon the heart is also of some interest.

The most important deduction, however, is the constant fact, that when the heart ceases to respond to one stimulus it may still be alive to others. This was further developed in very numerous experiments upon the effect of carbonic acid, nitrogen, ether vapour, and the vacuum, on the heart. Thus, the heart which ceased to act in vacuo again pulsated in air or water, and, having ceased to act in water, was still awake to the stimulus of alcohol. Even after this the muscular fibres, and sometimes the whole auricle, could be restimulated by the knife or a sharp tap. After stimulation by ammonia the heart could not be excited by any irritant. This fact, as well as the directly stimulating power of ammonia on the heart, are well seen in the following experiment:—

Experiment.—Temperature of air 70° F. A frog's heart beating steadily 27 per minute, was suspended over strong ammonia water. The organ instantly multiplied its pulsations, and, during six successive minutes, performed as follows: 44, 28, 32, 58, 36, 34, and suddenly ceased to act. Alcohol and mechanical means alike failed to re-excite its movement.

It was clear from the experiments stated that water at all temperatures affected the heart. My ultimate object was to make for the heart a practical vacuum by plunging it in a fluid which should not disturb it mechanically, and should simply shut out the air, and itself exert no stimulant influence. As pure water did not fulfil these ends I resorted to a solution of gum Arabic in water, sp. gr. 1.036.

Experiment.—The heart of the frog used in this experiment beat when taken out but nine in the minute. In fact, the hearts of frogs beat so differently as to number that it is always necessary to wait until the beat becomes regular through several successive minutes. The heart in question was plunged in a solution of gum, 1.036, temp. 70° F. It continued to pulsate nine times per minute during twelve successive minutes. A second heart furnished a result very nearly similar, the heart making fourteen beats in air and seventeen in gum water.

Pure glycerine was found, quite unexpectedly, to exert a depressing effect upon the heart. The results obtained may be briefly stated as follows:—

Experiment.—The heart of a frog in air, temperature 72° F., beat in successive minutes 36, 34, 30, 32; in glycerine 10, 14, 14, 13, 14, 14, and so continued to act unchanged for one hour, when it ceased to move. In a second experiment the heart beat unchanged in glycerine, but ceased to act at the close of seventy-eight minutes. Like results were observed in four similar experiments, so that the first one stated must be regarded as exceptional.

Glycerine, at a temperature of 100° F., acted as follows: A frog heart in air, at 71° F., settled to a regular pulse of thirty-four per minute. In glycerine, at 100° F., it beat in successive minutes 40, 52, 55, and ceased to act at the close of three minutes.

Glycerine at 32° F. rapidly depressed the heart's action. A heart beating forty in air at 70° F., fell at once in glycerine at 32° F. to 22, 21, 14, 14, 6, in successive minutes, and then stopped permanently. Several other experiments gave the same result, the heart rarely acting longer than five minutes.

Of all the agents used, olive oil gave the best results.

A frog heart, beating nineteen per minute at 72° F. in air, was suspended in neutral olive oil at 72° F. It underwent no change as to number or force for two hours, when it beat more feebly, and ceased to act in three hours and twelve minutes. In two additional experiments the heart gained one or two pulses per minute in the oil, continuing in one case for one hour and four minutes, in another for four hours and a half. Olive oil at 32° F. enfeebled the action, but did not affect the number of pulses, which could be readily observed during twenty minutes, when they ceased abruptly.

The effect of olive oil at aërial temperature was most admirably shown in the following experiment, which was designed to imitate closely the mechanical conditions existent during life, and, at the same time, to cut off from the heart the stimulus of the air.

Experiment.—The heart of a large sturgeon was rapidly removed at 8.30 A. M. As very often happens, it did not pulsate until it was hung up and inflated by means of a tube tied in the mouth of the auricle, the bronchial vein being closed by a tube having a stopcock, and bent at an acute angle three and a half inches above the bulbus arteriosus. After the heart had acquired a pretty steady pulse of thirty-four per minute, a funnel with a tube six inches long was fitted tightly in the mouth of the auricle, the tube used for inflation being removed. About half way down the stem the funnel was made narrow, so that oil placed in the funnel would not descend too rapidly into the auricle. The heart beat less regularly as the air was allowed to escape; but, upon being slowly and carefully filled with oil by means of the funnel fitted in the auricular opening, the heart regained its regular pulse, the number rising to 33, 34, 36, and, finally, settling to 33 per minute, with some irregularities to which the extracted heart of the sturgeon is usually liable. The heart was next placed in oil, and weighed down below the surface by a piece of lead attached to the apex of the heart by means of a piece of silk and a fine cambric needle. Thus situated, the heart was isolated from the air, while the oil being supplied through the auricular funnel was driven through the organ, up the arterial bulb, and, finally, over the bend in the pipe, which allowed it to escape. Under these circumstances the pulsations slowly rose in twelve minutes to thirty-seven per minute, when they began to fall, and in one hour were but twenty-one to the minute. With now and then a pause of many minutes, the pulses continued for an hour longer. At this time I was obliged to leave, and, therefore, replaced the funnel with a short open tube, plunged its upper orifice below the surface of the oil, and turned the mouth of the escape-tube into the vessel in which the heart was balanced. The organ was thus enabled to feed itself. On my returning in two hours, the auricle alone was beating. Its pulsations could still be seen one hour and eighteen minutes later, their number being reduced to twelve per minute. Other occupations prevented me from carrying the observations further. At this time, the pulsations being feeble and auricular only, had lasted for some five and a half hours, and could hardly have endured at the utmost more than two hours longer. The heart of a second sturgeon removed an hour after the first one had been placed in oil, when inflated in the air, pulsated irregularly about forty per minute during eight hours, and more irregularly, and finally, in the auricle alone during nine hours longer. It is probable, therefore, that complete isolation from the air, although at first it did not alter the number of pulses, lessened their ultimate duration.

These experiments seem to justify the following conclusions:—

1st. That the hearts of the frog and turtle beat much less rapidly in vacuo, and sometimes cease to act until the air is readmitted. That the vacuum most probably retards the heart's action by the mechanical effects it induces as well as by depriving it of oxygen, since the beat in vacuo is long and laboured and the accelerating influence of the readmitted atmosphere is almost instantaneous.

2d. That mere isolation from the air, as by placing the heart in oil, does not alter the rate of the heart's movements for some time, but lessens their ultimate duration.

3d. That water at aërial temperatures stimulates the heart, and very soon causes it to cease to pulsate. That water at higher temperatures, as 100° F. to 113° F., produces much more rapidly the same results.

4th. That glycerine at aërial temperatures affects the heart but little except as shortening the time during which it continues to pulsate. That glycerine, at 32° F., depresses the heart's action, lessening the number of pulses per minute at least one-half, and soon checking its movements altogether.

5th. That olive oil at 32° F. affected the heart very little at first as to the number of beats per minute, but soon rendered them feeble, and finally stopped them, though at the close of a longer interval than was required by glycerine at the same temperature.[1]

6th. That when the heart has ceased to respond to one stimulus, however violent, it will usually remain sensitive to others apparently far less powerful.

ART. VI.—*Observations on the Colourless Blood-corpuscle.* By WILLIAM A. HAMMOND, M. D., Assistant Surgeon U. S. Army. (Read before the Academy of Natural Sciences of Philadelphia, Biological Department, February 7th, 1859. Recommended for publication, February 21st, 1859.)

THE white blood-corpuscle, whether we regard it as the first or intermediate stage in the development of the red disk, or admit that we are unable to designate its uses, is entitled to far more consideration from physiologists than has, until recently, been awarded to it. Of late, however, through the researches of Wharton Jones, Virchow, Bennett, and others, it has attracted more particularly the attention of biologists, and perhaps ere long we may be enabled to understand its use in the economy.

The object of the present paper is to aid somewhat in the elucidation of one or two points of interest connected with this body, the principal of which relates to its persistence in its normal form in dried blood.

At the last meeting of the Department it will be recollected that in answer to an inquiry by my friend Dr. Woodward, I stated I had been unable

[1] The glycerine was thinner than the English glycerine (Price's) now in use, and altogether was a much less reliable article. For this reason I do not entirely trust the results observed when using glycerine.

to detect the white globule in blood dried on a glass slide, and that I did not believe it would remain intact under such circumstances.

It is, however, necessary that I should modify this opinion, as I have frequently since perceived these corpuscles, perfectly unaltered in blood which had been dried in the manner above stated. This has been more especially the case in the blood of reptiles, the corpuscles of which, however, both white and red, differ in many essential points from those of the mammalia.

Human blood, when dried upon glass or other hard substance, seldom retains its white corpuscles intact for any length of time. If exposed to the atmosphere for a few hours they break up, and can only be recognized by the remains of their walls. There are, however, bodies to be perceived in dried human blood which can with difficulty be distinguished, at first sight, from the dry, white corpuscle, except by the circumstance that they are of no uniform size. I have always regarded them as consisting of fat, and have recently ascertained that they are perfectly soluble in ether. A fact which may readily lead to the supposition that they are the white corpuscles is, that they are distinctly granular, and present, many of them, the appearance of containing a well-defined nucleus.

That they are really not such can be proven conclusively by mixing a little alcohol or ether with blood, and filtering off the supernatant fluid. A drop of this latter placed upon a glass slide, and allowed to evaporate, deposits numbers of these granular, fatty globules, most of them about the size of the white corpuscle. They are readily soluble again in the ether or alcohol, and to an experienced eye present other points of difference from the colourless corpuscle. Nevertheless a hasty examination, or a want of familiarity with the subject, might easily lead to the inference that they were veritable, colourless blood-corpuscles.

It is asserted by Robin, and recently reiterated by Flemings,[1] that the human white corpuscle is completely broken up in the act of the blood drying. Wyman, however, denies that any such disintegration occurs, and asserts that the colourless corpuscle is even more persistent than the red.

From my own observations I am satisfied that the red corpuscle will, under similar circumstances, remain much longer intact than the white. As above stated, I have never found the latter to remain in blood dried on glass, and exposed to the atmosphere, more than a few hours, whereas no difficulty is experienced in keeping the red disk, under such conditions, for several years.

With a view, however, still further to elucidate the subject, I soaked small pieces of cotton cloth in human blood, and examined them at different periods afterwards, as follows.

At the end of twenty-four hours I washed out one of the pieces with the

[1] American Journal of the Medical Sciences, January, 1859, p. 84.

filtered serum of frog's blood. Upon inspection with the microscope, the white corpuscles were generally perceived unaltered; a few were broken up. The red disks were contracted, but regained their ordinary size and form after floating a few minutes in the serum.

At the end of forty-eight hours a few white corpuscles were still found unbroken. The majority, however, were disintegrated, but the remains of their walls were still to be seen. The red corpuscles, as before, regained their normal form by endosmosis.

After seventy-two hours had expired the white corpuscles had entirely undergone disintegration, but the red disks still remained intact.

At the end of fifteen days the red corpuscles regained their ordinary size after maceration in the serum, but no trace of the white corpuscles could be found, beyond a few irregular fragments, which were probably the remains of the cell walls.

These experiments were repeated with several specimens of human blood, and that of other mammals, with similar results. From them it is seen that the white corpuscle did not retain its normal form after the third day, but became broken up into fragments, the contents of course escaping.

In human blood, dried in a thin film and covered immediately with thin glass, to the edges of which a cement impervious to air and moisture has been applied, the white corpuscle remains in a state of integrity for a considerable period; how long I am not prepared to say at present. Certainly, however, for at least fifteen days.

As an evidence of the presence of blood, the white corpuscle cannot be regarded as affording as valuable indications as the red. As a means of discriminating between the different kinds of blood nothing of importance has yet been done to warrant our expressing a definite opinion of its value. It is not, however, probable that much can be claimed for it in this respect. Its form being the same in all animals, would require us to rely entirely on its size, which does not vary greatly among the mammalia, probably not even to the same extent as the red corpuscle. In reptiles, birds, and fish, it is much larger than in mammals, but here the form of the red corpuscle furnishes much more valuable indications.

ART. VII.—*Cases of Gangrene of the Lungs treated in the New York Hospital from January*, 1857, *to September*, 1858. Reported by B. DARRACH, M. D., Resident Physician.

CASE I. *Gangrene of the Lung preceded by a sense of Stuffing in the side, Pain, and Paroxysmal Cough; Hæmoptysis. Discharged without relief.*—James Foster, aged 33, native of Ireland, country storekeeper, resident of Pennsylvania, was admitted into the New York Hospital, July 7th,

1858, and placed under the care of Dr. John H. Griscom. Always had enjoyed robust health; never had any serious illness until the present attack. No hereditary tendency to disease or constitutional taint discoverable; temperate; habits regular.

About the beginning of last December, while at his business and enjoying ordinary health, he was seized with a feeling as if something was stuffed in the left chest, and a soreness over a spot about the size of his hand around and to the outside of the nipple. He kept about for a week, but was finally obliged to go to bed. He then had a hard, dry, paroxysmal cough. From this time he rapidly failed in strength, until some time early in the spring; then his life was despaired of. About the middle of February, after a paroxysm of coughing, he suddenly expectorated a large quantity of very offensive matter, having an exceedingly disagreeable taste, and of a dirty ash colour. He says he could feel it coming from a point corresponding to the cartilage of the sixth rib. This continued for a week, and then gradually grew less in quantity, and lost its offensive odour. He has had several attacks of hæmoptysis since (once to the amount of a pint); which generally followed a period of more than usual exertion, and were always preceded by an increase of expectoration and of the gangrenous odour. Since March, he has gradually gained strength, and is now able to go about. He still suffers from the cough, and has a muco-purulent expectoration. His limbs are slightly œdematous; hands emaciated; tissues, about the ankles, wrists, and fingers, thickened; fingers clubbed; nails strongly curved. Complains of soreness and stiffness of the joints, with some tenderness. Has a sense of soreness about the left nipple, and still the feeling of something stuffed in there. Anteriorly, the left chest is resonant on percussion, as low as the fourth rib, but very flat below that point to the seventh. Behind, percussion is dull as high up as the centre of the scapula, and laterally to the same level. Respiratory murmur over the upper part of the left and all of the right lung natural. Loud bronchial respiration over the dull portion behind. Pulse 100, of good volume and fair strength. No evidence of disease of the heart or arteries. Appetite tolerable; tongue clean; constipation. Urine highly coloured and loaded with urates. Ordered—tr. cinchon. co. ʒj, three times a day; Labarraque's sol. chlor. sodæ, ʒss every four hours. Expectorants as may be required. Generous diet, with an allowance of porter or wine, as preferred.

July 12. His expectoration began to have the peculiar offensive smell of new mortar, so characteristic of the disease, on the second day after admission; it is now very much increased in quantity and offensiveness; amounts to about a pint and a half in twenty-four hours; is of a dull grayish colour, sometimes with a pink tinge, frothy, and disposed to be flocculent; has a very disagreeable taste to the patient; his cough is paroxysmal, reiterated, and very distressing; percussion of the chest elicits nothing new; bronchial respiration has been succeeded by a total absence of respiratory sounds and vocal fremitus over all the dull portion, with an equivocal ægophony. Other symptoms unchanged.

17*th*. Ordered—inhalations of turpentine in addition to his other remedies.

August 2. Thinks his cough relieved by the inhalation; had a slight hæmoptysis last night; expectoration more offensive.

6*th*. Dr. H. D. Bulkley in charge. No trace of ægophony. Feels less comfortable. Ordered—ol. morrhuæ ℥ss, syr. ferri superphosphatis ʒj, three times a day, in place of tr. cinchona. Blister to the side.

13*th*. Marked dulness over the whole left chest, posteriorly, and also in

front, increasing from the apex downward. There is a feeble respiratory murmur as low as the seventh rib behind, and to the fourth in front; but none below that. Along the posterior border of the scapula the inspiration is very feeble, with a bronchial expiration. Over the sixth rib it becomes cavernous, with pectoriloquy. Vocal resonance on that side increased; vocal fremitus diminished, and entirely absent below; marked tenderness on percussion of that side; motion of inspiration also less than the other side; over the right lung respiration is natural; expectoration unchanged. Respirations 34; pulse 108, full, after the examination of his chest. Can perceive no difference in his other symptoms. Wished to go home, and was discharged.

CASE II. *Gangrene of the Right Lung; Cough and Pain for a year before; Paroxysms of Dyspnœa, and Cough; Gangrenous Expectoration; Recovery.*—Albert Byard, aged 39, native of New York, seaman, admitted into the New York Hospital December 24th, 1850. Placed under the care of Dr. Joseph M. Smith, but was transferred to the care of Dr. Thos. F. Cock, on the first of January following. Hereditary and previous history not obtained. Eleven months ago, while on board a United States man-of-war, in the Mediterranean, he was taken with a severe pain in his right side, and a cough. Expectoration often streaked with blood; once he raised a quantity of pure blood. Since then the pain has left him, though the cough continued. He has never been able to do duty since. Six weeks ago he began to be troubled with severe paroxysms of dyspnœa, liable to be excited by the least exertion. Eleven days ago his pain returned, after exposure to wet, and was aggravated by cough and a full inspiration.

Present condition. Has the appearance of ordinary health. Complains of the severe paroxysms of coughing. These come on irregularly; last a considerable length of time, causing profuse sweats and urgent dyspnœa, for which the patient often gets out upon the floor, in hope of relief. Expectoration moderate in quantity, frothy, and muco-purulent. Says that powdered gum Arabic has always given him the most relief. Pulse 86, and moderately full. Tongue furred and slightly red. Anorexia and great thirst.

Auscultation and percussion reveal no distinct physical signs.

Ordered—a stimulating expectorant, and gum Arabic.

January 4. On examination of the chest to-day was found dulness on percussion, posteriorly, over the right lung, and a loose mucous rale, apparently deep-seated in the chest, and extending as high as the lower angle of the scapula.

5*th*. Had an unusually severe paroxysm of coughing last night, which was relieved by the sudden expectoration of a large quantity of a diffluent, frothy, dirty grayish, and extremely offensive sputum, having the characteristic new mortar odour of gangrenous lung. His breath also has the same odour; he also tastes it. General symptoms unchanged. Ordered—Labarraque's sol. chlor. sodæ ʒss, every four hours.

9*th*. Pulse 80, and feeble; cough about the same, losing strength.

12*th*. Distinct mucous rale at the base of the right lung, posteriorly, with feeble respiratory murmur. Ordered—stimulants.

20*th*. Visible improvement since last note. Tongue, which was before dry and striped, is now more natural; pulse has more fulness and strength; dyspnœa but slight; tendency to constipation relieved by enema; appetite no better, and very poor. Expectoration less copious, more mucous, more diffluent, and less offensive. Respiratory sound audible over the right side

22*d.* Severe coughing fit last night; but expectoration less, and not near so offensive. Appetite better; pulse more force, and tongue more natural in appearance.

30*th.* Has steadily improved since last note. Appetite better; tongue clean; no dyspnœa; pulse more volume and strength; expectoration small in quantity, and without fetor.

February 4. Now slight dulness over the right chest behind; respiratory sounds feeble and prolonged. Left chest percussion natural, respiration puerile.

March 25. Patient now assists in the duties of the ward. No expectoration, and but little cough.

April 17. Feels well, but has not yet gained his full strength. Discharged cured; but remained under observation some weeks longer, and steadily gained strength.

CASE III. *Gangrene of the Lung; Recovery.*—Jacob Mitchell, aged 35, native of New Jersey, seaman, admitted to the New York Hospital, March 16th, 1857, under the care of Dr. Thos. F. Cock. No previous history of the patient obtained. Was taken with a cold three weeks ago, from which he soon recovered, excepting the cough. This in a short time was accompanied by a fetid purulent expectoration. The cough was most frequent at night, in paroxysms, causing great distress, and preventing sleep. He has also had attacks of hæmoptysis daily, or every other day, for some time past. Is somewhat emaciated; appearance dull, with a yellow, dusky hue of the face. Intelligence perfect. Pulse 78, full, but very compressible. Appetite poor; tongue coated; bowels inclined to constipation. Percussion dull below the right clavicle; no perceptible difference behind. Still complains of the cough; sputa frothy, of a dirty grayish colour, and very offensive odour. Ordered—Labarraque's sol. chl. sodæ ʒss, *ter in die,* ol. morrhuæ. Generous diet and stimulants.

April 25. Fine, deep-seated crepitus noted over the posterior part of the right chest.

August 26. Has had numerous attacks of hæmoptysis since last note. The patient at one time became very low, and required the free use of brandy. His expectoration has varied somewhat in quantity and odour; it is now very much diminished and has lost its fetor. The pain in the side noted June 6th, for the first time, has also disappeared. His appetite and strength have very materially improved. Chlor. sodæ suspended.

September 3. Slight increase of sputa, with a slight gangrenous taste. Some pain in the side. Chlor. sodæ resumed.

24*th.* Has occasional cough and expectoration. The right side is markedly flattened anteriorly, and dull upon percussion throughout. The shoulder is depressed and the expansion of this side much less than that of the left. Respiratory murmur very feeble; left side clear. Feels much better, and quite strong. Discharged.

Two other cases occurred in the hospital the same winter.

One, had the characteristic cough and sputa, but was able to go about the whole time. Recovered.

The other was admitted for supposed malaria cachexia. Could give no history of himself. After lingering for about six weeks, he died. On *post-mortem* examination a large cavity was found in the lower lobe of the left lung, surrounded by indurated gangrenous walls. There was also fatty degeneration of his kidneys.

ART. VIII.—*Case of Fracture at Base of Cranium, with discharge from external ear of a portion of the substance of the brain, of watery matter, and of blood in large quantities; Recovery.* By JOHN A. LOCKWOOD, M.D., Surgeon U. S. Navy. (Communicated by W. Whelan, M. D., Chief Bureau Med. and Surg. U. S. N.)

JOHN SMITH, third ordinary seaman, aged 30, on the 19th September, 1855, on board the U. S. ship Constellation, at sea, fell from the spar to the main deck, down the fore-hatchway, striking plumply upon the crown of the head. When examined, there existed insensibility, a very feeble pulse, foaming at the mouth, stertorous breathing, copious flow of blood from right ear, and hence from the nose. Three hours subsequently the pulse had rallied; he was capable of being aroused; no epistaxis; bleeding from ear diminished; a small quantity of brain had come away with the blood from right ear. The amount of blood which escaped from the ear during the first hours was very considerable, estimated at fifty or sixty ounces. Cut cups depressed the pulse; a stimulating injection was retained for several hours, acting freely when consciousness was restored. During the night discharge of blood and serum continuous, and comparatively moderate. Reposed quietly.

September 20. Inclined to be comatose; aroused with some difficulty. Moderate discharge of serum or watery matter, coloured with blood. Small particles of brain taken from the meatus, and carefully examined by Assistant Surgeon Wyatt M. Brown and myself.

21*st.* Disposed to sleep. When not asleep, restless, and wishes to walk about; wants his grog; no lack of muscular power; mind wandering; complains at times of pain in head; pulse natural; coloured discharge as before.

22*d.* Intellect clearer; more headache; discharge continuous, but no longer discoloured.

23*d.* Mind quite clear; during night renewed discharge of blood from ear and nose.

24*th.* Violent pain across the forehead; watery discharge.

25*th.* Severe suffering from headache during the night; this morning succeeded by a sense of fulness; watery discharge as before.

26*th.* Less pain; sense of fulness instead; watery discharge as before.

October 23. Returned to duty. Still complains of a ringing in the ears, and a sensation described as a "stoppage in the head." He is quite robust, and otherwise in good health. The watery discharge from the ear did not entirely cease for nearly a month. At one time it was sufficiently copious to wet disagreeably during sleep his pillow, and necessitated plugging the

orifice of the ear before going to bed. For a short period the discharge was yellowish, and of an offensive odour, probably from an internal abscess.

Agreeably to Smith's own statement, he had been epileptic since 1850. He had experienced two attacks since joining the ship, seven weeks before the accident, and it was on the incursion of a third that he fell through the hatchway. After the injury, he escaped with a single fit during the succeeding three months. The following spring, he was discharged the navy, and I have not been able to trace his further history.

The diagnosis in this case I regard as unmistakable. The very copious flow of blood from the ear, the discharge by the same outlet of watery matter, and of a portion of the substance of the brain itself, proves that a fracture of the skull at its base (produced by a *contre-coup*, when the full force of the fall was sustained by the vertex), had involved the petrous portion of the temporal bone.

Surgical writers are agreed in ascribing the gravest prognosis to this injury, when accompanied by a watery discharge. (RANKING'S *Abstract*, vols. ii. and iii.) Robert, one of the authorities on this subject, declares it to be a fatal symptom. Ericson mentions a single case of recovery. I am not aware of any such result being on record, where a portion of the substance of the brain escaped by the external ear.

ART. IX.—*Modified Extension Apparatus for Fractures of the Leg.*[1] By ALONZO CHAPIN, M. D., Member of the Massachusetts Medical Society.

SOME fifteen years ago I was called to attend a man in the country, who had fallen with a scaffolding and broken both bones of the leg—the tibia and fibula. The fracture of both was oblique, and the management required a considerable extending force to bring the bones into coaptation, and to keep them there. The man was irritable, too, and impatient and troublesome to control. As is often the case with country practitioners, it was necessary to extemporize an arrangement which would suit the case. Desault's extension splint did not suit me for a model. The counter-extending force being dependent on a single perineal strap, gives an oblique inclination to the pelvis, does not act in a line of the axis of the limb, allows of more rotary motion of the foot than is sometimes admissible or

[1] This apparatus was exhibited to the members of the Middlesex East Medical Society at its meeting in March, 1858, and afterwards presented to the consideration of the Fellows of the Massachusetts Medical Society at its annual meeting held in Boston in the month of May following.

comfortable, and furnishes inadequate facilities for producing and keeping up the exact amount of extension needed.

The modification by Dr. Physick, in extending the splint to the axilla, though diminishing some and removing other objectionable features of Desault's apparatus, seemed to me to possess an additional and somewhat serious objection in the confined and constrained position which it must exact of the patient's body.

The apparatus contrived by Dr. J. F. Flagg, of Boston, suited me better. It is quite a different modification of Desault's splint, was described by him in the *New England Journal of Medicine and Surgery*, for January, 1821, and was afterwards copied into Sir Astley Cooper's *Treatise on Dislocations and Fractures of the Joints*, the edition published in 1844, by order of the Massachusetts Medical Society, at page 196. It has been in use in the Massachusetts General Hospital for a good many years. It consists, in addition to the single outside splint of Desault, of a second shorter, placed along the inside of the leg, its upper end well cushioned and resting in the perineum, and extending to the same distance below the foot as the outer splint. Both splints also pass through mortises in a cross-piece some six or seven inches apart. This cross-piece may thus be moved up or down, to suit occasion, and is kept in place by pegs stuck through the splints. "A wood screw passes through the centre of the cross-piece, on the upper end of which is an iron ring attached by a swivel. Through this ring are passed the bands from the foot or knee, with which the extension is made. This apparatus performs the work of extension and counter-extension well, and I prefer it to any other which I have seen described. The splints cannot, however, be separated from the leg to adjust the dressing without removing the cross-piece, and thus for the time relaxing the extending force, at an inevitable risk of displacement of the fractured bones.

I, therefore, modified Dr. Flagg's modification by making the mortise holes in the side splints, instead of in the cross-piece, and constructing the cross-piece with a long tenon on each end to pass through the mortises. Thus arranged, the splints may each be moved aside a sufficient distance from the leg, the lower ends being guided and the cross-piece kept in place by the long tenons, without relaxing in the least the extension power. It will be understood that the splints are not to be removed *from* the cross-piece, but to be slid along the tenon so far as is necessary to arrange the dressing. Instead of a single wood screw, two metallic screws are used, each piercing the cross-piece in such positions as to receive a loop or band from either side of the foot. The loops may be attached to a gaiter or bandage on the foot, or to adhesive straps lying along the leg. A screw on each side makes extension with less compression of the foot than where only one, in the centre, is used; and there may sometimes be an advantage in keeping up extension with one screw while adjusting the band attached

to the other, or while relaxing it for the relief of the other. Instead of the ring with a swivel attached to prevent twisting of the band, a hook may be preferable, which is most convenient for hitching a loop; and a nut applied to the screw on the bottom of the cross-piece extends or relaxes without causing any twist, as the screw is not itself turned at all in the operation.

Fig. 1.

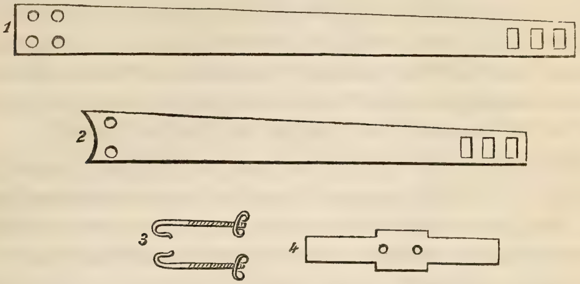
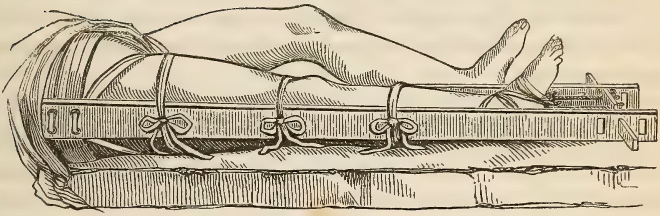


Fig. 2.



The above figures represent the apparatus—in its several parts, and as applied to the limb.

Fig. 1, 1. The long splint. It should extend from the crest of the ilium to six or seven inches below the foot. For a man of ordinary stature to be about four feet long. To be four or five inches wide at the upper end, three inches wide at the lower end; with holes near the top for tying the bands, and mortises as represented near the bottom for a tenon of the cross-piece.

2. Short or inner splint, six inches shorter than the outer, made concave at top to fit the perineum and well padded; in other respects the same as the long one.

3. The screws each six inches long; may be made of large iron or brass wire.

4. Cross-piece, fifteen inches long, three inches wide; tenons each five inches long, and of width to fit the mortises.

The apparatus has thus far seemed to answer all ordinary indications. The great improvement claimed for it, is the convenience of being enabled to move the splints off from the limb sufficiently to rearrange or change the dressing, without danger of displacement of the bones; a circumstance of much importance, both to the patient and to the surgeon.

It is, too, an admirable adjuster—furnishing so ready and powerful an extension force that the apparatus may be at once applied, and the lapping bones, gradually and without much pain to the patient, be brought into

position; thus enabling the surgeon to dispense with the services of an assistant in *setting the leg*—a circumstance, too, often of much convenience and importance.

It also serves as a fracture-box for the repose of the limb. Wadding and cushioning may be applied, *ad libitum*, between the splints and the leg, or it may be removed whenever it becomes too much heated. Rotary motion of the foot is prevented, thus obviating one objection to the single splint, and it is less confining and less irksome than when extended up to the axilla. Excoriations may easily be guarded against. The ischium and perineum may be relieved of pressure by counter-extending adhesive bands tied through the upper ends of the splints, and pressure on the foot and ankle may in like manner be avoided by using adhesive bands.

It is simple, too, in its construction, and may be made by any carpenter to suit occasion, or by the surgeon himself, if he is accustomed to the use of tools. This is no small recommendation in a country town, where the practitioner is so often thrown on to his own unaided resources.

The apparatus described is more especially calculated for fractured thigh and neck of the bone. I found it work well for oblique fracture of the bones of the lower leg. Other forms of splint may be preferred for the latter; but where simplicity, economy, and ready availability are sought, this has advantages.

Very many of the advantages claimed for this apparatus over most others in use, may also be claimed for Flagg's modification. Good authority has declared his "a great improvement upon Desault's," and "incomparably better than the method pursued by Sir Astley Cooper, in a case of fractured thigh." (See his work published by Massachusetts Medical Society, page 219.) And no higher recommendation need be sought for it than the statement that it has for so many years been continued in use in the Massachusetts General Hospital, and was used and approved by the late distinguished surgeon, Dr. John C. Warren. *The modification made by himself increases its efficiency, and thereby increases its value.*

The man for whose special benefit it was contrived, was, as before stated, irritable, and morose and impatient under restraint. And besides, his accident occurred in the heat of summer. He, however, got along well, could move his body about somewhat. The bones could, with gratifying facility, be kept in place by simply turning the screws whenever the bands became relaxed; very little padding was needed, thus allowing the limb to be kept cool; and in twenty-eight days from the occurrence of the injury the splints were removed—the limb perfect in *length* and *shape*, and it afterwards exhibited no appreciable evidence of the injury it had sustained.

ART. X.—*Observations on the Isthmus of Panama, and on the Hospitals of Havana.* By G. R. B. HORNER, M. D., Surgeon U. S. Naval Hospital, Warrington, Florida. (Communicated by W. WHELAN, M. D., Chief Bureau Med. and Surg. U. S. N.)

THE discovery of gold in California, ten years since, has vastly increased the transit of persons, merchandise, &c., across the Isthmus—greatly augmented the importance of this strip of land, and, indeed, rendered it the great highway of nations. The most important portion at this time is the western, or most northerly of the Republic of New Grenada, which forms nearly a semicircle. This is bounded on the east by the Caribbean Sea, west by the Pacific, and north by Nicaragua and Costa Rica. The narrowest part of the Isthmus lies between Panama and Chagres, a small town at the mouth of the river of that name, and about forty miles from the former town ; but the latter having a small, insecure harbour, Aspinwall has been built on the island of Manzanilla, in Navy Bay, some miles south of Chagres, and a substantial, well-graded railroad is constructed between the two places. But this was accomplished at the expense of more than a thousand lives, chiefly those of Chinese labourers. Miasmatic fevers were the principal causes of the mortality. Many, however, died of neglect and bad treatment. When a Chinaman got sick, it is said, it was common to lay him down in the woods, with some food and water by his side, and let him lie there until dead, or well enough to get away. Other patients were sent down to the hospital of the railroad company at Aspinwall, from whence every two weeks about two hundred were sent to New York, so that the precise mortality among them was not ascertained.

The railroad runs through several extensive morasses—densely overgrown with palms, wild cananas, bushes, vines, and large timber—strikes the Chagres River where the Gaton empties into it, runs up the right bank of the former, crosses it at Barbacoas by an iron bridge, passes up to Gorgona twenty-eight miles from Aspinwall, thence through a valley, bounded by several thickly wooded, very verdant mountains, and attains the summit level, which is only 487 feet above the level of the sea. From there the road gradually descends to Panama, through the valley of the little stream termed Rio Grande, perhaps in ridicule. Last year a party of men and officers from our squadron, headed by Com. Paulding and Col. Totten, chief engineer of the road, crossed the Isthmus by the railroad, closely inspected the route and bay of Panama, and reported the feasibility of a ship-canal along the same route. The colonel estimates the cost at $70,000,000 for one 300 feet wide and 30 feet deep, with several locks in its course, and a breakwater at Aspinwall. The greatest obstacles to be encountered are the scarcity of native labourers, and the diseases incidental to foreigners, especially of the white race.

The bay of Panama and its numerous islands might afford anchorage for all the ships of the world which could be brought there; and the bay of Aspinwall could accommodate as many as would probably ever be collected in it, being two miles wide and three long; but being exposed to the north during the winter, when the wind blows strongly from the sea, it would require at least one very long breakwater, projecting into it from the eastern side of the entrance. All the Isthmus traversed by the railroad, and visible

from any part of it, is hilly and mountainous, has a diversity of soil, abounds in several varieties of basaltic rocks and volcanic remains, and is very fertile naturally. Its vegetable productions are numerous—and among them are yams, maize, cassana or tapioca, coffee, cananas, plantains, palm-nuts of various sorts, including the cocoa and cabbage, which affords much oil. The mangoo, lime, orange, small cheremaya, a large yellow species of annona, weighing sometimes three or four pounds, and belonging to the same class of plants as the former; the sour sop, abbacata or calligator pear, and the pawpaw as large as a musklin, are likewise produced; but the orange, from neglect in cultivation, is very rare. Indeed, so poor is agriculture throughout the country, that much more of all its products is due to nature than to its inhabitants.

Papyrus abounds in the swamps; some species of cinchona are said to grow in the forests, and the cedron-bean (used as an antidote for poisoned wounds) on a small tree below Panama. Many species of cactus and of parasitical plants are found; and the latter are seen covering the monarchs of the forest, feeding upon their juices, until the stoutest trees die and crumble to dust. In time the parasites decay; those which had formed the longest vines, sweeping from the highest branches down to the ground, fall to pieces, nourish it, and help the growth of the young trees springing up in place of those destroyed. Near the sea-shore the mangrove, as if on stilts, elevates itself upon its limb-like roots, and spreads its branches high above the water, preventing all access ashore to every kind of vessels.

Both along the shores of Panama and Aspinwall, coral is abundant. Many reefs are formed of it, some are old and dry, others new and fresh; and the former, when crumbled, mixed with decomposed shells and sand, forms a very productive soil for some vegetables. Some of the finest land is upon the sides of the Chagres River, which arises in the eastern part of the Isthmus, winds along to the westward, then to the northward, just before it reaches the railroad and Gorgona; is navigable from Cruses, seven miles above, and averages about seventy feet in width, as far as Gaton. From thence it increases in size, until it reaches the town of Chagres. The banks of the river are bold, overhung with shrubs, vines, and trees, and produce the sugar-cane luxuriantly.

The island of Manzanilla is on the eastern side of Navy Bay, about two miles around; it is formed chiefly of sand and coral, and in no part exceeds eight feet above high tide. Near its centre is a swamp flooded with water, and a large pond is left near the middle of the town, which was formerly a cove; but has been converted into its present shape by being filled with earth next the harbour. This pond is nearly encircled by houses, receives the overflowing water from the swamp, and communicates with the harbour, beneath the railroad; but is a mere receptacle of filth, and mostly stagnant. The people of Aspinwall are, therefore, at all seasons infested with miasmatic fevers.

The islands in Panama Bay are numerous; some are large, all are rocky, high, and picturesque, and mostly composed of sandstone, extensively used in building. Tobago is the principal island, and is several leagues around; it contains some hundred people, produces the pine-apple, some cocoa, tamarinds, cocoa-nuts, and other fruits; and has the iron-works of the English Steam-Packet Company on a promontory, projecting towards Panama. The population of this is about 6,000, that of Aspinwall about 1,500, and both are principally inhabited by negroes, mestizos, and mulattoes. The

greater part of the former in Aspinwall are manumitted slaves from Jamaica, who subsist by working for the railroad company, and by peddling and shop-keeping. They drink to excess, and are devoted to music and dancing.

The animal kingdom of the Isthmus is nearly as rich as the vegetable. The waters teem with fish; insects, reptiles, birds, and quadrupeds abound on shore. With hooks and seines are caught the red rockfish, the cavallo, a large species of mackerel, the croaker, gar, perch, and parrot-fish. The last takes its name from its being as varied in colour as that bird, and tinged richly with green and blue. Mullets, porpoises, the large green turtle, crabs, and sharks likewise abound. The turtle affords a very large part of the meat used at Aspinwall; the cattle killed there, from being driven far and illy fed, are poor and insipid. A few sheep, some hogs, goats, fowls, turkeys, and ducks serve also for sustenance to the richer classes; but the poor live chiefly on yams, plantains, bananas, and other vegetable food.

Among the wild animals of the Isthmus are the tiger, wild cat, mamoss or ant-bear, the deer, several species of monkeys, the alligator, and zæcæ, a large species of hedge-hog, of a brown colour, and having dark stripes on his back. Alligators abound in the Chagres River and other watercourses, on the banks of which they catch fish, and dogs, and within a few years have killed several persons.

Climate.—This is always sultry, never really cool or dry. The thermometer averages from 80° to 85° during the day, when the wind is blowing towards the land; but when off of it, as it generally does after 8 o'clock at night, it falls frequently to 76°, sometimes to 74°: and this from the free perspiration and sensibility of the skin, imparts such a sensation of coldness as to make woollen clothes pleasant. At Panama the heat is sometimes above 90°, and is more oppressive than at Aspinwall, where, in spring, summer, and fall, the wind mostly blows from the sea during the day, and from the land at night. During the winter it blows commonly from the northeast in the day, and causes a heavy swell in the bay. In the summer season it blows occasionally in squalls from that quarter, and is so stormy as to endanger ships at anchor there. After a tempest in winter, some years ago, so heavy a swell occurred, that three or four vessels were wrecked on the eastern shore of Manzanilla. One broke through the high, strong wharf of piles and planks, owned by the United States Steam-Mail Company. Another vessel was driven upon a reef opposite the wharf, broken to pieces, and wrecked on the adjacent shore, where she still lies. The range of the barometer is small. It rarely rises above $30\frac{1}{4}$ of an inch, and very seldom falls below 30 inches, so that it is not considered at Aspinwall or vicinity a good indicator of the weather. Rain falls there in showers, ordinarily during the afternoon and at night, for nine months, at intervals; but rarely from the first of December to March. During last fall we had rain, more or less, and usually after meridian, for six days out of seven. During that period the atmosphere was saturated with moisture; clothes, books, and sails were spoiled with mould; small mushrooms sprang up in quantities on the tarred ropes coiled on deck, and especially on those not exposed to the sun. The rusting of all metallic substances liable to oxidation occurs correspondingly to the moisture of the atmosphere, rendered still more corrosive by the evaporation of sea-water, charged with chlorine and saline particles.

Diseases.—The most prevalent diseases among the inhabitants may be said to be those of the bowels and miasmatic fevers; but from these they

are exempt in comparison with strangers from North America and Europe. All of these, who are white, are certain in a short time to be affected with some form of those fevers, remittent or intermittent, of the quotidian or tertian type. These also, I was informed by Dr. Moore, of Aspinwall, preceded the yellow fever there, as has been observed in other countries; although it is stated by Dr. Hammond, U. S. Army,[1] to have followed the epidemic yellow fever, at Warrington, Florida, in 1853, when it was introduced by some recruits at the navy yard, and infected the inmates of the hospital. But the crew of the Wabash were remarkably exempt from bowel complaints and miasmatic fevers. This exemption we ascribed to the men drinking exclusively the pure rain-water collected in the vast boiler-iron cisterns of the United States Steam Navigation Company, which are filled from the roofs of their buildings, and hold many thousand gallons. The people of Aspinwall also use rain-water altogether, as they have no wells, springs, or streams convenient. That our crew's exemption from diarrhœa and dysentery was owing to this water, is proved by the case of an English ship of the line, which, I understand, had many cases, after using the water of the river Mindi—a small stream emptying into the south side of Navy Bay; and some cases of diarrhœa occurring in our crew, from the rain-water being sometimes made saltish by its being brought in open boats, which had taken in some sea-water, whilst being conveyed from shore during heavy swells. Our comparative exemption from fever was owing mainly to the Wabash keeping at a distance from shore, to being thoroughly cleansed, and having her crew as little as possible exposed to the sun and malaria. Few of the men were permitted to land. But many suffered from boils, lichen tropicus, and various herpetic eruptions; some from rheumatism and neuralgia. During the last of August, 1857, about one-half were affected with influenza, which first attacked the crews of our ships of war at Panama, next the people of that town and Aspinwall, and was last heard of at the island of St. Thomas, 800 miles to the eastward. Besides the above, we scarcely had any diseases of the respiratory organs to treat—not a case of phthisis occurred in many months; but one of pneumonia, attending an attack of remittent fever with hepatitis, proved fatal last October. A man on board the Saratoga likewise died of pneumonia at Greytown, 300 miles north of Aspinwall.

The cutaneous affections were very numerous and troublesome to cure. Wounds and ulcers were indolent, and so irritable that the metallic lotions, as of sulph. of zinc, nitrate of silver, and acetate of lead, appeared poisonous, especially when applied to the privates; wounds and ulcers, moreover, often bled profusely; and in the treatment of the numerous cases among the passengers from Gen. Walker's army, brought to the United States, no dressings would retain the blood. This was dark, thin, and plainly venous. For the cure of ordinary prickly heat, the application of diluted aqua ammoniæ was successful; but when it became scabby and formed the regular lichen tropicus, the warm-bath, citrine ointment, and a solution of five or ten grains of argent. nitras were commonly used efficaciously; sometimes the flowers of sulphur or other laxatives were given. In the cure of ulcers I conjoined local and general remedies, and in that of boils I at first applied poultices of powdered linseed; but they often increased the size of the boils, were inconvenient to make, and consumed a large amount of muslin. I substituted, with advantage, the pure nitrate of silver, before or after

[1] See the Army Medical Statistics, lately published.

lancing. In the treatment of miasmatic fevers, I used saline purgatives, mercurials, the acetates of potash and ammonia, acidulous drinks, hot and cold water bathing, and the sulph. of quinine largely, in solution with the elixir of vitriol and white sugar. The complicated case above mentioned was the only one which terminated fatally. Although the yellow fever was at Havana twice, while our ship was in the harbour not a case of it occurred in her; I saw many, however, in the mercantile and great military hospitals in its vicinity, which had been brought there from the vessels and the different posts about the city. The former hospital is on the eastern part of the harbour; and near the suburb of Reglus is a private establishment, rented and kept by the two Drs. Belot. The latter hospital consists of two distinct buildings, some distance apart; but only one of them was occupied when I was there. The cases of yellow fever seen were principally convalescent, and two had been attended with black vomit. One was that of a native boy, the other that of an Englishman, and one American had died of the fever the day before. Two dollars a day were charged for each patient, and twenty dollars for every one buried. 1331 cases were treated last year, of which from 20 to 22 per cent. ended fatally; but in November, 44 out of 202 died. One of the Drs. Belot stated to me that the yellow fever was infectious, atmospheric, and epidemic, but not contagious like smallpox; that it was always worse in the harbour among the shipping; he pointed out a vessel in which a number of cases had happened, two of which were fatal; but at the same time showed me an American ship, which had been in port for two months and a half, without having had a case on board.

The worst cases I saw were in the military hospital, which stands on the western side of the harbour, near the beach, between it and the new part of Havana. This hospital was built in 1842, is of marble from the Isle of Pines, about 300 feet wide and 600 long, two stories high, forms two large courts, filled with flowers, trees, and plants, and in its arrangements, conveniences, and attendance, was not excelled by any other hospital I have seen in any part of the world. The wards were well planned, all opened into the galleries encircling the courts; contained 800 patients, attended by twenty physicians, a half of whom were residents, and nursed chiefly by twenty-three sisters of charity. The wardrobe, dispensary, laboratory, and kitchen were spacious and well furnished. The cooking was done in fine iron ranges, burning coal. Infected clothes were purified by fumigation from a furnace in a room appropriated for that purpose. Another one is converted into a chapel; a third one contains a library, mostly of French works, and some handsome anatomical preparations. Convalescents eat in a back portico, overlooking the harbour. The food is neatly cooked, and principally consists of rice, bread, soup, beef, and chicken.

Sick and wounded seamen are in separate wards, and not mingled with the soldiers. All patients lie on iron bedsteads, with linen sacking bottoms, save a few on cots. Every bedstead is supplied with two sheets, one blanket, and a pillow. Between every two beds was a close stool, behind a curtain, hanging from a semicircular rod of iron, fixed at its ends into the wall. Last year, during the prevalence of the yellow fever, the hospital contained 1200 patients per day, of which number 250 were of the above fever. It was then necessary to use the verandas as wards, and they were so wide as to hold the beds crosswise, and yet to allow persons to pass between them and the courts. In a ward containing fifty-five beds, I saw a number of cases of yellow fever indiscriminately mixed with those of other complaints, and was assured by the resident physician who attended the

ward that the fever was non-contagious, and very rarely infectious in the hospital. The cases were in the primary, middle, and last stages. In the first stage there was headache, injected eyes, hot skin, frequent pulse; tongue moist, and covered with a thick, white fur, and red about the edges. Those in the middle stage were more severely affected, and more jaundiced; but in none did I find the pulse very full and strong. One patient pointed to a tub of water, and said he had just thrown up blood. Two other soldiers, in the last stage, were most distressing objects. Their eyes and skin were of a saffron hue; their faces haggard and stupid; their mouths expanded; their tongues dry, brown, and hard; one of them had hiccough; blood stained their lips; the pulse was small, weak, frequent; the skin cold and dry. The bed of one was deeply stained with dark blood, and this was sputtered also on the floor at the side of his bed, and that of another young, dying Spaniard. No painter could find two more horrible pictures of death to copy.

Treatment.—There was some difference in this at the private and public hospital. In the former, the first medicine given was the sulphate of magnesia, in doses large enough to purge, for several days. They were aided by cathartic enemata; cups and blisters were applied over the epigastrium, when nausea and pain in the stomach occurred. Cups were also applied to the nape of the neck for pain in the forehead, a striking symptom; blisters were sometimes placed upon the legs, and, when black vomit supervened, Dr. Belot gave small doses of astringents—ratanhia, alum, and acetate of lead, with some opium. His patients were given for drink and nourishment, chicken-broth, rice, and farina.

In the military hospital the primary treatment was likewise purging, with the sulphate of magnesia; but it was given in the dose of ℥j with gr. j of tart. emetic, and then followed by the administration of other internal remedies, chiefly astringents, of which the principal was the tinctura ferri chloridi, given agreeably to circumstances. Of the comparative success of the treatment at the two hospitals I was not informed accurately, but understood it was about the same.

The practice on board American vessels, of giving calomel, jalap, and then castor oil, the above physicians condemned as injurious. Several hundred of their seamen died last year. One was deprived of her whole crew, and part of two others, in a fortnight, according to the statement of our consul; and yet the practice does not seem worse than that in the Spanish men-of-war at Havana, which, the admiral of the port informed me, "lost at the above time 900 seamen out of 4,000, of the vomito," as the fever is there termed commonly, a mere symptom and effect being taken for the disease itself. Dr. Belot and other intelligent physicians regard the vomit as a mere mixture of blood, bile, and gastric juice. But although he thinks the fever non-contagious, there are many proofs to the contrary advanced by other eminent physicians; and we might concede, at least, that sometimes typhus fever might be conjoined with it, as it is believed it was at Pensacola and Warrington, in 1853, and caused it to spread from ship to shore, and then to spread from person to person at Fort Barancas and the Naval Hospital, among patients already there with other complaints, and likewise among the attendants.

TRANSACTIONS OF SOCIETIES.

Art. XI.—*Summary of the Transactions of the College of Physicians of Philadelphia.*

1858, *Sept.* 1. *Intermittent Lochia.*—Dr. Corse read the following account of a case of this affection:—

I attended Mrs. J—— in her confinement, which presented no unusual symptom; the delivery was effected in what is usually considered to be a favourable manner. After delivery, the secretion of milk was established on the third day, with a slight chill and a smart fever, familiar in the lying-in chamber under the name of milk fever. Upon its subsidence, the breasts filled with milk, and lactation was comfortably established.

Coincident with the gush of milk, the lochia underwent the usual diminution in quantity and change in quality from deep red to pale; and in the course of four or five days the sanguineous character had almost disappeared, leaving the usual straw-coloured or yellow discharge. On the eighth or ninth day, at 9½ o'clock A. M., the patient was seized with great pain in the lumbar region; this, in the course of half an hour or a little more, extended down the sacrum to the coccyx, and then a gush of blood or bloody discharge took place, which continued to flow as long as the pain lasted, which was nearly an hour; it then ceased with a subsidence of the pain.

I was immediately sent for, but did not reach her until after the pain had left and the flow ceased. The pulse was good, tongue clean, and all appearance of a comfortable and safe state presented.

The next day, at the same hour, the same symptoms came on, and ran the same course. I was again sent for, but only got to the patient in time to learn the history of a paroxysm in no respect different from the preceding; and seeing no indication for medicine, I gave none.

The third day I was again sent for, and was too late again; but, on my arrival, heard a repetition of the story, describing a paroxysm which ran precisely the same course. I made a very particular inquiry into all the attending circumstances—sleep, food, discharges of bowels, bladder, etc. All was perfectly satisfactory.

Next morning I called just after the time when the paroxysm should

come, and found her suffering great pain; the pulse not disturbed; tongue clean; surface normal in temperature; facial aspect exhibiting pain, but colour unchanged; nothing of the pallid cheeks and purple lips common in ague; hands and feet of usual temperature; in short, nothing but her words to indicate disease, except the sanguineous discharge from the vagina. A per vaginam examination did not reveal any change whatever in the uterus; it was not tender to the touch, swollen, or hot. The os was patulous to the extent that is usual from the eighth to the twelfth or fourteenth day after delivery. There were no inflammatory symptoms in the uterus or adjacent parts; the vagina was soft, moist, and cool; no mechanical obstacle to the exit of the lochia was observable; the character as well as the quantity of the discharge was totally changed during the paroxysm. The case, to me, being new, left me to speculate as to its nature.

I began my investigation by inquiring into inherited peculiarities, idiosyncrasy, and previous diseases. I thus learned that about a year ago she had spent some weeks, during the summer and fall, a short distance below Salem, New Jersey, and there had had the chills; she had been quite cured, however, and had not had them since. This brought to mind two cases of dysentery, formerly under my treatment, in which the dysenteric symptoms returned at regularly recurring periods. The first was a quotidian, which ran on nearly a week before I awakened to a knowledge of its nature; the other was a tertian; both of which were treated with antiperiodics, and both yielded to the treatment. I therefore, after closely inquiring into the state of the general organism, considered it expedient, in the absence of any special contra-indication, to apply the antiperiodic plan of treatment to this case.

It is well known that the puerperal state is one that contra-indicates very active treatment of any kind, and especially of stimulation or excitation. Inflammation of the uterus, puerperal mania, or meningitis, might be brought on or occur coincidently, therefore I adopted a moderate course to begin with. Eight grains of quiniæ sulphas, with one grain, each, of opium and ipecacuanha, were made into four pills, and one given every hour, beginning at 5 o'clock A. M.; by this plan all of them were to be taken by 8 o'clock A. M., one hour and a half before the paroxysm.

My next visit being made after the period for the usual diurnal attack, I found the patient comfortable and cheerful, and learned that the discharge was diminished in quantity and the attendant suffering greatly ameliorated. Not a single unpleasant symptom had been caused by the quinia; I therefore increased the quantity to twelve grains, which, with one grain, each, of opium, and ipecac., I had made into four powders, and gave one every hour, beginning, as before, at 5 A. M. On my next visit I learned that the patient had missed both pain and discharge. I then ordered twelve grains of quiniæ sulphas, to be divided into twelve pills, one to be taken every hour, beginning at 5 A. M., until four should be taken each

day. This terminated the treatment without any unpleasant symptom as the result, the patient having ever since continued well.

Oct. 6. *Large Dose of Opium taken by a Child, without Fatal Consequences.*—Dr. HAYS related to the College the particulars of a case in which a child, not quite six years old, was given a powder containing seven and a half grains of opium with the same quantity of prepared chalk (the former having been, by mistake, substituted for rhubarb, which had been ordered). Dr. H. did not see the patient until fourteen hours after the powder had been administered. He was told that the child, after taking the medicine, had seemed much excited; this was followed by restlessness and drowsiness, which continued at the time of Dr. H.'s visit. No vomiting had taken place. The narcotism was at no time very profound; it gradually wore off, and at the end of three days had entirely disappeared.

Drs. CONDIE, GRISCOM, and PAUL mentioned several cases in which large doses of laudanum had been taken by children without serious mischief.

Nov. 2. *Vesico-vaginal Fistula.*—Dr. R. K. SMITH read the following report of a case of vesico-vaginal fistula successfully treated, by D. HAYES AGNEW, M. D., of Philadelphia:—

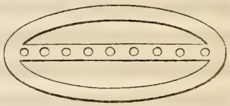
Frances Hargraves was admitted into the Philadelphia Hospital, Blockley, suffering from a vesico-vaginal fistula. The following account of the accident was obtained from the patient: In January, 1858, she gave birth to a child. Her labour was exceedingly difficult and prolonged, to aid which ergot was freely administered by her medical attendant. After delivery, for several days, she found herself unable to pass urine; which continuing to accumulate, and not being relieved by instrumental interference, she suddenly felt a large gush of water escaping from the vagina, since which time her urine has continued to flow by this route. Calling the attention of her physician to this condition of things, he suggested the necessity of an operation for her relief, which was accordingly performed in May, 1858. This failing, a second one was tried two or three weeks subsequently, with a similar result. The operation adopted was, I presume, that of Dr. Sims, with the button of Bozeman, as she described the employment of silver wires and a lead plate. Since the accident, she informs me, she has never menstruated; but alleges that, when the period comes round, a very copious flow of urine takes place, and continues for two or three days. About the 1st of July I was invited to see her by Dr. Robert K. Smith, the present chief resident physician of the Philadelphia Hospital, and, in company with himself and Dr. Elwood Wilson, made an examination. An extensive transverse rent was discovered, extending from one side of the vagina to the other, certainly one inch and a half in extent. Through this protruded a large amount of thickened and inflamed mucous membrane of the bladder, and along its edge the marks of

the old sutures were quite visible. At the suggestion of Dr. Smith, and her own earnest entreaty, I concluded to attempt her relief by another operation. As the last, however, had been done only three weeks previous, it was deemed most prudent to delay any efforts of the kind for six or eight weeks. The edges were soft, extensible, and deficient in callosity or density, which showed that they had not fully recovered from the recent attempt to cure. There was evidently not sufficient resistance in the tissue to maintain the tension of sutures for any length of time. To favour the desired condition, the parts were directed to be brushed over with a strong solution of tannic acid in glycerine every day. On the 23d of August I proceeded to operate, in the presence of Drs. Smith, Wilson, Levis, McClellan, Darby, Nichols, and the internes of the house. The bowels having been previously well emptied, the patient was thoroughly etherized, and, being supported on her breast and knees, lever speculæ were inserted along the lateral and posterior walls of the vagina, and the parts drawn well asunder; the edge of the fistula was seized by a pair of rat-toothed forceps, drawn well down, and pared by means of a long-handled straight bistoury. The greatest difficulty at this stage of the procedure was in freshening the angles of the wound, in consequence of the obstinate protrusion of large folds of the vesical mucous membrane. Succeeding, however, to my satisfaction, nine needles armed with sutures of fine silver wire were introduced by means of a forceps admirably constructed by Mr. Gemrig for this purpose, the two ends of which wires were brought out of the vagina and given in charge of assistants. These were next passed through a lead plate or button, having in it a number of holes corresponding to the sutures, and modified for reasons which I shall presently state. This being accurately adjusted, and the wires well tightened, pellets of shot were slipped down over each to the button, and firmly compressed by a pair of strong dressing-forceps, thus securing my ligatures in position. The long ends of the wires protruding from the vagina were well wrapped with adhesive plaster, to prevent excoriation, and a catheter was carried through the urethra into the bladder; the patient was placed on her side, an anodyne being administered, and a diet of arrowroot and cream ordered. She was then left in the care of Dr. Cowsins, one of the resident physicians of the house, to whose careful and judicious management I owe much of my success. The whole operation consumed about two hours, and though she was kept completely under the influence of the anæsthetic, with her head and trunk dependent, no inconvenience whatever was experienced, not even the ordinary nausea. It is unnecessary to give the notes of this case as kept from day to day, nothing of any interest having occurred until September 1. It is sufficient to say that her bowels were kept confined by the exhibition of a pill night and morning, containing a quarter grain of opium, the catheter was removed daily and cleansed, and the position on the side was carefully maintained. No constitutional disturbance whatever occurred,

very little local soreness was experienced, and no leakage discovered from the vagina. On Wednesday afternoon, September 1, being ten days after the operation, I proceeded to remove the plate and sutures, in the presence of Drs. Wilson, Levis, McClellan, and Richardson. The shot were clipped, the button withdrawn, and the wires severally picked out, when we had the pleasure of learning that complete union had taken place. As a precautionary measure, the catheter was allowed to remain for eight days longer, and the patient confined to bed on the side. On the twelfth day her bowels were gently moved, and again locked up for five or six days. Ten days after the removal of the ligatures she was allowed to walk about.

Remarks.—It will be seen from the above details that the operation in this case was that of Dr. Sims, varying only in the employment of the Bozeman button as modified by myself. To that modification I am disposed to attribute the success in this particular case. The proneness of the mucous membrane of the bladder to project through a fistula into the vagina I suppose has been noticed by every one who has seen a case of the accident. When the ordinary button is used, it is impossible for the surgeon to determine whether a portion may not have insinuated itself between the edges which he is approximating, and thus defeat the desired union. I think this may explain the failures which often occur. To obviate the possibility of such an occurrence, I requested Mr. Gemrig to make me such a button (lead) as is represented in the annexed cut, in which there is a centre-piece with holes through which to pass the ligatures, and between this and the circumference on either side two half-moon shaped pieces are cut out. The advantage is obvious. When it is placed in position, the operator can readily see if there be anything interfering with the accurate adaptation of the parts. Had I not adopted this button, I feel satisfied failure must have ensued, as I was obliged to press back in two places small portions of the mucous membrane of the bladder, which had worked down between the approximated edges of the wound, and which were easily discovered through the portions of the button cut out. This case proved also very conclusively, to my mind, the value of the metallic over the silk suture. There was scarcely a trace of suppuration, the ligatures being all well in place on the day of their removal, ten days subsequent to their introduction. This fact would induce me, in another case, to dispense with the button altogether, as, I believe, is now practised by Dr. Sims, simply twisting the wires, or otherwise to employ the twisted suture, using silver pins, and protecting their ends by shot.

Fig. 1.



Dr. R. K. SMITH stated that he had not seen this case since the middle of September. At that time he had been satisfied, with Dr. Agnew, that the cicatrization was complete, and the fistula entirely closed.

Since then the patient had been repeatedly and very carefully examined

by different surgeons, who all agreed that no solution of continuity could be discovered in the track of the fistula, although there was evidently some incontinence of urine. The cause of this leakage could not be determined, but it was not through any defect in the cicatrix. [The incontinence of urine has since been ascertained, by Dr. R. P. Thomas, at the Episcopal Hospital, to have been owing to an adhesion of the neck of the bladder to the uterus, and a displacement of the latter, by which the urethral orifice was drawn open, whenever the patient assumed the recumbent position. The difficulty was overcome by the introduction of a pessary. The patient has continued apparently well up to the present date, March 2, 1859.]

Dr. CONDIE remarked that he had been taught by experience the necessity of allowing, in cases of apparently successful operations for vesico-vaginal fistula, some time to elapse before deciding upon the permanent relief of the patient. In the three cases of operation for vesico-vaginal fistula that had fallen under his notice, the opening between the bladder and vagina remained apparently completely closed, in one three weeks, in another six, and in the third upwards of eight months, when, unexpectedly, it became open to the same extent as at first.

In the third of the cases just referred to, the operation was performed, in a most skilful manner, by Dr. Pancoast. The cure of the fistula was apparently permanent, and so continued to the end of eight months, when, in consequence of some unusual sensations experienced by the patient in the region of the bladder, an examination was made, and a large calculous concretion detected in the bladder, at the place where the fistula had existed. Ulceration soon after took place, and caused a reopening of the communication between the bladder and vagina. In the meantime the case had been reported as a triumphant cure.

It may be that the improved methods of operating recently introduced may be attended with a more permanent success than those adopted in the three cases just referred to.

Dec. 1. *Effect of Respiration on the Position of the Heart.*—Dr. DA COSTA read the following paper on the effect of respiration upon the position of the heart:—

In auscultating persons who supposed themselves affected with cardiac disease, I have had my attention directed to several points connected with the influence of the respiration on the heart, which seem of sufficient interest to warrant my laying them this evening before the College. The phenomena in question may have been noticed by others, but I have not been able to meet them referred to, and to me, at least, they presented the allurement of novelty.

If a full inspiration be taken, it may be usually observed that the action of the heart becomes slower, again to quicken in expiration; and, secondly, that the impulse between the fifth and sixth ribs is hardly or not at all per-

ceptible, whilst the sounds of the heart are only very faintly heard. The latter fact has been treated of in a paper which is now in print.[1] The effects of inspiration in rendering slower, and of expiration in quickening the movements of the heart, have been well investigated by able physiologists; but the change of the position of the heart during the acts of respiration, and the causes of this change, are comparatively unnoticed, and are the subjects which I wish especially to lay before the College.

The impulse, then, at its normal situation, disappears if a long breath be drawn, and the heart sounds at the left apex become almost inaudible. If now, whilst the inspiration is held, the chest be carefully watched, a new impulse can be seen, somewhat lower than between the fifth and sixth ribs, and towards the median line, at a point in which, in quiet breathing, no movement is perceptible.

Desirous of investigating these phenomena, I examined this new impulse —if, for convenience sake, it may be so termed—in a series of observations, made at different times, on healthy persons, having in each instance first accurately ascertained the extent of the percussion dulness of heart and the normal position and strength of the impulse of quiet breathing.

Inspiration.—During a full inspiration in ten adults the upper border of percussion dulness over the heart shifted to nearly the extent of an intercostal space. The impulse became, in all the instances examined, entirely or almost entirely imperceptible under the nipple, between the fifth and sixth ribs; but it was distinctly felt downwards and inwards, at a spot nearly on a level with the ensiform cartilage, immediately below the cartilages of the ribs, and varying from three-fourths to one and a fourth inch from the median line. In some cases, in which it was measured, the new point of distinct impulse was three and a half inches from the impulse beat of quiet breathing.

Percussion over this new point shows dulness and greater resistance to the finger. Near the nipple, between the fifth and sixth ribs, the sound becomes clear. Compared with the point of previous impulse, the beat is not quite as strong; the cardiac sounds heard over it are distinct, far more distinct than near the nipple, where they are almost inaudible, but not quite as distinct as they are at the apex during natural breathing. Especially is this the case with the first sound. The pulse corresponds, during full inspiration, to the somewhat diminished impulse; it is feebler. Yet neither diminished impulse nor feebler pulse is an invariable accompaniment of a forced inspiration.

Expiration.—During a full expiration the phenomena that are observable are almost exactly opposite. The percussion dulness over the heart increases markedly, especially at the upper border. The impulse is forcibly felt, and is extended over a larger space. Its point of distinctness moves

[1] American Journal of the Medical Sciences, January, 1859.

upwards; in some persons by more than the breadth of the rib. It may or may not be at all perceived lower down. The first sound of the heart increases in loudness, but by no means uniformly in distinctness; in some persons it is during a held expiration weightier but less distinct than during quiet breathing.

Having settled these points by repeated clinical inquiry, I sought to explain the cause of the phenomena. To me the conclusion seemed inevitable that not only is the heart during a full inspiration lowered, but its whole position is changed. It is carried not only downwards, but its apex is pressed forwards and inwards by the distended lung. If this be correct, it is evident that the effects of respiration upon the position of the heart have been erroneously stated even by those few authors who have said anything at all on the subject. To quote from standard authorities :—

"In inspiration, also," say Sharpey and Quain, "when the diaphragm sinks and the lungs expand, the apex is withdrawn from the thoracic parietes."

In the article "Respiration" in the *Cyclopædia of Anatomy and Physiology*, vol. iv., John Reid remarks: "But during forcible inspiration the heart recedes deeper into the chest, and during expiration it again comes forwards."

Valentin (p. 185 of his *Physiology*), mentioning the fact that during a full inspiration the heart's impulse is not as distinctly felt, attributes it to the intervening lung. He further adds: "And if a person turns in bed from the left side to the right side, the sensible impulse may likewise disappear. These facts, at any rate, teach us that the heart, which is inclosed air-tight in the chest, does not under all circumstances press against its wall."

Now these statements all indicate that the heart during a full inspiration recedes, or at any rate is removed from the walls of the chest; but do they explain the fact that a new impulse beat is perceptible lower down? Certainly not. The fact itself is not at all alluded to; the explanation of the fact is not possible according to any of the views just stated.

Walshe, who with characteristic completeness discusses the effects of posture and respiration upon the heart, states that inspiration, by carrying the diaphragm downwards, lowers the heart sometimes by an entire interspace, and removes the organ somewhat from the thoracic walls; "but it is to be remembered," he concludes, "that the depression of the diaphragm displaces the base more than the apex."

It is true that this lowering of the organ might account for the new impulse, by supposing it to be that of the right ventricle; but the disappearance of the apex beat from its normal point, and the visible impulse at another, seemed to me to demand a different explanation, namely, that the heart is not only somewhat lowered, but changed in its whole position, during the act of inspiration. It is carried not only downwards, but its

apex is moved inwards and forwards, and this change of position can only be brought about by the pressure of the distended lung.

Reasoning on the phenomena as observed in the living, led to these conclusions. The aid kindly afforded by Dr. Agnew enabled me to test by experiment their truth. The sternum of a subject was removed, and we then proceeded carefully to inflate the lungs. The change in the position of the heart was most striking. As the lung rose more and more, until it nearly reached the ribs, the apex gradually turned inwards, was somewhat lowered, and came distinctly forwards. If it had then struck the parietes of the chest, the impulse would have been visible at exactly the point in which it is observed to strike in the living subject during a full inspiration. The experiments were subsequently repeated, and invariably with the same results.

The next subject that suggested itself for inquiry was, what share the diaphragm has in producing the change in the position of the organ. On the inward and forward movement of the lower part of the heart it evidently has none. The descent of the diaphragm can, of course, only influence the heart through the connection of the pericardium to the diaphragm; and, again, through the attachment of this membrane to the large vessels after they leave the heart. Now, the effect, if the diaphragm move, will be to lengthen the vessels, and thus to lower the whole situation of the heart, but not to turn its apex inwards. Nor is it by any means certain that the downward action of the diaphragm can lower the heart much. The tendinous centre of the diaphragm, to which the pericardium is attached, moves but little, far less than is usually supposed, and its descent is further counteracted by the connection of the deep cervical fascia with the upper end of the pericardium. In order to study the motions of the diaphragm, the abdominal walls of a subject were opened, and all the viscera, excepting the liver, removed. The lungs were then inflated by a tube passed into the trachea. The muscular fibres of the diaphragm descended more and more, and finally bulged out on each side of the tendinous centre; but this neither descended nor ascended much during the acts of inspiration or expiration.

These experiments prove the displacement of the heart to be only in part produced by the diaphragm, much more by the sweep of the distended lung. The forward motion of the organ owns the same cause.

In cases of hypertrophy of the heart the act of inspiration produces a similar displacement; but whether as much, I am not yet able to say.

It may be well here to allude to an argument which might be urged against the interpretation of the changed impulse. As during a full inspiration the ribs are elevated, the finger applied over them is naturally brought up higher, and the heart might thus appear displaced, when, in reality, the ribs are. This is true. But it will not explain an impulse perceived near the median line; and, again, the experiment of removing

the sternum permits the influence of this source of fallacy to be exactly appreciated.

It is hardly necessary to discuss the causes of the phenomena observed in expiration. They follow as a corollary to those of inspiration. The recedence of the lung, the somewhat altered position produced by the different action of the diaphragm and by the absence of all traction on the large vessels, will explain the increased extent and change of impulse, and the enlarged percussion dulness. There are also probably other causes connected with the quantity and state of the circulating blood, and the varying distension of the heart during the respiratory acts, which must be taken into account; but until physiology lifts the veil which enshrouds the relation of the circulation to the respiration, no absolute conclusion can be arrived at as to an apparent increased size, and, still more so, none as to changes in the strength of the impulse.

Tænia cured with Pumpkin-Seeds.—Dr. HUNT exhibited a large portion of a tapeworm, which had been discharged from a patient whose case he briefly narrated.

The patient had been suffering at least two years, and during that time had taken all the ordinary remedies, without apparent relief. Dr. Hunt determined to try the pumpkin-seeds, and at first administered them in the form of an emulsion. For this purpose the seeds were deprived of their envelops, and rubbed with sugar, gum, and water. Two doses, of six fluidounces each, were given in this way on an empty stomach. The effect produced was slight, small pieces only of the worm coming away, and mostly in single joints, as it had been coming for the previous two years. He next prescribed the kamala, which has been recently extolled. This, however, only brought on a violent purging, without expelling any of the worm.

After the irritation from the cathartic action of the kamala had subsided, the pumpkin-seeds were again resorted to; but this time they were not deprived of the hulls, and were bruised in a mortar, or rather crushed or ground in a press, by an apothecary. Eight ounces of the seeds were thus reduced to a mass, of which the patient swallowed about one-half at bedtime. The remainder was made into a decoction with boiling-water, strained and taken before breakfast the next morning. The effect was very prompt; a large section of the worm coming away after the first dose, and a much larger one after the second. Although the head was not found, there was good reason for presuming that the patient had been cured, since he had continued several weeks entirely free from the symptoms which had, until that time, indicated the presence of the parasite.

Dr. E. WALLACE mentioned a case of a child, in which he had succeeded in removing a tapeworm by the use of pumpkin-seeds, crushed and rubbed up in a mass with sugar.

Imperforate Rectum.—Dr. Corse reported the following case:—

Peter and Alice Kayton called at my office with their infant, on Friday afternoon, November 26, 1858, for the purpose, as they stated, of getting something to open the child's bowels, and from them I learned that she was born, at some distance from this city, on Sunday morning, November 21, 1858, a little after midnight. On Sunday evening it was observed that she had had no passage from the bowels, and this led to an examination which gave rise to the suspicion that something was wrong; up to this time she had not sucked, although the nipple had been repeatedly placed in her mouth.

On Monday morning a piece of soap was pushed into the anus, and a small quantity of black matter came away with it. She still refused to suck.

On Tuesday morning a dose of magnesia was administered to her, and "soon after she sucked as well as any child." Upon inquiry I found that the mother's breasts then for the first time filled up with milk.

On Wednesday morning a practitioner of the neighborhood was called in, and ordered three powders, one to be given every six hours.

On Thursday morning the mother took castor oil, having been told that that would open the child's bowels. This day the child for the first time vomited, and the matter discharged was of a green colour.

On Friday the child again vomited "*yellow, nasty, ropy stuff.*" They then became uneasy, came to Philadelphia, and called on me late in the afternoon; on examination, by means of a gum-elastic bougie, I immediately discovered the nature of the case, and appointed 10 o'clock next morning to operate.

On Saturday morning the infant "*vomited milk, only a little.*" On more elaborate examination I found the anus perfect, with an opening a full half an inch in depth, consisting of a blind pouch or *cul-de-sac*. I then passed a silver female catheter into the vagina, and found that the posterior wall was very close to the integument, and followed the course natural to the rectum, laying on the floor of the perineum, there being nothing between the two membranes which I could suppose was a canal.

Although generally denominated imperforate anus, the several varieties of this abnormity present important differences. 1. Simple occlusion of the anal opening by a membrane more or less thick; in this variety there will appear at the point where the anus should open, a fluctuating tumour of a dark colour, distended by meconium. 2. The anus may be perfect, and a short distance up there may be a septum of greater or less extent. 3. The rectum is absent, and there may or not be an anus: to this last variety the case above described belongs.

There are other abnormal arrangements of the rectum which have sometimes been described under the name of imperforate anus; but improperly

so. They consist of the rectum opening into the vagina, bladder, or urethra.

Among the different operations which have been proposed to remedy the deformity under consideration, there are three, each of which is based upon a different principle; and each admits of greater or less variation, according to the circumstances of the case.

1st. An artificial opening is made in the perineum, and the dissection carried into the pelvis in the direction where the rectum would naturally be located.

2d. An operation for artificial anus, invented by Callisen and since modified by Amussat. It is based upon the fact, that the colon in the left lumbar region has not a peritoneal covering posteriorly; it may, therefore, be reached without opening the peritoneum. Callisen's operation consists in making an incision in the left lumbar region, parallel with the spinal column, from the crest of the ileum nearly to the lower rib, and not far from the ends of the transverse processes of the lumbar vertebra. Amussat has modified this operation by making the incision almost at right angles with the vertebral column, and deepening it with great care until the colon is reached; the gut is then seized with a hook, drawn out, and opened.

3d. Littrè's operation in the left iliac region, is effected by an incision parallel with Poupart's ligament, opening the cavity of the peritoneum, and thus exposing the bowel; this must be drawn out, retained in apposition to the integumental aperture a sufficient length of time for the establishment of adhesion, and then evacuated by incision.

The operation performed here was in the perineum, and the cutting extended as far as prudence would permit.

I commenced by making an incision posteriorly, cutting through the sphincter ani muscles; and after dilating the orifice, sought for a fluctuating tumour of a dark colour, but found nothing of the kind. The incision was then extended posteriorly towards the coccyx, all the while keeping a vigilant look out for any soft body that might appear. By degrees the dissection was cautiously carried backwards along the hollow of the sacrum, using the catheter in the vagina to prevent cutting into it, until I reached the superior strait of the pelvis; having separated the pelvic contents very freely from the sides around, an opportunity was thus afforded for the intestine to descend; nothing of the kind made its appearance, and I was fearful to carry the dissection any further, lest I should cut the aorta or primitive iliac.

I placed a tent of lint in the wound, and had the child brought to me the next day; a careful examination was made, but nothing like the bowel appeared, nor was there any tumefaction of the abdomen.

Monday morning the father called to inform me that no evacuation from

the bowels had taken place; but the child sucked well, slept well, and passed plenty of urine.

Tuesday morning I learned from the father that the child seemed in every way comfortable; she sucked greedily, slept well, urinated abundantly, and had some bearing-down efforts, as in evacuating the bowels.

Wednesday—this morning, at $4\frac{1}{2}$ o'clock, a change took place; the child seemed weak, gradually sank, and expired without a struggle, at $9\frac{3}{4}$ A. M., having lived a little more than ten days.

The case had several features, which struck me as being extraordinary. 1. The child sucked greedily from the time the flow of milk was established until a very short time before its death. 2. It rested remarkably well, not at any time evincing suffering of any consequence. 3. It vomited very little, indeed; this leads to the supposition that the milk was digested and absorbed. 4. There was no tumefaction of the abdomen until the last few hours of its life.

We are then to suppose that the detritus of the azotized tissues passed away as urea and other constituents of the urine, which was very copious; and that the carbo-hydrogens were converted into carbonic acid and water, and passed away by the lungs, skin, and kidneys. The distension of the abdomen towards the last was sudden, and was, probably, due to the evolution of gas, and not to accumulation of feces. The child was named after a relative, and many fine expectations were disappointed by its death; under these circumstances I was denied the privilege of making a *post-mortem* examination, although I earnestly solicited it.

An autopsy in this case would, of course, have afforded a contribution both to teratology and physiology.

Hemorrhage from the Pharynx, resulting from the partial swallowing of a piece of cartilage.—Dr. Packard exhibited a thin triangular and pointed piece of cartilage, $2\frac{1}{4}$ inches long and $1\frac{1}{4}$ inch wide, the epiphysis of the breast-bone of a young turkey, which had been partially swallowed by a man about forty years of age. It was supposed to have lodged in the œsophagus, about one inch below the level of the top of the larynx. After suffering more or less from it, and being unable to take any solid food until it was expelled, he managed to pull out the fragment on the sixth day.

No unpleasant effects were observed by the patient until the evening of the eighth day, and the second day after the removal of the foreign body. He was then suddenly attacked with a copious arterial hemorrhage, losing, in a short time, about three pints of red blood. Dr. Packard was immediately called upon, and succeeded in stopping the flow of blood by free applications of tinct. ferri chlor., with a probang and sponge, introduced into the pharynx. Another less copious hemorrhage occurred, after an interval of forty-eight hours, and was checked without difficulty in the same manner. The patient has since then escaped a further attack.

1859, *Jan. 5.* *Improved Spirometer.*—Dr. S. W. MITCHELL exhibited an improved form of spirometer, of which he gave the following description:—

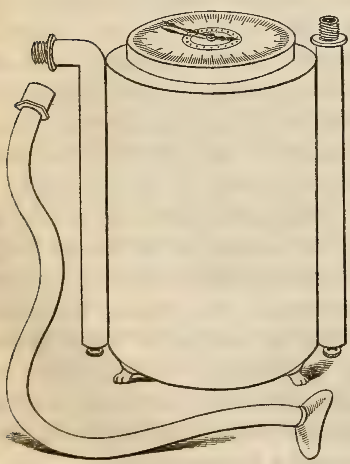
While conducting some recent researches upon the physical statistics of natives of this country, I was struck with the clumsiness, and, in some cases, with the inaccuracy, of the form of spirometer in common use. This instrument, well known as Hutchinson's spirometer, is accurate in its indications when well made, but is always inconvenient, because it requires to be filled with water, and, on account of its weight and form, is not easily carried from place to place. This latter objection applies equally to all the spirometers I have examined; while most of the instruments made on this side of the water are also open to the additional objection of want of accuracy.

The instrument which I have used in its place is merely a small "dry gas-meter," numbers of which are made in this city by Messrs. Code, Hopper & Co. With the aid of Mr. Gratz, a member of this firm, some slight alterations were made in the valves of the meter, and the inlet-pipe was somewhat narrowed. A large dial-plate, graduated to inches and halves of inches, was also placed on top of the meter, so that the rotation of an indicating hand would mark the number of cubic inches which passed through the machine.

It is unnecessary to describe the interior details of the meter thus adapted to spirometric use. In the form of a dry gas-meter it has been used, almost to the exclusion of the very inconvenient "wet meter," in many parts of this country, and has been found to perform its work of registration, for years together, without serious error and without needing repairs.

The instrument thus briefly described is figured in the annexed cut. Its height is but fourteen inches, its width eleven. The inlet-pipe is so marked, and to this, when arranged for use, an India-rubber tube and mouth-piece are attached. The inlet and outlet-pipes form convenient handles when the spirometer is to be carried.

Fig. 2.



I have tested the accuracy of this little instrument with great care, and have put it to more severe trials than the meter is usually subjected to. Its indications appeared to be almost perfect. The new form of spirometer runs with so little friction, that a pressure of one-eighth of an inch of water will move it readily. One

source of error is thus avoided, since, if the instrument did not move easily, the first air blown into it would be more or less condensed, and so occupy less space than it should do.

After the spirometer has been used fifty or sixty times, the moisture from the breath, which collects in the form of a few drops of water within the instrument, should be allowed to escape by the removal of a button placed underneath the meter.

The form of spirometer here described has lately been employed by a committee of the biological department of the Academy of Natural Sciences of this city, to measure the pulmonary capacity of some five hundred men. In every way the instrument was found to be satisfactory, being portable, requiring no water, and not being readily deranged.

So far as the committee have gone, they have seen increasing reason to regard with distrust all spirometric valuations of men. In every case they have been careful, by repeated trials, to obtain the best result of which the individual was capable; but they have been, so far, much impressed with the difference between their own results and those obtained by others abroad. As the height, weight, and girth of chest have also been taken, the committee hope, at some future time, to be able to report in detail their results, as bearing upon the lung capacity of men born in this country.

The spirometer is to be obtained from Messrs. Code & Hopper of this city, well known as manufacturers of gas-meters. The cost of the new instrument will probably not exceed fifteen dollars, which is less than that of the worst form of the ordinary spirometer now in use.

Feb. 2. Report on Meteorology and Epidemics.—Dr. Wilson Jewell read the following report on meteorology and epidemics for 1858:—

In submitting the annual report to the College, on meteorology and epidemics, I have occasion anew to refer to the incompleteness of the tables and computations, the inevitable result of inherent difficulties from defective returns under the existing law for the registration of deaths.

The best estimate of the health of our city can only be derived from its records of mortality as compared with those of former years and other cities. The value, therefore, of such a record, depends upon its accuracy and truthfulness.

Our statistics, however, are, like many elsewhere collected, not only imperfect in systematic arrangement, but very general, and, in some instances, of doubtful character. This latter defect is attributable in a great measure to the irresponsible sources from which they emanate, and to the frequent false returns made by ignorant and knavish pretenders in medicine, between whom and the scientific and educated physician the statute-books of our commonwealth, as well as a misguided public, recognize no distinction.

While it is humiliating to refer to the existence of such irregularities, it

is no less essential, in order that proper allowance be made for the incongruities which may be discovered in these records of mortality.

But, notwithstanding the imperfections alluded to, fragments of reliable information have been gathered through these channels, from which may be drawn truthful conclusions bearing upon the relative cause and effect of disease, as it has prevailed from year to year in the midst of our population.

Nor are these scanty records less useful for stimulating us with enlarged desires to secure the collection of more perfect returns, not only of deaths, but of births and marriages, until the vital statistics of our extended municipality shall become a stand-point from which we may not only look abroad, and, with some degree of precision, discover and investigate the nature and cause of our prevalent diseases, but provide adequate means to guard against the introduction of those deleterious agents that vitiate our atmosphere, or in other ways become the causes of sickness and death in our community.

I shall not therefore be considered ultra in my opinion if I affirm, that our mortality tables for Philadelphia never can be otherwise than as represented, until a more complete system of registration has been secured by legislative enactment. In this connection I cannot resist adverting to the many imperfections in the nomenclature of diseases, as employed by those who furnish the certificates of death received at the health office in our city. I refer more especially to the entire absence of a uniform method in reporting the causes of death. Very few of our physicians adhere to any one particular nosological arrangement. Indeed, I apprehend that many, whose duty it becomes to give certificates of death, have so limited an appreciation of this important department of science, that they feel no incentive to answer its demands; while to the unscrupulous pretender, who never looks beyond the sordid motive of pecuniary gain in the pursuit of his calling, it is a question of entire indifference what name he shall give the disease of which his patient died, admitting that he possesses sufficient intelligence to inform himself of the true cause of death. I do not make these remarks in a censorious spirit, nor from a belief that this want of accuracy is peculiar to our own city. Similar imperfections exist elsewhere, in other cities, and, in some instances, to a far greater extent. The true cause of the irregularity to which I have reference will be found in the absence of a uniform and approved nosological classification of diseases.

A careful comparison of the record of deaths for 1858 with that of the two preceding years, will present a single feature which can only be attributed to the defective plan now pursued in certifying to and in recording deaths. In the table of mortality for 1857, as reported to the College, the causes of death are represented by 201 distinct names, and in 1856 by 195; in the one under consideration, for 1858, there will be found only 112, less by 86 than the average of those in the two preceding years. In presenting this statement, I do not desire to be understood as entertaining for a single

moment the opinion, that the causes of death from one year to another should correspond in every particular, both as to numbers and variety; but while I take a common sense view of this subject, and make careful allowance for changes and inaccuracies in each tabulated record of names of diseases, I cannot understand on what principle so great a difference should occur as that referred to, especially when there are no deaths recorded from some of the most frequent forms of disease.

Under the title of cancer and scirrhus will be found 125 deaths, but in no instance is the special organ or structure involved, designated. Nor are there deaths named in the table from any form of disease of the uterus or ovaries, except the 36 deaths from puerperal fever, which fall below the usual amount for the year. Deaths from hemorrhage of the lungs, stomach, or bowels, disease of the bladder or kidneys, are all wanting. It is asking too much to believe that during the year there had not been a single death from any of these diseases. The fact should not be overlooked, however, that the unusual health of our population might possibly have exerted an influence upon the change in the causes of death to which I have reference. Still, I am not disposed to advocate the idea, that this apparent exemption from sickness would effect an alteration to the like extent as manifested in the record, much less from the same class of diseases. It must have resulted from other and less legitimate causes.

The only rational causes I can educe for the absence of these terms from the record are the loose and imperfect system of registration, a defective classification of diseases, and, last, though not least, sheer carelessness and incompetence in many to investigate the cause of death.

This entire subject of registration and classification of diseases in this country is now commanding more attention than ever before. Physicians, men of science, statesmen, and political economists are becoming interested in those great principles which involve the science of life, and are thoroughly investigating their influence.

Eight of these United States have at this time in successful operation well regulated and reliable plans, legally enforced, to register the births, marriages, and deaths in their respective commonwealths. In several other States the subject has been agitated, and preliminary steps taken to secure similar laws. Pennsylvania still slumbers over these vital interests of humanity. Other and less important claims she watches with an Argus eye, but can behold no wisdom in watching over the health and the lives of her citizens.

I am happy to announce, however, that another effort, bearing the approval of this College, is now in progress to procure a law for Philadelphia only, for the registration of births, marriages, and deaths. I confidently hope that the next report on this subject will embrace so desirable an improvement, and harbinger the dawn of a new era, in the collection and analyzation of the vital statistics of our city. In this event, the foundation

for a superstructure will be laid, that shall augment in value and importance from year to year, as the work progresses, until it shall exhibit a proud monument, teeming on every side with reliable data, for the employment of the intellect and industry of a future statistician, out of which he may determine with accuracy the growth or the decay of the public health.

Nor will the advantage be confined to our own city. I flatter myself, that, if the law is secured, its usefulness will be made so evident in a few years, that the necessity for one embracing the entire State will be so widely recognized, that no difficulty will be experienced in obtaining its passage through the Legislature.

The report of that distinguished statistician, Edward Jarvis, M. D., of Mass., presented at the last session of the American Medical Association, on the law of registration, is replete with interest. It evinces a sound knowledge of the subject, and reflects great credit upon its author, placing him high in the list of writers on medical literature and medical statistics. In this article a statistical nosology is furnished, for the consideration of the profession, which is a revision of the one adopted by the association in 1847. Dr. Farr's plan of classification of diseases, as followed by the Registrar-General of England in his valuable reports, is also appended; and the writer says, "it is worth our consideration whether it would not be better to adopt this system for the American States, although looking to its merits alone, it may be inferior to that already in use."

Highly as I appreciate the experience and judgment of Dr. Jarvis in all matters that appertain to sound logic in medical statistics, and much as I may approve the motive that directed the above opinion, I am not ready to indorse it. There is no reason why we should not establish a system of statistical nosology peculiarly our own. We are not only capable, but our national character, our social condition, our climate, our physical and mental peculiarities, our habits and manners, and our principles of free government, all seem to urge upon us strong claims for an American system of classification of diseases.

At present this question is an unsettled one in our country. Doubtless it will remain so for some time, or until the combined judgments of the States shall see eye to eye in behalf of those vital interests of health and life, which are intimately allied to this whole subject.

When that enlightened period arrives, and it may not be far distant—when each State shall be prepared to enact uniform laws of registration, then will be the proper time, and the appointment of a medical commission, embracing delegates from all the States, suggests itself to my mind as the only sure provision for arranging and securing a system of classification of diseases, which shall not only be uniform in its arrangement, but acceptable, efficient, and permanent in its operation.

For present use, the table of the American Medical Association, adopted

in 1847, with a few slight alterations, may answer our purpose. It is concise, simple, easily understood, and available.

The annexed record contains an abstract from observations on the atmosphere and its phenomena, as made by my friend, Jas. A. Kirkpatrick, A. M., Professor of Civil Engineering in the Philadelphia High School, whose kindness I must again acknowledge.

These observations have been prepared with great care for the Smithsonian Institution, at Washington, and may be relied upon for their accuracy.

From this abstract we learn that the mean temperature for the year was 55.20°, an increase of 1.72° over that of 1857, and a higher mean temperature by 1.31° than for the last seven years.

The maximum temperature for the year was 96½°. This was on the 28th of June. The minimum temperature was 10° on the 5th and 6th of March.

The warmest days were the 28th of June and the 11th of July—the mean temperature being 89.2°.

The coldest day was the 5th of March, when the mean of the thermometer was 14.3°.

February was the coldest month in the year—the mean temperature being 30.11°.

July was the warmest month—the mean heat being 79.20°.

The monthly range of the thermometer for the year was 86½°, and the daily range 5.18°.

The mean of the thermometer for the summer was 77.24°, and for the winter 37.22°.

The least variable month was May, the range standing 35°.

The barometer ranged for the year, monthly, 1.325 inch—while the annual mean was 29.885 inches; nearly equivalent to the mean for seven years.

The highest point of pressure shown by the barometer was on the 8th of January, when it stood 30.531 inches; and the lowest was on the 21st of December, 29.206 inches.

The due point in its maximum for the year was 78.5°, and the minimum 13.5°.

The relative humidity of the atmosphere in its maximum was 100 per cent., while the minimum per cent. was 18.

The amount of rain that fell during the year was 41.059 inches, which was less than the rain in 1857 by 7.389 inches. The greatest depth of rain and snow in any month was in December, amounting to 5.459 inches. The least quantity was in March, only 1.124 inch.

The rain for the year was 2.94 inches less than for the last seven years.

General Abstract of Meteorological Observations, made at Philadelphia, Pa., during the year 1858.
By JAMES A. KIRKPATRICK, A. M., Prof. of Civil Engineering in the Philadelphia High School.

1858.	Thermometer.									Barometer Reduced to 32° F.							
							Range.									Range.	
Months.	7 A. M.	2 P. M.	9 P. M.	Mean.	Max.	Min.	Monthly.	Mean daily.	Mean of daily oscillations.	7 A. M.	2 P. M.	9 P. M.	Mean.	Max.	Min.	Monthly.	Mean daily.
	°	°	°	°	°	°	°	°	°	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January . . .	36.65	45.44	40.34	40.81	62	22	40	5.94	14.0	29.970	29.925	29.959	29.951	30.531	29.410	1.121	.208
February . . .	24.84	34.95	30.52	30.11	53	11	42	6.08	14.4	29.907	29.848	20.867	29.874	30.253	29.404	.849	.190
March	34.11	47.71	40.19	40.69	71	10	61	5.98	17.1	29.829	29.777	29.815	29.807	30.271	29.306	.965	.171
April	47.00	59.40	51.15	52.52	84	33	51	7.03	17.3	29.774	29.729	29.751	29.751	30.049	29.325	.724	.150
May	55.26	64.79	57.87	59.31	81	46	35	5.48	14.6	29.819	29.790	29.809	29.805	30.250	29.386	.864	.158
June	73.78	83.57	75.28	77.55	96½	53	43½	4.39	18.3	29.836	29.797	29.808	29.813	30.041	29.605	.436	.072
July	75.13	85.31	77.23	79.22	95½	60	35½	3.67	17.2	29.845	29.805	29.831	29.827	30.124	29.534	.590	.094
August	70.08	81.03	73.77	74.96	90	54	36	4.42	16.1	29.843	29.809	29.836	29.829	30.098	29.550	.548	.095
September . . .	61.32	76.03	66.37	67.92	88	41	47	4.05	19.7	29.944	29.892	29.925	29.922	30.217	29.343	.874	.135
October	53.32	66.63	58.53	59.49	90	35	55	5.32	17.3	29.934	29.876	29.918	29.906	30.318	29.382	.936	.143
November . . .	38.52	46.23	41.90	42.22	68	24	44	3.53	12.3	29.805	29.780	29.807	29.797	30.060	29.430	.630	.136
December . . .	34.50	40.97	37.47	37.65	63	15	48	6.30	11.8	30.002	29.955	29.966	29.974	30.459	29.206	1.253	.206
Annual means .	50.38	61.00	54.22	55.20	96½	10	86½	5.18	15.8	29.875	29.832	29.858	29.855	30.531	29.206	1.325	.146
Winter	32.97	41.92	37.05	37.32	64	11	53	5.91	13.9	29.931	29.881	29.912	29.908	30.531	29.266	1.265	.201
Spring	45.46	57.30	49.74	50 84	84	10	74	6.16	16.3	29.807	29.765	29.792	29.788	30.271	29.306	.965	.160
Summer	73.00	83.30	75.43	77.24	96½	53	43½	4.16	17.2	29.841	29.804	29.825	29.823	30.124	29.534	.590	.087
Autumn	51.06	62.96	55.60	56.54	90	24	66	4.30	16.4	29.891	29.849	29.883	29.874	30.318	29.343	.975	.138
For seven years .				53.89	100½	—5½	105½	5.52					29.880	30.709	28.895	1.814	.151

Meteorological Observations—Continued.

1858.	Relative Humidity.					Force of Vapour.						Winds.	Clouds. Tenths of sky covered.			Dew-point.				
Months.	7 A.M.	2 P.M.	9 P.M.	Max.	Min.	7 A.M.	2 P.M.	9 P.M.	Max.	Min.	Rain and melted snow.	Monthly resultant; No. of times in 1000.	7 A. M.	2 P. M.	9 P. M.	7 A.M.	2 P.M.	9 P.M.	Max.	Min.
	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Inch.	Inch.	Inch.	Inch.	Inch.	Inches.					°	°	°	°	°
January . . .	75	59	69	96	26	.171	.187	.182	.505	.078	2.686	N. 54° 44′ W., 299	5.9	6.4	5.1	29.40	31.19	30.76	59.3	12.9
February . . .	71	54	67	96	26	.102	.111	.119	.246	.042	2.393	N. 64° 32′ W., 302	6.1	6.1	4.5	16.70	19.32	21.02	39.8	—0.7
March	65	44	58	90	18	.138	.162	.156	.448	.023	1.124	N. 71° 0′ W., 398	4.9	4.6	3.0	23.10	25.31	25.86	55.9	—13.5
April	71	49	66	96	23	.238	.248	.256	.505	.089	4.681	N. 3° 38′ E., 104	6.4	6.9	6.0	37.59	38.06	39.64	59.3	15.8
May	75	60	72	97	26	.330	.366	.352	.691	.191	5.308	N. 63° 26′ E., 210	7.8	6.9	6.2	46.87	49.07	48.32	68.3	29.2
June	72	55	72	94	31	.606	.627	.641	.934	.361	4.205	N. 71° 34′ W., 234	5.3	5.9	4.1	63.78	64.85	65.42	77.2	50.0
July	72	50	69	86	32	.632	.615	.657	.975	.368	1.454	S. 37° 42′ W., 173	4.8	5.2	2.6	65.09	64.10	66.26	78.5	50.5
August	78	55	71	95	37	.589	.583	.605	.864	.319	5.157	N. 77° 28′ W., 55	5.6	6.2	4.5	63.03	62.52	63.71	74.9	46.7
September . .	79	50	72	95	34	.451	.465	.483	.792	.166	1.389	S. 88° 38′ W., 258	3.9	3.9	2.5	54.61	55.35	56.65	72.3	29.9
October . . .	79	53	73	97	24	.238	.351	.373	.591	.126	1.778	S. 84° 48′ W., 202	5.3	5.1	4.3	46.62	47.06	49.81	63.7	23.5
November . . .	79	61	73	96	33	.192	.195	.204	.361	.090	5.225	N. 41° 5′ W., 309	6.5	6.8	6.5	32.43	32.23	33.82	5.00	16.1
December . . .	80	69	78	100	33	.169	.172	.188	.424	.071	5.459	N. 34° 26′ W., 245	7.6	7.2	6.3	28.82	30.80	31.11	60.3	10.9
Annual means .	75	55	70	100	18	.330	.340	.351	.975	.023	41.059	N. 61° 40′ W., 180	5.8	5.9	4.6	42.34	43.32	44.36	78.5	—13.5
Winter	75	58	70	96	26	.153	.161	.165	.526	.042	10.582	N. 59° 2′ W., 277	6.4	6.0	4.6	25.82	27.23	27 95	60.4	—0.7
Spring	70	51	65	97	18	.235	.259	.255	.691	.023	11.113	N. 27° 25′ W., 123	6.4	6.1	5.1	35.85	37.48	37.94	68.3	—13.5
Summer . . .	74	53	71	95	31	.609	.608	.634	.975	.319	10.816	S. 82° 39′ W., 127	5.2	5.8	3.7	63.97	63.82	65.13	75.5	46.7
Autumn . . .	79	55	73	97	24	.327	.337	.353	.792	.090	8.592	N. 71° 43′ W., 233	5.2	5.3	4.4	44.55	44.88	46.76	72.3	16.1
For seven years.		58		100	16		.344		.975	.023	43.153	N. 73° 33′ W., 215					44.40		78.5	—13.5

TABLE I.—*Mortality for the year* 1858, *Collated from*

DISEASES.	FIRST QUARTER, COMMENCING JANUARY 2, 1858.									SECOND COMMENCING					
	Jan.		Feb.		March.		Adults.	Minors.	Total.	April.		May.		June.	
	M.	F.	M.	F.	M.	F.				M.	F.	M.	F.	M.	F.
Abscess	5	1	1	4	2	3	10	6	16	1	1	..	..	1	..
Albuminuria	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Aneurism	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Apoplexy	5	1	6	4	11	1	27	1	28	3	6	6	5	8	7
Asphyxia	..	..	..	..	..	..	..	..	..	..	1	4	1	2	..
Asthma	..	..	..	..	1	..	1	..	1	2	..	..	..	2	1
Cancer and scirrhus	2	7	1	6	3	6	23	2	25	1	6	3	8	4	9
Caries	..	1	..	..	..	..	..	1	1	..	..	..	..	..	..
Catarrh	4	1	..	..	4	3	..	12	12	2	3	2	2	..	1
Casualties	13	6	13	5	17	6	40	20	60	11	7	6	3	10	6
Burns and scalds	1	3	1	..	4	6	5	10	15	1	2	..	2	2	1
Drowned	6	..	3	1	4	..	13	1	14	8	2	12	..	16	..
Fracture	1	..	1	..	1	1	3	1	4	..	..	..	..	..	..
" of pelvis	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..
Poisoning	1	..	..	..	..	..	..	1	1	..	..	..	..	..	..
Suicide	2	..	1	..	2	5	9	1	10	1	..	..	2	2	..
Cholera infantum	..	1	..	..	2	..	..	3	3	2	1	4	1	32	29
" morbus	..	..	..	1	1	1	1	2	3	1	..	2	1	2	2
Cirrhosis	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Childbed	..	..	..	1	..	..	1	..	1	..	..	..	..	..	..
Concussion of the brain	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..
Congestion of the brain	11	11	6	6	5	5	14	30	44	8	10	11	3	15	11
" " liver	1	..	..	..	..	..	..	1	1	..	..	..	..	..	..
" " lungs	2	1	..	7	3	4	8	9	17	2	5	5	4	2	8
Consumption of the lungs	77	79	63	70	84	85	391	67	458	86	69	53	77	81	67
Convulsions	21	30	22	27	31	26	11	146	157	28	10	14	21	46	31
Croup	20	12	21	13	17	22	..	105	105	8	8	9	6	14	12
Cyanosis	4	1	2	..	1	2	..	10	10	2	1	..	2	4	2
Coup de soleil	..	..	..	..	..	..	..	..	..	..	..	..	..	20	3
Coxalgia	..	..	..	..	..	..	..	..	..	2	1	..	..	..	..
Constipation	..	..	..	..	..	1	1	..	1	..	..	..	..	..	..
Debility	15	23	17	17	28	20	76	44	120	15	20	19	14	12	8
Diabetes	..	..	..	..	..	..	..	..	..	1	..	..	..	1	..
Diarrhœa	2	3	..	3	2	2	6	6	12	3	..	5	3	1	..
Disease of the bladder	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
" " brain	6	2	4	4	9	4	8	21	29	7	4	14	2	14	8
" " chest	..	..	..	..	..	..	..	..	..	..	..	..	..	2	1
" " heart	5	11	8	11	14	12	36	25	61	9	4	8	9	13	11
" " liver	3	4	..	..	7	4	12	6	18	2	3	5	8	5	2
" " lungs	3	..	3	2	3	2	7	6	13	3	4	2	1	..	..
" " spine	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
" " stomach and bowels	1	1	..	..	2	2	5	1	6	4	2	..	..	..	..
Dropsy	18	12	17	11	16	15	37	52	89	3	3	..	..	..	..
" of the brain	1	1	..	4	12	13	3	28	31	15	11	15	9	23	14
" " chest	..	..	..	..	3	3	6	..	6	7	7	8	10	4	4
" " heart	..	..	..	1	..	1	1	1	2	1	..	1	3	4	2
Dysentery	1	2	2	..	4	4	7	6	13	1	1	3	2	7	11
Dyspepsia	..	..	..	..	1	..	..	1	1	..	..	..	..	..	..
Effusion on the brain	6	2	3	3	3	3	2	18	20	2	2	3	..	3	2
Enlargement of the heart	..	..	..	1	..	1	..	2	2	..	..	..	1	..	..
Epilepsy	1	1	..	..	..	..	1	1	2	..	..	..	..	..	..
Erysipelas	2	3	4	4	5	8	17	9	26	3	2	2	2	4	4
Fever, bilious	..	1	1	..	1	2	5	..	5	..	..	..	1	2	..
" brain	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..
" congestive	1	..	..	..	..	..	..	1	1	..	..	..	..	..	..
" intermittent	..	1	..	1	..	..	1	1	2	..	..	..	..	..	..
" nervous	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..
" puerperal	..	..	..	..	..	1	1	..	1	..	7	..	3	..	7
" remittent	..	..	1	1	..	..	1	1	2	..	1	1	1	..	..
" scarlet	21	21	18	20	19	21	1	119	120	9	18	18	9	6	8
" typhoid	8	3	9	10	14	7	33	18	51	5	8	3	4	4	5
" typhus	4	1	4	3	5	4	11	10	21	4	2	2	3	2	2
" " icterodes	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Gangrene	..	..	1	1	2	1	3	2	5	..	..	..	..	..	1
Gout	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Hemorrhage	2	..	3	..	3	2	6	4	10	4	5	2	2	1	4
Hernia	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..
Hooping-cough	1	3	4	4	2	8	..	22	22	5	8	4	3	8	21
Hydrophobia	..	..	..	..	1	..	1	..	1	..	..	..	..	..	..

Returns made to the Health Office. By WILSON JEWELL, M. D.

QUARTER, APRIL 3, 1858.			THIRD QUARTER, COMMENCING JULY 3, 1858.									FOURTH QUARTER, COMMENCING OCTOBER 2, 1858.									Total for the year.
			July.		Aug.		Sept.					Oct.		Nov.		Dec.					
Adults.	Minors.	Total.	M.	F.	M.	F.	M.	F.	Adults.	Minors.	Total.	M.	F.	M.	F.	M.	F.	Adults.	Minors.	Total.	
2	1	3	4	..	..	1	..	1	5	1	6	1	2	..	..	3	..	4	2	6	31
..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	1	..	1	1
..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	1	..	..	1	1	2	2
32	3	35	6	1	7	3	4	3	23	1	24	2	1	7	5	3	6	24	..	24	111
..	8	8	4	3	5	..	3	..	1	14	15	2	..	..	1	7	4	4	10	14	37
5	..	5	..	1	1	..	..	1	2	1	3	1	..	..	..	..	2	3	..	3	12
27	3	30	2	7	4	17	3	10	36	7	43	1	8	5	8	3	2	24	3	27	125
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
..	10	10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	22
28	15	43	11	3	5	4	21	6	23	27	50	6	3	9	2	17	1	25	13	38	191
..	8	8	..	1	..	1	3	..	1	4	5	2	5	2	5	5	8	6	21	27	55
25	13	38	26	1	9	1	19	1	35	22	57	10	..	4	2	2	..	15	3	18	127
..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	1	..	1	5
1	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
4	1	5	3	2	..	1	..	1	7	..	7	2	1	1	..	..	..	4	..	4	26
..	69	69	151	128	117	102	39	38	..	575	575	4	8	..	3	..	..	..	15	15	662
5	3	8	12	11	5	7	4	2	34	7	41	..	..	1	..	..	..	1	..	1	53
..	..	..	..	..	..	..	1	..	1	..	1	..	..	..	..	..	..	..	..	..	1
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
1	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
16	42	58	19	20	16	22	14	6	34	63	97	1	6	7	7	16	5	19	23	42	241
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
10	16	26	2	5	1	1	3	3	10	5	15	4	1	1	3	5	6	9	11	20	78
381	52	433	55	62	47	68	72	87	350	41	391	45	55	51	55	92	79	359	18	377	1659
10	140	150	30	48	23	25	28	18	14	158	172	23	25	24	16	20	22	8	122	130	609
1	56	57	7	3	5	8	6	4	1	32	33	13	9	22	11	20	22	..	97	97	292
1	10	11	..	..	1	2	2	1	..	6	6	4	2	..	1	3	2	..	12	12	39
22	1	23	3	..	..	..	..	..	2	1	3	..	..	..	..	..	..	..	..	..	26
2	1	3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
51	37	88	20	22	24	12	38	17	60	73	133	13	14	20	13	27	29	55	61	116	457
2	..	2	1	..	..	..	..	..	1	..	1	..	..	..	..	..	..	..	..	..	3
6	6	12	32	23	19	13	8	9	24	80	104	3	1	1	2	3	..	7	3	10	138
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	1	..	1	1
19	30	49	7	6	6	5	10	1	11	24	35	4	..	5	2	5	5	12	9	21	134
3	..	3	..	1	..	..	..	1	1	1	2	1	2	..	..	1	2	5	1	6	11
40	14	54	9	6	8	10	12	5	34	16	50	4	5	10	11	10	10	40	10	50	215
19	6	25	..	..	3	3	3	2	8	3	11	2	2	3	2	3	2	12	2	14	68
4	6	10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	23
..	..	..	1	..	1	..	1	..	1	2	3	..	..	..	1	..	1	..	2	2	5
4	2	6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	12
4	2	6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	95
4	83	87	21	17	19	19	9	7	2	90	92	10	8	7	7	7	12	6	45	51	261
36	4	40	4	9	7	6	8	9	37	6	43	5	7	9	6	11	13	50	1	51	140
10	1	11	2	2	1	2	2	1	8	2	10	1	..	1	1	1	..	4	..	4	27
8	17	25	33	28	43	41	23	18	53	133	186	2	3	1	3	4	3	7	9	16	240
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
4	8	12	9	3	4	4	1	5	5	21	26	2	3	3	2	3	1	5	9	14	72
..	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2
12	5	17	2	1	2	2	4	..	4	7	11	3	..	..	..	1	..	2	2	4	58
3	..	3	4	1	7	5	6	1	18	6	24	3	4	2	1	1	..	9	2	11	43
..	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2
1	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
15	2	17	..	6	..	5	..	2	11	2	13	..	2	..	..	..	3	5	..	5	36
..	3	3	..	..	..	..	2	3	2	3	5	1	1	3	2	..	..	6	1	7	17
4	64	68	4	3	4	4	..	5	1	19	20	7	7	3	3	7	6	2	31	33	241
21	8	29	7	3	13	9	15	10	35	22	57	14	9	7	5	14	11	46	14	60	197
10	5	15	5	4	3	1	2	4	14	5	19	1	3	2	1	5	4	13	3	16	71
..	..	..	..	..	7	2	4	3	14	2	16	..	..	..	..	..	..	..	..	..	16
..	1	1	3	..	1	..	3	2	1	8	9	..	..	..	1	..	..	..	1	1	16
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	1	..	1	1
14	4	18	3	1	2	1	3	4	9	5	14	1	3	1	3	1	3	7	5	12	54
..	1	1	..	..	..	..	..	..	..	..	..	1	..	1	1	..	1	3	1	4	5
..	49	49	7	8	15	13	5	11	1	58	59	5	5	2	6	2	3	..	23	23	153
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1

TABLE I.—*Mortality*

DISEASES.	FIRST QUARTER, COMMENCING JANUARY 2, 1858.									SECOND COMMENCING					
	Jan.		Feb.		March.		Adults.	Minors.	Total.	April.		May.		June.	
	M.	F.	M.	F.	M.	F.				M.	F.	M.	F.	M.	F.
Icterus	..	2	3	..	..	1	3	3	6	2	..	..	1	..	..
Inanition	3	4	1	3	3	7	3	18	21	4	2	4	1	6	3
Inflammation of the bladder	1	..	..	1	..	..	1	1	2	1	..	..	..	..	..
" " brain	16	11	13	8	16	8	19	53	72	22	5	9	6	16	14
" " bronchi	3	5	2	5	5	4	11	13	24	5	7	3	3	7	5
" " chest	..	..	1	..	..	..	..	1	1	..	..	..	..	..	..
" " heart	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..
" " kidneys	1	..	1	..	..	..	2	..	2	..	1	..	..	..	..
" " liver	..	1	4	3	..	..	7	1	8	1	..	..	..	3	2
" " lungs	18	6	34	15	30	31	48	86	134	25	16	28	24	29	25
" " peritoneum	1	..	..	3	..	4	5	3	8	3	6	3	2	3	4
" " pleura	..	1	..	1	..	..	2	..	2	..	..	..	..	..	..
" " stomach & bowels	5	7	8	7	12	10	24	25	49	5	12	10	10	17	12
" " throat	..	..	..	..	2	..	..	2	2	2	2	..	..	..	..
Intemperance	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..
Intussusception	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Malformation	1	..	..	..	..	..	..	1	1	..	..	..	..	..	..
Mania	..	..	..	1	..	..	1	..	1	..	1	1	..	..	..
Mania à potu	7	..	4	2	6	2	..	..	21	4	..	3	2	10	1
Marasmus	10	8	16	12	21	13	3	77	80	20	17	18	8	27	20
Measles	..	..	..	1	..	..	..	1	1	..	1	..	2	2	2
Mortification	..	..	..	..	1	3	..	4	4	..	1	..	..	1	1
Obstruction of the bowels	..	..	..	..	..	..	..	..	..	2	..	..	..	..	..
Old age	3	8	7	14	9	15	56	..	56	18	17	18	19	20	30
Osteo-sarcoma	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..
Neuralgia	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Palsy	2	1	7	5	4	5	24	..	24	4	2	3	2	8	4
Purpura	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Rheumatism	2	2	..	..	4	4	10	2	12	5	6	1	..	1	..
Scrofula	1	1	1	3	7	4	9	8	17	1	..	3	3	2	3
Smallpox	..	..	..	..	1	..	1	..	1	..	..	1	..	1	1
Softening of the brain	..	..	..	..	1	..	1	..	1	1	..	..	1	..	..
Sore throat	1	1	1	1	..	..	..	4	4	..	..	..	..	..	..
Stillborn	31	11	32	22	31	25	..	152	152	25	19	24	15	31	20
Syphilis	..	..	1	..	..	..	..	1	1	..	..	..	1	..	1
Tabes mesenterica	1	3	1	3	5	4	1	16	17	2	1	..	2	1	2
Teething	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
Tetanus	..	..	..	..	1	..	1	..	1	1	1	..	..	2	..
Tumours	..	..	..	..	..	1	1	..	1	..	1	1	1	..	1
Strangury	2	1	1	..	1	1	1	5	6	..	2	..	..	..	..
Ulceration	..	..	..	1	..	..	1	..	1	..	..	..	..	..	..
Unknown	6	7	8	2	17	8	32	16	48	10	5	10	7	13	7
Worms	..	..	1	..	..	..	..	1	1	..	..	1	..	..	..
Uremia	..	..	..	..	..	..	..	..	..	..	..	2	1	..	..
Totals of the sex	392	330	387	359	526	467	1133	1328	2461	437	371	390	342	580	464
Monthly totals	722		746		993		..	..	..	808		732		1044	
From the almshouse	39		43		54		..	..	..	49		61		79	
From the country	..		3		..		..	..	..	8		5		13	
People of colour	39		65		61		..	..	..	71		42		72	

for 1858—Continued.

QUARTER, APRIL 3, 1858.			THIRD QUARTER, COMMENCING JULY 3, 1858.									FOURTH QUARTER, COMMENCING OCTOBER 2, 1858.									Total for the year.
Adults.	Minors.	Total.	July.		Aug.		Sept.		Adults.	Minors.	Total.	Oct.		Nov.		Dec.		Adults.	Minors.	Total.	
			M.	F.	M	F.	M.	F.				M.	F.	M.	F.	M.	F.				
2	1	3	..	..	2	1	..	..	1	2	3	1	..	..	2	..	1	2	2	4	16
6	14	20	8	3	10	4	5	4	4	30	34	..	3	1	4	5	3	..	16	16	91
1	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3
13	59	72	29	14	23	14	21	13	11	103	114	3	8	14	8	12	12	14	43	57	315
7	23	30	2	6	2	3	5	6	11	13	24	4	2	1	6	2	7	14	8	22	100
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
1	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
1	..	1	..	..	..	..	1	1	2	..	2	..	2	..	..	..	..	1	1	2	7
6	..	6	2	2	1	1	3	3	9	3	12	..	..	..	1	1	2	4	..	4	30
42	105	147	23	17	19	22	21	12	15	99	114	18	16	23	20	52	38	36	131	167	562
13	8	21	4	2	3	3	..	3	12	3	15	1	1	1	1	4	3	7	4	11	55
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2
33	33	66	23	17	12	11	17	24	52	52	104	4	10	4	7	16	13	40	14	54	273
..	4	4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	6
1	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
..	..	..	..	..	..	..	..	1	1	..	1	..	..	1	..	..	..	1	..	1	2
..	..	..	1	..	..	..	..	..	..	1	1	..	..	..	..	..	..	..	..	..	2
2	..	2	2	..	..	2	1	..	5	..	5	..	..	..	1	..	..	1	..	1	9
20	..	20	7	..	9	3	10	2	30	1	31	3	1	5	1	3	..	13	..	13	85
11	99	110	31	31	55	45	29	24	11	204	215	12	10	7	7	14	8	8	50	58	463
..	7	7	2	3	4	2	..	..	1	10	11	..	1	4	..	3	1	..	9	9	28
1	2	3	..	..	1	..	..	..	1	..	1	..	2	..	..	..	..	1	1	2	10
1	1	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2
122	..	122	19	22	12	12	15	18	98	..	98	7	12	12	13	11	27	82	..	82	358
..	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	1	..	1	1
22	1	23	5	5	5	3	6	2	25	1	26	7	4	2	5	6	8	31	1	32	105
..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	1	1	1
10	3	13	1	..	..	1	1	1	2	2	4	2	..	1	1	..	1	4	1	5	34
3	9	12	4	3	3	..	6	2	6	12	18	2	..	..	3	1	1	..	7	7	54
1	2	3	..	..	1	1	1	..	..	3	3	..	..	..	..	..	..	..	..	..	7
2	..	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	4
..	134	134	18	15	18	12	27	24	..	114	114	25	17	24	20	23	26	..	135	135	535
1	1	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3
1	7	8	4	7	3	1	4	4	2	21	23	1	..	3	2	2	..	..	8	8	56
..	1	1	1	1	2	2	2	2	..	10	10	..	..	1	..	..	..	..	1	1	12
1	3	4	1	..	2	2	1	..	4	2	6	1	..	..	..	1	..	1	1	2	13
4	..	4	..	1	..	..	..	4	4	1	5	..	..	..	..	..	..	..	..	..	10
1	1	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	8
..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
34	18	52	12	13	8	6	9	7	31	24	55	6	4	5	3	7	3	18	10	28	183
..	1	1	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	1	..	1	3
3	..	3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3
1233	1351	2584	708	602	630	571	566	459	1272	2264	3536	303	299	327	298	467	422	1086	1030	2116	10697
..	..	..	1310		1201		1025		..	..	..	602		625		889		..	..	..	..
..	..	..	58		58		51		..	..	..	30		24		27		..	..	..	573
..	..	..	9		1		6		..	..	..	3		1		3		..	..	..	52
..	..	..	70		50		46		..	..	..	31		26		38		..	..	..	611

TABLE I.—*Mortality for* 1858—Continued.

	FIRST QUARTER, COMMENCING JANUARY 2, 1858.						SECOND QUARTER, COMMENCING APRIL 3, 1858.					
	Jan.	Feb.	March.	Adults.	Minors.	Totals.	April.	May.	June.	Adults.	Minors.	Totals.
Under 1 year . . .	194	186	248	..	..	628	199	193	303	..	..	695
From 1 to 2 years . .	116	96	113	..	..	325	85	57	105	..	..	247
" 2 to 5 " . .	38	56	89	..	..	183	66	60	99	..	..	225
" 5 to 10 " . .	22	23	39	..	..	84	31	16	28	..	..	75
" 10 to 15 " . .	13	11	14	..	..	38	13	11	9	..	..	33
" 15 to 20 " . .	18	26	27	..	..	71	23	21	31	..	..	75
" 20 to 30 " . .	89	77	112	..	..	278	87	92	102	..	..	281
" 30 to 40 " . .	68	92	90	..	..	250	75	72	100	..	..	247
" 40 to 50 " . .	48	48	68	..	..	164	64	59	76	..	..	199
" 50 to 60 " . .	35	35	61	..	..	131	44	44	56	..	..	144
" 60 to 70 " . .	35	45	49	..	..	129	52	47	56	..	..	155
" 70 to 80 " . .	31	28	49	..	..	108	43	35	39	..	..	117
" 80 to 90 " . .	14	20	22	..	..	56	22	23	33	..	..	78
" 90 to 100 " . .	1	2	11	..	..	14	4	2	7	..	..	13
" 100 to 110 " . .	..	1	1	..	..	2	..	..	..	..	..	..
Total of monthly mortality	722	746	993	..	..	..	808	732	1044	..	..	..
Total males for the quarter	..	..	..	..	..	1305	..	..	..	..	..	1407
" females "	..	..	..	..	..	1156	..	..	..	..	..	1177
" adults "	..	..	..	1133	..	..	..	..	..	1233	..	..
" minors "	..	..	..	..	1328	..	..	..	..	..	1351	..
" for the quarter . .	..	..	..	..	..	2461	..	..	..	..	..	2584

	THIRD QUARTER, COMMENCING JULY 3, 1858.						FOURTH QUARTER, COMMENCING OCTOBER 2, 1858.					
	July.	Aug.	Sept.	Adults.	Minors.	Totals.	Oct.	Nov.	Dec.	Adults.	Minors.	Totals.
Under 1 year . . .	584	516	305	..	..	1405	173	174	231	..	..	578
From 1 to 2 years . .	145	156	116	..	..	417	51	55	78	..	..	184
" 2 to 5 " . .	95	69	61	..	..	225	40	51	68	..	..	159
" 5 to 10 " . .	28	17	16	..	..	61	12	9	15	..	..	36
" 10 to 15 " . .	22	19	18	..	..	59	13	5	13	..	..	31
" 15 to 20 " . .	22	40	36	..	..	98	11	10	27	..	..	48
" 20 to 30 " . .	99	95	119	..	..	313	65	76	94	..	..	235
" 30 to 40 " . .	81	57	99	..	..	237	68	62	87	..	..	217
" 40 to 50 " . .	59	74	81	..	..	214	54	46	80	..	..	180
" 50 to 60 " . .	57	52	62	..	..	171	47	38	67	..	..	152
" 60 to 70 " . .	49	45	39	..	..	133	31	45	37	..	..	113
" 70 to 80 " . .	43	37	45	..	..	125	20	33	53	..	..	106
" 80 to 90 " . .	20	22	22	..	..	64	10	11	31	..	..	52
" 90 to 100 " . .	5	1	5	..	..	11	7	8	8	..	..	23
" 100 to 110 " . .	1	1	1	..	..	3	..	1	..	..	..	1
" 110 to 120 " . .	..	..	..	..	..	..	..	1	..	..	..	1
Total of monthly mortality	1310	1201	1025	..	..	..	602	625	889	..	..	..
Total males for the quarter	..	..	..	..	..	1906	..	..	..	..	..	1097
" females "	..	..	..	..	..	1630	..	..	..	..	..	1019
" adults "	..	..	..	1272	..	..	..	..	..	1086	..	..
" minors "	..	..	..	..	2264	..	..	..	..	..	1030	..
" for the quarter . .	..	..	..	..	..	3536	..	..	..	..	..	2116

TABLE II.—*Mortality from Diseases of the Lungs and Air-passages.*

DISEASES.	ANNUAL AGGREGATES.			QUARTERS OF 1858.			
	1856.	1857.	1858.	1st.	2d.	3d.	4th.
Abscess, pulmonary	1	...	...	...	...	...	...
Angina pectoris	5	1	...	...	...	...	...
Apnœa	1	...	...	...	...	...	...
Asphyxia	28	15	37	...	8	15	14
Asthma	21	33	12	1	5	3	3
Catarrh	...	33	22	12	10	...	...
Collapse of lungs	1	...	...	...	...	...	...
Congestion of the lungs	97	132	78	17	26	15	20
Consumption, laryngeal	1	...	...	...	...	...	...
" of the lungs	1501	1544	1659	458	433	391	377
Croup	268	256	292	105	57	33	97
Disease of the chest	6	7	11	...	3	2	6
" lungs	37	53	23	13	10	...	...
Dropsy of the chest	61	48	140	6	40	43	51
Effusion on the chest	7	3	...	...	...	...	...
" lungs	4	4	...	...	...	...	...
Emphysema	2	1	...	...	...	...	...
Empyema	2	1	...	...	...	...	...
Gangrene of the lungs	...	1	...	...	...	...	...
Hemorrhage from the lungs	28	18	...	...	...	...	...
Influenza	2	9	...	...	...	...	...
Inflammation of the bronchi	250	179	100	24	30	24	22
" " chest	7	16	1	1	...	...	...
" " larynx	34	14	...	...	...	...	...
" " lungs	379	504	562	134	147	114	167
" " pleura	16	27	2	2	...	...	...
" " trachea	9	11	...	...	...	...	...
Ulceration of larynx	2	...	...	...	...	...	...
Totals	2770	2910	2939	773	769	640	757
Hooping-cough	77	51	153	22	49	59	23
Totals	2847	2961	3092	795	818	699	780
Total mortality, exclusive of stillborn	11722	10338	10162				
Per cent. from diseases of the lungs	22.77	28.13	30.43				
Per cent. from consumption of the lungs	12.80	14.93	16.33				

TABLE III.—*Deaths from Consumption of the Lungs, during each Month in the year* 1858, *at fourteen distinct periods of life, with the Sexes designated for each month.*

AGES.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
Under 1 year . . .	3	2	...	2	1	1	...	...	...	...	...	...	9
From 1 to 2 years . .	7	2	3	2	...	...	...	...	...	...	...	...	14
" 2 to 5 " . .	...	1	3	3	2	1	...	...	...	...	...	...	10
" 5 to 10 " . .	4	1	5	2	1	3	1	1	...	...	...	...	18
" 10 to 15 " . .	3	1	1	3	3	1	3	2	1	...	...	...	18
" 15 to 20 " . .	6	16	9	11	5	11	3	19	11	6	2	10	109
" 20 to 30 " . .	45	48	58	43	50	52	41	31	64	27	45	50	554
" 30 to 40 " . .	34	33	38	39	31	37	25	26	37	32	22	44	398
" 40 to 50 " . .	25	11	15	25	19	25	16	20	18	11	15	26	226
" 50 to 60 " . .	13	9	18	14	9	12	17	12	17	15	9	25	170
" 60 to 70 " . .	10	8	16	7	9	3	7	4	7	3	12	9	95
" 70 to 80 " . .	6	1	2	3	...	2	3	...	3	1	1	5	27
" 80 to 90 " . .	...	...	1	1	...	...	1	...	1	3	...	2	9
" 90 to 100 " . .	...	...	...	...	...	...	...	...	...	2	...	...	2
" 100 to 110 " . .	...	...	...	...	...	...	...	...	...	...	...	...	...
Male	77	63	84	86	53	81	55	47	72	45	51	92	806
Female	79	70	85	69	77	67	62	68	87	55	55	79	853
Monthly totals . . .	156	133	169	155	130	148	117	115	159	100	106	171	1659
Quarterly totals . . .	458			433			391			377			1659

TABLE IV.—*Mortality from Diseases of the Nervous System.*

DISEASES.	ANNUAL AGGREGATES.			QUARTERS OF 1858.			
	1856.	1857.	1858.	1st.	2d.	3d.	4th.
Abscess of the brain	2	...	...	...	...	...	...
Apoplexy	123	115	111	28	35	24	24
Chorea	2	1	...	...	...	...	...
Catalepsy	1	...	...	...	...	...	...
Coma	2	1	...	...	...	...	...
Compression of the brain	8	...	...	...	...	...	...
Concussion of the brain	...	7	1	...	1	...	...
Congestion of the brain	177	201	241	44	58	97	42
Convulsions	603	556	609	157	150	172	130
Coup de soleil	2	6	26	...	23	3	...
Cramp	11	10	...	...	...	...	...
Disease of the brain	107	100	134	29	49	35	21
Dropsy of the brain	242	173	261	31	87	92	51
Effusion of the brain	94	92	72	20	12	26	14
Epilepsy	24	18	2	2	...	...	...
Hysteria	3	...	...	...	...	...	...
Inflammation of the brain	329	306	315	72	72	114	57
" " tympanum	...	...	...	...	...	...	...
Irritation of brain and spinal marrow	...	...	...	...	...	...	...
Mania or insanity	12	7	9	1	2	5	1
Mania à potu	43	62	85	21	20	31	13
Neuralgia	1	...	1	...	...	...	1
Palsy	104	92	105	24	23	26	32
Softening of the brain	40	14	3	1	2	...	...
" " spinal marrow	2	...	...	...	...	...	...
Teething	29	17	12	...	1	10	1
Tetanus	13	11	13	1	4	6	2
Trismus	2	2	...	...	...	...	...
Totals	1976	1791	2000	431	539	641	389
Puerperal convulsions	2	1	...	...	...	...	...
" mania	...	...	...	...	...	...	...
Totals	1978	1792	2000	431	539	641	389
Total mortality, exclusive of stillborn	11722	10338	10162				
Per cent. of total mortality	16.87	17.33	19.58				

TABLE V.—*Mortality from Diseases of the Organs of Nutrition.*

DISEASES.	ANNUAL AGGREGATES.			QUARTERS OF 1858.			
	1856.	1857.	1858.	1st.	2d.	3d.	4th.
Abscess	40	35	31	16	3	6	6
" abdominal	...	...	...	...	...	...	...
" of the liver	1	1	...	...	...	...	...
Cancer of the stomach and bowels	4	7	...	...	...	...	...
Cholera	16	3	...	...	...	...	...
" infantum	722	534	662	3	69	575	15
" morbus	35	10	53	3	8	41	1
Cirrhosis of the liver	6	3	...	...	...	...	...
Colic	17	7	...	...	...	...	...
Constipation	3	...	1	1	...	...	...
Consumption of the bowels	3	5	...	...	...	...	...
Diarrhœa	149	119	138	12	12	104	10
Disease of the liver	27	41	68	18	25	11	14
" stomach and bowels	27	17	12	6	6	...	...
Dropsy	213	239	95	89	6	...	...
" abdominal	8	6	...	...	...	...	...
Dysentery	301	198	240	13	25	186	16
Dyspepsia	...	2	1	1	...	...	...
Gout	7	5	1	...	...	...	1
Icterus	21	11	16	6	3	3	4
Ileus	1	...	...	...	...	...	...
Inflammation of the liver	39	25	30	8	6	12	4
" " peritoneum	68	56	55	8	21	15	11
" " stomach and bowels	184	299	273	49	66	104	54
Intussusception	6	4	2	...	...	1	1
Marasmus	484	506	463	80	110	215	58
Obstruction of the bowels	9	2	2	...	2	...	...
Scrofula	61	51	54	17	12	18	7
Tabes mesenterica	36	44	56	17	8	23	8
Tuberculosis	...	...	...	...	...	6	...
Ulceration of the stomach and bowels	15	6	...	...	...	...	...
Totals	2503	2236	2253	347	382	1314	210
Total mortality, exclusive of stillborn	11722	10338	10162				
Per cent. of total mortality	21.35	21.62	22.17				

TABLE VI.—*Mortality from Diseases of the Urino-Genital Organs.*

DISEASES.	ANNUAL AGGREGATES.			QUARTERS OF 1858.			
	1856.	1857.	1858.	1st.	2d.	3d.	4th.
Albuminuria	9	7	1	...	...	...	1
Amenorrhœa	2	...	...	...	...	...	...
Cancer of mamma	2	...	...	...	...	...	...
" uterus	5	4	...	...	...	...	...
Childbed	2	7	1	1	...	...	...
Chlorosis	2	1	...	...	...	...	...
Convulsions, puerperal	2	1	...	...	...	...	...
Diabetes	...	7	3	...	2	1	...
Disease of the bladder	1	1	1	...	...	...	1
" kidneys	16	8	...	...	...	...	...
" ovaries	1	2	...	...	...	...	...
" prostate gland	1	...	...	...	...	...	...
" uterus	5	2	...	...	...	...	...
Dropsy, ovarian	1	...	...	...	...	...	...
Fever, puerperal	69	49	36	1	17	13	5
Hemorrhage from uterus	7	5	...	...	...	...	...
Inflammation of the bladder	8	9	3	2	1	...	...
" " kidneys	7	7	7	2	1	2	2
" " prostate gland	...	...	...	...	...	...	...
" " uterus	15	8	...	...	...	...	...
Mania, puerperal	...	...	...	...	...	...	...
Rupture of the bladder	...	...	...	...	...	...	...
" urethra	...	1	...	...	...	...	...
" uterus	...	...	...	...	...	...	...
Stone in bladder	...	...	...	...	...	...	...
Strangury	...	...	8	6	2	...	...
Suppression of urine	...	3	...	...	...	...	...
Syphilis	5	2	3	1	2	...	...
Tumour, ovarian	1	1	...	...	...	...	...
Ulceration of the uterus	...	1	...	...	...	...	...
Totals	161	126	63	13	25	16	9
Total mortality, exclusive of stillborn	11722	10338	10162				
Per cent. of total mortality	1.37	1.21	0.62				

TABLE VII.—*Mortality from Fevers.*

DISEASES.	ANNUAL AGGREGATES.			QUARTERS OF 1858.			
	1856.	1857.	1858.	1st.	2d.	3d.	4th.
Fever	10	3	...	...	...	...	...
" bilious	24	25	43	5	3	24	11
" cerebral	...	2	1	...	1	...	...
" congestive	3	5	1	1	...	...	...
" continued	...	2	...	...	...	...	...
" enteric	...	2	...	...	...	...	...
" gastric	2	4	...	...	...	...	...
" hectic	2	2	...	...	...	...	...
" intermittent	14	5	2	2	...	...	...
" malignant	...	...	...	...	...	...	...
" nervous	7	9	1	...	1	...	...
" pernicious	...	...	...	...	...	...	...
" puerperal	69	49	36	1	17	13	5
" remittent	22	23	17	2	3	5	7
" scarlet	992	704	241	120	68	20	33
" typhoid	229	175	197	51	29	57	60
" typhus	49	38	71	21	15	19	16
" " icterodes	5	...	16	...	...	16	...
" yellow	...	...	...	...	...	...	...
Totals	1428	1048	626	203	137	154	132
Total mortality, exclusive of stillborn	11722	10338	10162				
Per cent. of total mortality	12.18	10.13	6.16				

TABLE VIII.—*Mortality from Measles.*

DISEASES.	ANNUAL AGGREGATES.			QUARTERS OF 1858.			
	1856.	1857.	1858.	1st.	2d.	3d.	4th.
Measles	141	56	28	1	7	11	9

TABLE IX.—*Causes assigned for Death where the number is* 90 *and upward.*

DISEASES.	ANNUAL AGGREGATES.			QUARTERS OF 1858.			
	1856.	1857.	1858.	1st.	2d.	3d.	4th.
Apoplexy	123	115	111	28	35	24	24
Casualties	158	118	191	60	43	50	38
Cholera infantum	722	534	662	3	69	575	15
Consumption of the lungs	1501	1544	1659	458	433	391	377
Congestion of the brain	177	201	241	44	58	97	42
" " lungs	97	132	...	...	...	...	...
Convulsions	603	556	609	157	150	172	130
Croup	268	256	292	105	57	33	97
Debility	430	378	457	120	88	133	116
Diarrhœa	149	119	138	12	12	104	10
Disease of the brain	107	100	134	29	49	35	21
" heart	179	222	215	61	54	50	50
Dropsy	213	239	95	89	6	...	...
" of the brain	242	173	261	31	87	92	51
Dysentery	301	198	240	13	25	186	16
Effusion on the brain	94	92	...	...	...	...	...
Erysipelas	100	95	...	...	...	...	...
Fever, scarlet	992	704	241	120	68	20	33
" typhoid	229	175	197	51	29	57	60
Hooping-cough	...	...	153	22	49	59	23
Inanition	114	...	91	21	20	34	16
Inflammation of the brain	329	306	315	72	72	114	57
" " bronchi	250	179	100	24	30	24	22
" " lungs	379	504	562	134	147	114	167
" " stomach and bowels	184	299	174	49	66	104	54
Marasmus	484	506	463	80	110	215	58
Measles	141	...	...	...	...	...	...
Old age	172	195	358	56	122	98	82
Palsy	104	92	105	24	23	26	32
Smallpox	390	...	...	...	...	...	...
Stillborn	612	557	535	152	134	114	135
Unknown	257	291	183	48	52	55	28
Drowned	105	134	127	14	38	57	18

TABLE X.—*Deaths, with the Sexes, for each Month in the Year, and the Number at Sixteen Distinct Periods of Life, with the Percentages to Total Mortality, Exclusive of the Stillborn—also the Stillborn Children for each Month, and their Sexes.*

MONTHS.	STILLBORN.			Males.	Females.	Boys.	Girls.	Under 1 year.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	110 to 120.	Total.
	M.	F.	Total.																					
January	31	11	42	392	330	228	173	194	116	38	22	13	18	89	68	48	35	35	31	14	1	...	...	722
February	32	22	54	387	359	208	190	186	96	56	23	11	26	77	92	48	35	45	28	20	2	1	...	746
March	31	25	56	526	467	282	248	248	113	89	39	14	27	112	90	68	61	49	49	22	11	1	...	993
April	25	19	44	437	371	218	199	199	85	66	31	13	23	87	75	64	44	52	43	22	4	...	...	808
May	24	15	39	390	342	214	144	193	57	60	16	11	21	92	72	59	44	47	35	23	2	...	...	732
June	31	20	51	580	464	322	253	303	105	99	28	9	31	102	100	76	56	56	39	33	7	...	...	1044
July	18	15	33	708	602	486	410	584	145	95	28	22	22	99	81	59	57	49	43	20	5	1	...	1310
August	18	12	30	632	569	443	374	516	156	69	17	19	40	95	57	74	52	45	37	22	1	1	...	1201
September	27	24	51	566	459	305	247	305	116	61	16	18	36	119	99	81	62	39	45	22	5	1	...	1025
October	25	17	42	303	299	151	149	173	51	40	12	13	11	65	68	54	47	31	20	10	7	...	...	602
November	24	20	44	327	298	160	144	174	55	51	9	5	10	76	62	46	38	45	33	11	8	1	1	625
December	23	26	49	468	421	229	203	231	78	68	15	13	27	94	87	80	67	37	53	31	8	...	...	889
Totals	309	226	535	5716	4981	3246	2734	3306	1173	792	256	161	292	1107	951	757	598	530	456	250	61	6	1	10697
Per ct. of totals				53.45	46.55	30.35	25.62	37.25	11.54	7.78	2.52	1.57	2.86	10.89	9.35	7.45	5.88	5.21	4.48	2.46	0.60	.05	.009	

Table I. The whole number of deaths returned for the year has been 10,697. This amount indicates a falling off of 201, or nearly 2 per cent., from the deaths of 1857. Fixing the population at 600,000, which, I believe, is a fair estimate, the death returns furnish a favourable indication of the health of our city, and give one death to every 56 of its inhabitants.

Of the deaths recorded, 5,715 were males and 4,982 females, presenting an excess of deaths in the male sex equivalent to 14.70 per cent. For the last ten years the mortality records of Philadelphia give an excess of male deaths equal to 15.58 per cent. The average of deaths for each quarter of the year is 2,674; for each month $891\frac{5}{12}$; for every day $29\frac{1}{3}$.

According to the seasons of the year, so are the rates of death determined for each quarter. The first quarter furnished 2,461, the second 2,584, the third 3,536, and the fourth 2,116. The second or spring quarter yielded an increase of 5 per cent. over those of the first quarter, embracing two of the winter months. The third quarter, which rates the highest in mortality, and includes the summer and autumn in part, gave 36.76 per cent. more than the second quarter, and 43.60 per cent. than the first quarter. The fourth or last quarter, in which the deaths were reduced to 2,116, showed a decrease of 40.15 per cent. from those in the third. A large proportion of the increase of mortality in the third quarter may be safely charged to the diseases of the organs of nutrition, embracing many of the zymotic class.

Appended to this table will be found the number of deaths returned from the almshouse, or Blockley Hospital; they amount to 573. Also the mortality among the coloured population, which was 611. The number of interments in the city burial-grounds of bodies brought from other counties and States amounted to 52. I make these statements from the record, but their accuracy is very doubtful. From a more reliable source I learn that while there has been an annual increase of the inmates of the Blockley Almshouse—our great pauper establishment—the rate of mortality shows a reduction from 21.89 per cent. in 1854 to 14.50 per cent. in 1858.

In order to arrive at a safer estimate of the deaths from diseases alone, it seems proper to deduct those in the table registered under external causes, as well as those from debility, malformation, old age, unknown causes, stillborn, and those from the country, amounting in all to 1,967, or 18 per cent. of the whole. By this calculation we reduce the deaths from certified diseases to 8,730. Taking this as the correct standard of deaths from disease during the year, and it gives us only 1 death in every 67 of the population.

Table II. presents the deaths returned to the health office from those diseases to which the lungs and air-passages are liable. They amount to 3,092,

or 30.43 per cent. of the entire number of deaths for the year.[1] This percentage is higher by 2.30 per cent. than the preceding year, and shows an increase of deaths from this class over any former year, in proportion to the annual mortality.

The deaths from croup have increased 14 per cent. over those for 1857, and amount to 292, or 9.44 per cent. of the mortality from diseases of the air-passages. The deaths in the first and fourth quarters, which include the winter months, more than double those for the second and third quarters of the year. The steady increase of croup, according to the tables of mortality for this city, cannot have escaped the attention of the Fellows of the College. It is one of those diseases peculiar to childhood, and most prevalent between the ages of one and five years; it is not often met with in early infancy, although during this year 110 of the deaths were under one year of age. Boys are said to be more liable to croup than girls, according to the known statistics of writers. The present year fully confirms this opinion. 160 of the deaths were boys and 130 girls, showing an increase of $23\frac{1}{2}$ per cent. of the former over the latter. The distinct character of the disease causing death, whether pseudo-membranous or catarrhal croup, is rarely designated in the certificate, and hence it is impossible to ascertain the comparative mortality of the two forms of this frequent and too often fatal disease among children. A more complete system of classification of diseases, as well as of registration of deaths, would, if practically carried out, supply a necessary want in our mortality statistics of this as well as other diseases.

The deaths from dropsy of the chest present an increase of 191.73 per cent. above those recorded for 1857. This augmentation will be found to belong to the last half of the year, and occurred principally among adults. The cause in all probability may be looked for in the unusual prevalence of inflammatory affections of the bronchial passages and lungs, the result of a catarrhal epidemic influence in the atmosphere.

This catarrhal disease furnished 140 deaths, or 4.52 per cent. of the mortality assigned to this class.

Inflammation of the lungs contributed 562, or 18 per cent., of the deaths in this table, and inflammation of the bronchia 100, or 3 per cent. Only two deaths are ascribed to inflammation of the pleura, one to inflammation of the chest, none to that of the larynx or trachea. This is an unusual occurrence.

Hooping-cough will be seen to have supplied more deaths than for the two previous years combined, viz., 153, an increase of 200 per cent. over those for 1857.

The extreme prevalence of catarrhal diseases in our city during the last

[1] It will be understood that the calculations in these tables having reference to the entire mortality are prepared by excluding the stillborn, unless otherwise expressed.

four months of the year, amounting almost to an epidemic influenza, may in some degree account for the increase of deaths as indicated in the table.

Table III. In this table I have set apart the deaths from consumption of the lungs which have been recorded during the year. I have also designated the sexes, and have shown the total of deaths for every decennial period.

The annual mortality from this dreaded scourge of the human family is placed at 1,659. Of this number, 806 were males and 853 females; the excess of deaths among females is peculiar to all mortality tables in all places. In the present instance it was equal to about 6 per cent.

The deaths from this disease contribute more than half of those classed with diseases of the lungs and air-passages, while they furnish 16.33 per cent. of the total mortality.

The increase of the deaths from consumption above those for 1857 is equal to 7.45 per cent.

Estimating the population at 600,000, the ratio of deaths to population was as 1 to every 361.66, or 2.76 in every thousand.

According to this table, and it does not differ materially from others that I have prepared for this region, the decennial period in which the deaths from consumption of the lungs are most frequent is that between 20 and 30 years. The next in frequency is between 30 and 40. This result would confirm the opinions of Sir James Clark and of Louis on this point.

There is no period of life, however, even from infancy (which interesting season is thought to be very prolific of consumption) down to extreme old age, in which there will not be found recorded, deaths from the ravages of this hereditary pestilence. In the accompanying table you will find 9 deaths under 1 year, and 2 over 90 years; while the intervening periods gave larger proportions, marking a gradual increase up to 30 years, when they as gradually declined.

The highest monthly mortality was in December, 171; the next March, 169; the next September, 159. October and November furnish the lowest number—100 in the former, and 106 in the latter.

Table IV. contains the numerical estimate of the deaths during the year from diseases appertaining to the nervous system. They amount to 1,990, or 19.58 per cent. of the entire mortality.

Under the separate titles of convulsions and congestion, dropsy, inflammation, and disease of the brain, will be found 1,560 deaths. These diseases belong peculiarly to children, and we may safely infer that 78 per cent. of the deaths from affections of the nervous system are contributed by our infantile population.

Convulsions, by itself, gives a large proportion, equal to 609, or 6 per cent.

Twenty-six deaths are noticed from coup de soleil, or sunstroke. Twenty-three of the deaths occurred in June, and of these, twenty-one were in the last week of the month; during this term, the thermometer stood at its highest point for the year, 96½ on the 28th. These deaths were the result of the extreme heat of the season.

The deaths from mania-à-potu were 85, an increase of 37 per cent. above those for 1857, and a larger annual mortality than for several years back.

Table V. enumerates the deaths for the year from diseases peculiar to the organs of nutrition, amounting to 2,253, and equal to 22.17 per cent. of the entire mortality.

Under this head is placed cholera infantum, a summer disease of infancy, confined principally to the crowded suburbs and badly-ventilated districts of our large cities and towns. The third quarter of the year, embracing July, August, and September, together with the month of June, furnishes almost the entire mortality. The whole number for the year was 662, equivalent to about 14 per cent. of the deaths for the year, in children under five years of age.

Marasmus (?).—Under this term will be found 463 deaths; the months of June, July, August, and September furnishing 262 of this number. This also is one of those diseases—or, more properly, results of disease—peculiar to children, and found prevailing extensively in large cities during the warm season. It seems to be a favourite name with many physicians, and an easy mode of certifying to deaths of a chronic form in children where doubt exists in the mind as to the real nature of the case. I believe it to be the sequel of several varieties of disease of the organs of nutrition in children; and, with that understanding, the numerous deaths ascribed to marasmus obviously belong to those affections, and are placed to their account.

The third quarter of the year, which includes almost the entire warm season, contributes a large portion of the deaths under this head. It will be seen, also, that the increase is from those affections which are generally classified with the zymotic or epidemic. The entire number for the quarter is 1,314, constituting 58 per cent. of the deaths in this class; while the epidemic diseases alone, in the quarter, are 1,093, or 48 per cent.

Table VI. Here will be found the deaths assigned to those diseases which belong to the kidneys, bladder, and the organs of generation. They consist, this year, of a limited percentage when compared with the whole mortality. There is a falling off of one-half from those of last year; they number 63. Of these, puerperal fever deaths make 36, or more than half. In 1857 the largest proportion of deaths from this fever were in the first quarter, the coldest period of the year. This year, almost the entire mortality was in warm weather.

Table VII. The mortality from fevers during 1858 is shown in this table.

A comparison with the fever table of 1857 is the best evidence of the exemption of our city in 1858 from the destructive influence of this class of diseases upon its citizens. The former year they numbered 1,048, in the latter 626, presenting a decrease equal to 40 per cent. In 1856 the proportion of deaths from fevers to the entire mortality was 1 in 8; this year (1858) they are only 1 in 16.

This great difference is explained in part by the falling off in the deaths from scarlet fever. In the two years preceding 1858 they amounted to 1,696; during this year they were only 241.

To the entire mortality, fever deaths rate only 6 per cent.

Typhoid and typhus fevers have increased in the number of their deaths since 1857 at the rate of 25 per cent.

If the record of deaths be considered a fair index, our city has been peculiarly exempt from the ordinary forms of autumnal fevers. The few cases I have observed invariably partook of a mild type, and were easily managed. Intermittent fever and its varieties, considered as of miasmatic origin, which were exceedingly prevalent, a few years ago, in the urban as well as in the suburban districts during the fall months, were neither fatal nor numerous. It has been remarked that this more common type of fever seemed to be gradually disappearing from our vicinity as an endemic, or to be yielding in form to those of an asthenic character, as the typhoid or enteric.

After an experience of thirty-four years, and a somewhat familiar acquaintance with the endemics of the county, I have never known the outer districts of our city to have enjoyed a larger share of health than during the past year, especially as regards the ravages from fever. The common bilious fevers have been few in number; and when found, their active inflammatory type was doubtful, and features were presented, which corresponded with those above alluded to, of an enteric character.

One of the local causes of this comparative immunity of the suburban districts of the city from miasmatic fevers, in addition to the meteorological or climatic changes may be found, especially during the past two years, in the almost entire suspension of those rapid and extensive improvements which for years have been in progress along the outskirts of the city. In the prosecution of these changes, streets were opened and graded, which required the digging down of embankments and the filling up of excavations; the removal of earth was needed also from cellars dug for new buildings, and in the levelling of lots.

The great amount of loose and new earth, thus exposed to the influence of heat and moisture, and the decomposition of vegetable remains contained therein, in connection with numerous collections of stagnant water, could not do otherwise than give rise to the exhalation of malaria, which, with other causes, highly favoured the appearance and extensive prevalence

of fevers. When the money panic of 1857 swept over the country, our city experienced its prostrating influence; business suddenly declined, property depreciated in value, building improvements at once ceased, and with these reversions an abatement of miasmatic fevers followed.

Sixteen deaths are recorded from typhus icterodes. As far as I could obtain correct information, these deaths were from undoubted cases of yellow fever. They all occurred in the months of August and September.

About the middle of July there was considerable anxiety in the community, in consequence of the arrival and detention at quarantine of a number of vessels from ports in the West Indies, where yellow fever was prevailing at the time of their sailing. Several of these vessels had cases of the fever on board in a most malignant form, when they dropped their anchors opposite the lazaretto. They received a visit from the physician at the station, Dr. L. S. Filbert, and the sick were immediately removed to the hospital. Other vessels had lost several of their seamen with yellow fever on the voyage, at sea, or in foreign ports, while others again arrived with their crews in health, having had no sickness during the voyage, but were detained for purification. In a few days, cases of yellow fever were received into the hospital from some of these vessels.

Every sanitary precaution was strictly observed by the officers at the station, to prevent the spread of the disease among the crews of the fleet riding quarantine, and to preserve the health of those persons remaining on board the infected vessels. These were moored at some distance north and east of the others, which were detained, not on account of sickness, nor because they came from infected ports, but because it was according to law.

Dr. Filbert received into the hospital from time to time, in all, twenty-seven cases of yellow fever. Four of these died; two within ten hours after admission. Two of the four had black vomit. This was a mortality of only fourteen per cent. to cases, and shows a result more favourable than is usual. The treatment followed by the doctor in these cases he has had the kindness to forward me, which I herewith transcribe in his own words.

"The treatment, which I am happy to say has proved very successful in my hands, was as follows: As soon as the patients (except the two who arrived in a dying condition) were received into the hospital, I had them immersed in a hot mustard-bath, after which, upon being wiped dry, they were put to bed, well covered with blankets, and bottles of hot water placed around their bodies and lower extremities, with the intention of producing diaphoresis. Entire rest in the recumbent position was strictly enjoined, and maintained throughout the treatment. As early as possible I adminstered the following remedies: ℞.—Hydr. chlor. mit. gr. vj; sulph. quin. gr. v; pulv. nitr. potas. gr. v; pulv. ipecac. gr. $\frac{1}{3}$. This prescription was repeated every three hours. The treatment had the desired effect of

producing copious perspiration. In from three to four days the fever abated.

"My object was to give a sufficient amount of calomel to move the bowels without the addition of other purgatives, at the same time to lay a mercurial foundation at the start. It may not be out of place to say, that in no instance did my patient give me trouble where mercurial action could be produced in due time. In those cases where there was much gastric distress, I applied mustard to the epigastric region. Ice was allowed, as much as the patient desired."

The great solicitude felt by the lazaretto physician, for a faithful discharge of his duties under the trying circumstances in which he found himself placed, and his unremitted attention to the sick, were calculated to depress the energies of his nervous system; while his frequent visits to the infected ships from day to day, in order that those left on board might have his protection, exposed him to the influence of a poisoned atmosphere, and he contracted the fever, which proved to be a mild case. He convalesced on the sixth or seventh day of his sickness.

Unfortunately two infected vessels escaped the vigilance of the officers at the lazaretto, and either evaded the laws or the restrictions of the Board of Health, or else through a misunderstanding, had received permits to leave and thus found their way to the city, where they discharged their cargoes and their crews. In a few days after their arrival, several cases of yellow fever made their appearance among the persons employed about these vessels, and in the vicinity of their moorings.

It has not been possible to ascertain the true number or exact locality of cases of yellow fever that were under treatment during its limited appearance in our city. The Board of Health, as I learn, preserved no record of the cases reported to them, and, besides, there were instances of the disease that never came to their knowledge.

From the most reliable information, there were between thirty and thirty-five cases. A number of them occurred in the vicinity of South and Front Streets. Several were in Kensington; while a few were under treatment in one or two of our public hospitals. Many of these cases could be traced to direct communication with the atmosphere of the vessels that had reached our wharves, coming directly from yellow fever ports, without having undergone quarantine; while with others no satisfactory information could be obtained as to the origin of the disease.

Twenty-five of these cases of fever were under the care of Dr. John Gegan, of South Front Street. His first case occurred two days after the arrival of the first vessel at Lombard Street wharf, in a baker, residing in Lombard Street near Front, who told the doctor he had been about the wharf for several days previous to his sickness. Five of the cases under the care of Dr. Gegan died—a mortality of twenty per cent. All of his

cases, with the exception of the night inspector of one of the above infected vessels, were in houses east of Second and south of Lombard Streets.

It becomes, therefore, a difficult question to decide upon the origin of these cases of yellow fever. Were they the offspring of a foreign poison, introduced by vessels from the West Indies, or are they to be regarded as the production of domestic causes? Were they the result of epidemic or endemic influences? That they were of sporadic or accidental origin, will, perhaps, be admitted. That the disease neither lasted until a frost should banish it from our midst, nor spread to any extent, are facts well known. The hygrometrical and the thermometrical conditions of the atmosphere were by no means favourable to its production, or administered to its extension; hence, the activity of the cause, whatever it might be, was soon exhausted for the want of these and other propitious circumstances.

The extensive prevalence of scarlet fever in our city for a number of years past, the fearful ravages it has committed by death in that portion of the population which, of all others, is the most interesting while it is the most cherished, and the increased attention that has been bestowed upon it of late by writers on medicine, would seem to claim for it, at our hands, rather more than an ordinary notice.

For this purpose I have taken the trouble to compile, from the mortality records, the following table, which gives us the deaths from scarlet fever in our city for a period of twenty-eight years, from 1831 to 1858, inclusive, with the ages and sexes for each decennial period:—

YEARS.	Males.	Females.	Boys.	Girls.	Under 1 year.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Total.
From 1831 to 1840, ten years	968	1036	945	1010	181	353	915	433	58	15	25	14	4	6	..	..	2004
" 1841 to 1850, ten "	1267	1318	1255	1284	249	461	1216	545	52	16	26	12	6	2	..	..	2585
" 1851 to 1858, eight "	1740	1735	1712	1707	329	687	1574	722	82	25	33	17	4	..	1	1	3475
	3975	4089	3912	4001	759	1501	3705	1700	192	56	84	43	14	8	1	1	8064

During the twenty-three years preceding 1831, beginning with 1807, when the first annual statement of deaths in our city was published by the Board of Health—as prior to this event we have no authentic account of the mortality—there were only 162 deaths from scarlet fever. Within this interval of time there was a series of years, from 1812 to 1818, that a death from scarlet fever was unknown to the record; and from 1807 until 1812, only 13 deaths are to be found on the register.

In 1830 the deaths increased to 40, from 9 the previous year, and from the following year, 1831, when, as the above table indicates, they run up to 200, we may date the advent of this wide-spread and severe exanthe-

matous fever. Not only has it prevailed extensively, and with marked fatality, but during each successive decennial period it has steadily increased, provided the increased ratio of deaths be taken as the index of its prevalence.

All writers agree that no period is fixed for the duration of scarlet fever when it makes its appearance in an epidemic form, and it would not be an assumption of authority to assert, that the past twenty-eight years have proved a memorable epidemic cycle, especially when contrasted with the preceding period of twenty-three years, during which term only 162 deaths from this disease were returned out of 56,000 deaths. It has been conjectured by Dr. Emerson that in the period of exemption referred to, there may have been deaths from scarlet fever reported under the title of sore throat, which in all probability was the case. Making every allowance, however, for the 355 additional deaths thus recorded, it would not, in my opinion, change the proportional mortality between the two periods—since the like conjecture, for the returns of deaths from scarlet fever during the epidemic visitation, by other and vague terms, would far exceed those of the former period.

A careful inspection of the table will reveal several points of interest involved in the history and character of this dreaded malady. That it is a disease of infancy and childhood, scarce a doubt need be entertained. Of the 8,064 deaths which have occurred in this city since 1831, 7,665, or 95 per cent. were under ten years of age. Of the deaths recorded beyond 20 years, or in adult life, there were 151, not quite 2 per cent. Eight of these were between 50 and 60, one between 60 and 70, and one between 70 and 80 years of age, proving that the disease may attack the aged and terminate fatally.

Some diversity of opinion has been expressed as to the influence of scarlet fever upon the sexes. While one author decides that girls are more liable to it than boys, another claims the influence for the boys; while a third believes that under puberty, sex exerts no influence whatever, but beyond that age it is most frequently found among females.

It may be thought impossible, with our present imperfect and limited means of observation, to decide correctly this question. Should it never be settled, it is not one of vital importance. Nevertheless, I am of opinion that females are more subject to the disease than boys. The evidence for this opinion will be found in the accompanying table, unless it can be satisfactorily shown that the result there given of the excess of female deaths, was occasioned by a less resistance to a fatal termination of the disease, owing to their greater delicacy of conformation.

Of the deaths recorded in this table, 4,089 were females, and 3,975 were males, presenting an excess equal to 3 per cent. of female deaths over those in the male sex. This result, while it may conflict with the estimate of a

number of able writers, is in accordance with the experience and decision of the Registrar-General of England.

Table VIII. For several years a separate table has been prepared for measles, smallpox, and varioloid. The falling off in the deaths from smallpox and varioloid since 1855 has rendered a distinct notice unnecessary. Only seven deaths are registered from smallpox during the entire year, and of these victims, five were children. Not a death took place from varioloid. For measles the table has been continued, although it might with propriety be dispensed with, in the present instance, as the deaths from this exanthem have reached only 28 during the year. No epidemic of measles has prevailed, and the few isolated cases which made their appearance were generally mild in character. The last two quarters of the year show an increase in the deaths, the disease no doubt having been complicated with catarrhal inflammations, which were quite prevalent.

Table IX. In this group are those only, in which the deaths have exceeded ninety for the year.

A glance at the table will indicate a change in favour of 1858, as to its comparative health, with the results of the previous years appended thereto.

Consumption of the lungs produces the highest mortality. Cholera infantum is the next in the scale.

Disease of the heart, which in all probability stands for a variety of distinct morbid affections, the true diagnosis of which has either been overlooked through indifference, or waived in the absence of a knowledge of exploration according to modern investigations, numbers 215 of the entire mortality from diseases of the organs of circulation, which were 248.

The remarkable increase of deaths from old age, seen in this table, numbering 358, when compared with those in former years, will not escape attention. They more than double the number for either 1855 or 1856, and nearly double those for 1857. I shall not attempt any explanation, as it may be purely an accidental occurrence, although it might serve as an argument in favour of an approximation towards a higher average of human life in our city.

Table X. This is a useful table for the statistician. It presents at first sight an analysis of the annual mortality, numerically arranged. The stillborn, with the sexes for each month, are given. The deaths per month at each of the fifteen distinct periods of life, with the sexes, and the number of boys and girls under twenty years of age, are also enumerated. A calculation of percentages of deaths for each month to the whole number has been prepared, as well as for the several designated periods of life, all of which will be found available in making up comparative tables.

The stillborn children amounted to 535, which would be equal to 5 per cent. of the entire mortality for the year. This is a more favourable comparison than was presented last year, or for the seven previous years. The male stillborn exceeded the female by 36.72 per cent. This fact of the excess of stillborn males is noticed in the statistics of other cities.

The largest number of stillborn occurred in March—viz., 56; the lowest in August—viz., 30. The coldest months yielded the greatest amount of stillborn children.[1]

The mortality in children, or those deaths which have occurred before the twentieth year of life,[2] exclusive of stillborn, were 5,443, and make up 53.38 per cent. of the deaths. Those under one year, deducting the stillborn, amounted to 2,767, equivalent to 27.25 per cent. of the mortality, and constitute more than half of all the deaths under twenty years.

The deaths under five years amounted to 4,731, or 46.40 per cent. of the annual mortality. Between five years and twenty they were less than 7 per cent.

The mortality in infancy and childhood in our city will compare favourably with that of other large cities; still, the large number of deaths at these interesting periods of life demand a far greater share of attention from the medical profession and the corporate authorities than they now receive. It is a fact, and cannot be denied, that the prevalent causes for this heavy mortality in early life are preventable. The improper hygienic management of children, with regard to their diet, dress, exercise, and air, together with a neglect by the civil authorities of the enforcement of sanitary police measures, are mainly the agents that lay the foundation for the existence of infantile diseases, and thus invite this early harvest of death.

In adult life, that decade between 20 and 30 gave the highest mortality; it was equal to 10.89 per cent. The deaths beyond this period gradually declined, up to extreme old age. Two hundred and fifty deaths, or 2.46 per cent., took place in the decennial period between 80 and 90, sixty-one between 90 and 100, and seven died in their centennial term, one of whom was beyond 110 years.

The proportion of deaths in advanced life, when compared with the total mortality, and with the like proportion in former statistics, shows that the probabilities of life have been increased during the year. In the previous year (1857), although the mortality was greater in amount than this year, yet the aggregate ages were only 221,327 years, which made the duration of life $20\frac{1}{3}$ years; whereas this year (1858) the sum of the ages amounted to 238,585, which would increase the average existence of life to $23\frac{1}{2}$ years.

[1] No estimate can be formed of the number of stillborn to the births in our city, as the Board of Health have failed in securing the birth statistics for several years.

[2] In the tables these deaths are classified as minors, and those above twenty as adults.

Adhesive Plaster in maintaining Counter-extension in the Treatment of Oblique Fractures of the Lower Extremity.—Dr. GILBERT read the following paper on the use of adhesive plaster in maintaining counter-extension in the treatment of oblique fractures of the thigh and of the leg:—

From the time of Hippocrates to the present these fractures have occupied the mind of the profession to a greater extent perhaps than any other; yet surgeons of the largest experience, who occupy the most prominent positions in the profession, have told us, and even now tell us, that a large majority of the cases of fracture of the femur result in shortening and permanent lameness. John Bell said: "The machine is not yet invented by which a fractured thigh can be perfectly recovered." Benj. Bell said: "An effectual method of securing oblique fractures of the thigh-bone is perhaps one of the greatest desiderata in modern surgery." Ferguson says: "The fractured thigh is almost invariably shorter than its fellow." Chelius says: "Fractures of the thigh are always difficult of cure. There is most commonly deformity and shortening of the limb." With these views his learned British and American editors fully agree. Malgaigne says, in the work recently translated by Dr. J. H. Packard of this city: "When the two ends cannot be made to oppose one another to counteract the muscular contractions, it is impossible to preserve the normal length of the limb, whatever may be the apparatus or method employed." Again: "When the fragments remain in contact, or when we can replace them and keep them so by means of their serrations, it is easy to cure a fracture of the femur without shortening; in the absence of these two conditions the thing is simply impossible." Dr. V. Mott, of New York, testified, in a trial for malpractice, a few years ago, that "more or less shortening of the limb is uniformly the result after fractured thigh, even under the most favourable circumstances;" and Drs. Parker, Post, Cheeseman, and others, sustained the same view in their testimony. Dr. F. H. Hamilton, of Buffalo, N. Y., after furnishing the most interesting and reliable statistics of the results of the treatment of this fracture on record, says: "I declare, in the most positive manner, that I have never obtained like results [such as claimed by those who profess to have made perfect cures] either in the use of my own apparatus or with that of others." To this almost any amount of corroborative testimony might be added.

A full history of all the contrivances invented and used in the mechanical treatment of this fracture, if written, would constitute a large volume, every page of which would impress the mind of the reader with the fact that the treatment of this lesion is surrounded by difficulties of the gravest character.

The retentive apparatus of the ancients consisted of bandages of cerecloth and some rude attempts in the use of splints and fracture-boxes, in the straight position, depending entirely upon lateral pressure for the maintenance of the fragments in apposition. Percival Pott, who wrote after the middle of the eighteenth century, was the first to recommend the bent

posture of the fractured limb. His object was to relax the muscles, and thus obviate their mischievous effects in causing shortening. He laid the limb in carved angular splints, and placed the patient upon his injured side, resting the sound limb upon the fractured one. Dupuytren, Astley Cooper, Chas. Bell, and others, improved upon this by introducing the double inclined plane, and placing the patient upon his back, thus using the body to keep up counter-extension by its weight. Desault had previously adopted the straight method, adding a long outside splint, with extending and counter-extending bandages. These two general methods, viz., the angular and the straight, have been varied in every conceivable manner by practitioners of all countries, each projector of a new plan supposing that a great improvement had been achieved, only, however, to be superseded by the modification of some other surgeon. At present it is conceded pretty generally, that, in the greatest number of cases, the extended plan, using an outside splint long enough to reach up nearly to the axilla, meets all the indications most satisfactorily. As in most of our surgical procedures, the simplicity and constant availability of this plan constitute the measure of its value. The common apparatus is composed of a long splint, having a block projecting inwards six or eight inches below the sole of the foot; extending and counter-extending bandages; cotton batting, or junk bags, and common roller. Short splints may or may not be used. In the use of this all the muscles are equally relaxed, and their contractions are controlled more effectually by the extending and counter-extending forces employed than by any other method in ordinary use. Although all the indications *seem* to be met by this simple apparatus, yet statistics abundantly prove that many failures occur in practice. This failure of complete success by the straight method has long been attributed by surgical writers to a want of coincidence between the line of counter-extension and the axis of the limb and body. To remedy this, various modifications in the application of the counter-extending power have been proposed, and alterations made, in the several forms of apparatus. Dr. Physick improved upon the plan of Desault by extending the outside splint to the axilla, which, as will appear presently, was a real improvement. Van Houte attached a bar of wood, which projected at right angles from the upper end of the splint, and extended to the middle of the lower part of the abdomen. To this the counter-extending bandage was fastened so as to make traction in a line coinciding directly with the axis of the body. Volpe added an inner splint, and joined this to the inner extremity of the cross-piece, as a further improvement. Others, since then, have been influenced by the same idea in constructing their apparatus, some of whom abut the upper extremity of a long inside splint against the tuber ischii.

It ought to be borne in mind, however, that in a well-formed lower extremity the inner condyles of the thigh-bones come together at the knees; whilst the trochanters, by the interposition of the pelvis and the necks and

heads of the thigh-bones, are about twelve inches asunder. The shafts of the tibiæ are usually parallel with each other, and their axes, when extended upwards, coincide very nearly with the axis of the trunk. The axis of the femur, however, which diverges from the knee upwards and outwards, forms an angle with the general line of the axis of the leg and body. The application of the counter-extending power, therefore, to coincide with the axis of the femur, should be made from the perineum upwards and outwards, and not in a line directly upwards. It is very evident, then, that Dr. Physick's modification of Desault's apparatus disposes of the extending and counter-extending forces in a manner which insures their coincidence with the axes of the thigh and leg more accurately than any other plan yet proposed, and is decidedly the most useful so long as loose or unadherent bandages are used for counter-extension. The extending power exerted directly downwards, coinciding with the axis of the leg, and the counter-extending power upwards and outwards, coinciding with the axis of the femur, as nearly as may be, will exert their influence in controlling the muscular contractions more effectually, and in retaining the fragments in apposition more accurately, than if the two forces acted in a line with the axis of the trunk. The salient angle formed by the axes of the femur and tibia, where they meet at the knee, is concealed by the arrangement of the muscular mass placed at the upper and inner part of the femur. The inequalities of the external contour of the limb are filled up by the wadding, or junk bags, so that the splint supports the whole extremity firmly, although extension and counter-extension are not in the same line with each other.

Why, it may then be asked, are there so many imperfect cures, even by the use of this most approved method in the hands of the most skilful? Why are limbs shorter, and patients lame for life, when we possess an apparatus which appears to meet so fully and clearly every indication as a means of retention after reduction and coaptation of the broken bone? These questions may be answered more satisfactorily after we consider briefly some of the anatomical elements which are involved in this lesion.

The femur is the largest bone of the skeleton. It is simple in its outline, and well defined in all its parts. It constitutes the upper half of the column upon which the body reposes, and, at the same time, is one of the chief agents through which all the movements of the body are performed. These diverse functions of absolute rest and free motion are provided for by the form and position of the bone. The gentle forward arching of the shaft, the neck and head diverging from this at nearly a right angle, and the salient inclination of the entire bone downwards and inwards from the sides of the pelvis, admirably fit it for the extensive and varied movements which it is destined to perform; whilst its strong cylindrical body, terminating at each extremity in large processes, forming its base and capital, fits it equally well to perform the office of a column for the support of the

trunk. The muscles appropriated to the thigh-bones are remarkable for their number, size, diverse traction, and far-reaching attachments, upwards above the pelvis, and downwards to the foot. When we consider the fact that these, in their aggregate, comprise nearly, if not fully, one-half of the muscular mass of the entire system, the magnitude of their power in steadying and moving the bone becomes very apparent. Between the bone and these muscles, during their integrity, there is the most accurate adaptation of function, and the most perfect harmony of action.

When, however, fracture in any part of the continuity of the former, takes place, all this beautiful symmetry of form and reciprocity of action is destroyed. The contractions of this most voluminous and energetic of all the muscular combinations in the system are powerfully and continually exerted in effecting derangement of the fragments, and disturbing their quiet apposition. In view of these anatomical and physiological facts, we ought not to be surprised, then, that the experience of all the world forces upon us the conviction that these powerful muscular contractions are not sufficiently controlled, to make cures without shortening, by the provision of any apparatus hitherto in use for counter-extension. I admit that all desirable and necessary power is at our command through the use of any of the most approved modifications of apparatus on the extended plan; but its tension cannot be endured by the patient long enough, at the seat of counter-extension, to secure union without deformity. When well adjusted, the limb is restored to its normal length, and the patient feels quite comfortable for a time. To the eye of the observer every indication appears to be fully met; but the loose counter-extending bandage, or the crutch-head of the inner splint, no matter how wide a surface they were made to occupy when first applied, will narrow down under the powerful contractions of the muscles, the pressure will become intensely concentrated over the bony points of the pelvis underlying the line of their application, and insupportable pain will be the consequence. In a large proportion of cases, in which the patient resolves to indure the pain, abrasion, excoriation, and even sloughing, will be produced by these counter-extending means, no matter how soft the cushion or unirritating the materials of which they are composed. It is a well-authenticated fact that Gen. Lafayette owed his lameness to excoriation and deep-seated ulceration produced in his perineum and groin by the counter-extending means used in the treatment of his fracture. He doubtless obeyed the orders of his surgeon not to disturb the apparatus, and endured his sufferings like a soldier, regardless of consequences. There are very few patients, however, so obedient. Malgaigne prefers the apparatus of Boyer; yet, in his opinion, in oblique fractures even this "cannot restore the limb to its normal length," in proof of which he cites a case in which "extension was kept up steadily until the forty-eighth day, when it had to be abandoned on account of the *deep ulcerations about the ankle and groin.*" He explains by asserting that the power used to counteract mus-

cular action "causes pressure or constriction in proportion to the resisting force of the muscles," which, he says, "rarely fails to cause acute pain, excoriations, blisters, or even sloughs." Hence the infinite variety in the form and composition of the perineal band; in the fashioning of the upper extremity of the inner splint, by which counter-extension is made against the perineum; in the method of treating by the flexed position, with or without weights and cords passing over pulleys; and, finally, in the application of dextrine or starch; all with the same intention of saving the perineum and groin from the effects of pressure and friction. It would require more space than can well be appropriated in this communication to describe the numerous plans proposed for the defence of the perineum. To defend this part has been a principal desideratum with surgeons, and has, more than any other consideration, led to the endless modification of apparatus in the treatment of this fracture. Ordinarily, it is found, at the first visit after any of the forms of retentive apparatus in use has been adjusted, that the patient or his friends have slackened the counter-extending means, or are anxiously waiting for the surgeon to do it. In most of the cases this has to be done, and the seats of pressure and friction bathed with spirits, and soft materials interposed, so as, if possible, to relieve the pain and prevent abrasion. The same round of efforts, to abate suffering and ward off injury, must be repeated from time to time; in the meanwhile there is no constantly acting force sufficiently powerful to overcome muscular contraction, and, finally, union of overlapping bones takes place. To restrain patients in their efforts to loosen the counter-extending means, various plans have been proposed. Amongst the most recent, Dr. John Neill proposes the union of the extending and counter-extending bands outside of the long splint, partly for this purpose; and Dr. J. F. Flagg, of Boston, suggests that a padlock be put on the buckle of the counter-extending band whenever the patient persists in undoing it.

After encountering difficulties of this character during a practice of eighteen years, in the treatment of fractures of the thigh, the very favourable impression made upon my mind by the use of adhesive plaster in keeping up *extension* without pain, led me to a determination to use it for *counter-extension* as soon as an opportunity offered. This presented itself very soon afterwards in a case of so complicated a character as to afford a very fair test of the value of the plan. *This first case in which adhesive plaster was used as a means of counter-extension*, may be found recorded in the January number of the *American Journal of the Medical Sciences* for 1851. Since then I have used it in every case of fracture, not only of the thigh but of the *leg*, for counter-extension, as well as extension, with the happiest results. In the January number of the same journal for 1858 a number of cases of fracture of both thigh and leg, selected from my general practice, will be found published; in all of which it is shown most conclusively that any amount of force necessary to overcome muscular con-

traction, may be used without causing pain or excoriation at the seats of its application. This may be explained in the following manner: The adhesive plaster counter-extending bands become firmly adherent to a large extent of integument, consequently there can be no friction upon its surface; and through this extensive union with the skin, *pressure is widely and evenly diffused.* In the use of any of the ordinary unattached counter-extending means, the extent of surface occupied at the seat of pressure does not exceed eight square inches; a fractional part only of which sustains its greatest intensity, viz: that which overlies the tuber ischii, the edge of its ascending ramus, and a narrow space of the body of the pubis. The extent of surface to which the adhesive plaster counter-extending bands are attached, on the other hand, amounts to about one hundred square inches, over all of which the tension and pressure are equally distributed. In the use of the former, all the tissues lying upon the points of bone mentioned endure constant pressure, amounting often to constriction; in the use of the latter, through the elasticity of the skin, and the extensive distribution of the tractive power, pressure is slight and painless. In the use of the former, friction is produced continually by the movements of the body or limb; in the use of the latter, friction is impossible. The former glides over the surface, and acts as a ligature; the latter being adherent, cannot act thus. The former does not fix the pelvis; the latter holds it firmly, and keeps all the parts steady from the chest to the foot. The former requires the daily attention of the surgeon, to relieve suffering and prevent abrasion; the latter requires no such attention, unless the bands lose their attachment, which ordinarily does not occur more than once during the whole period of treatment. In short, by the adhesive counter-extending bands pressure is completely neutralized, friction cannot occur so long as they remain adherent, perfect quietude of the fragments is maintained; the union, consequently, requires less time, and less attention from the surgeon, and the patient is entirely free from the annoyance and suffering inseparable from the ordinary methods, no matter how great the power used to overcome the muscular contractions, or how protracted the period required for union in complicated cases.

Adhesive plaster has been used for years as a means of keeping up *extension.* Dr. F. H. Hamilton, in his report to the American Medical Association, *vide Transactions* for 1857, says: "The adhesive plaster bands are beyond all comparison the best means of making permanent extension which are at present known to surgeons. Hitherto one of the most serious difficulties in the way of extension, and the objection which has been most effectively urged against its adoption, has been the excoriations, ulcerations, and even sloughing, which so often occurred from the use of the various extending bands about the ankle. This, together with the injuries occasionally inflicted by the *perineal band,* has been regarded as a sufficient reason for preferring the flexed position. But no one who

has employed the adhesive plaster extending bands will doubt that so far as injuries to the foot and ankle are concerned, this objection is entirely disposed of." Again, he says: "I regard this simple invention, therefore, as one of the most important improvements in the treatment of fractures of the thigh."

An experience of eleven years, by this method, in the treatment of fractures of the thigh and leg, warrants me in asserting most positively that adhesive plaster as a means of making and keeping up *counter-extension*, is no less valuable, than it is here regarded by Dr. Hamilton, as a means of making and keeping up *extension*. The same difficulties arising from pressure and friction, viz., "excoriation, ulceration, and even sloughing" of the perineum and groin, are encountered when any of the unattached means are used, and are as certainly prevented when adhesive plaster is applied instead. The cases already alluded to, published in the January numbers of the *American Journal of the Medical Sciences* for 1851 and 1858, are a part of this experience, and fully sustain all that is here assumed.

In these cases it is shown that the most seriously complicated, compound, and comminuted single and double fractures in adults, children, and infants were treated without pain at the seat of counter-extension; and that the cures were perfected in unusually short periods of time. In some of the cases, adhesive plaster bands were used instead of the common roller and many-tailed bandage, with great advantage. Although the anterior and posterior counter-extending bands are usually quite sufficient, yet any additional amount of adhesive plaster may be applied in order to diffuse the tractive force still more widely. The pelvis may be fixed by girding it with a broad horizontal band, from which any desirable amount of counter-extending power may be commanded, by strips extending from it anteriorly and posteriorly to the upper extremity of the splint. Thus the perineum, if any injury there should render it necessary, may be left free and unencumbered. Since the publication of these cases the plan has been adopted by several private practitioners, who have informed me that their success has been equally gratifying. Amongst these, Dr. Kerr, of York, Pa., recently treated a case of severely comminuted compound fracture of the thigh with the happiest results. It has also been used at the Episcopal Hospital of this city in several cases, by Dr. Kenderdine, one of the surgeons of that institution, who informed me that the results have been so favourable that hereafter the plan will be preferred by him to every other. Dr. Hunt, one of the surgeons of the same institution, informs me that he has a case now under treatment, in which adhesive plaster is being used for counter-extension.[1]

[1] *March* 1*st*. Dr. Hunt says: "This case has now been under treatment for six weeks. It has progressed favourably, in every respect. The patient has experienced no pain from pressure, abrasion or excoriation, at the seats of extension

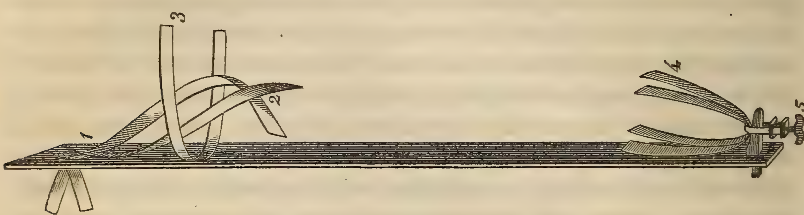
As additional confirmation of the value of the practice the following cases, also of unusual character, are submitted:—

1. I was sent for, January 21, 1858, to take charge of Mrs. J. Van Gunton, aged sixty-six years, residing at No. 1206 Alder Street above Girard Avenue, who, it was stated, had fallen from the flat roof of a one story back building, and fractured her left thigh bone. When I arrived at the residence of the patient, I was informed by her son, Mr. F. Van Gunton, that the accident had occurred *seven weeks previously*, and that an "eclectic doctor" had had charge of the case and treated her from the time of the receipt of the injury to the present time. As retentive means the "doctor" had used two long narrow splints, the outer one extending from the trochanter, and the inner from the perineum, to the foot. These were bound to the limb, after supposed reduction of the fracture, by a roller. No other effort was made to keep up extension and counter-extension except with this lateral pressure. On the day when I was sent for he had removed this retentive apparatus, and informed the patient and her friends that union had taken place. It soon became evident, however, not only that union had not resulted, but that *the thigh was fractured in two places.* For these mistakes in diagnosis and treatment he was discharged. On examination, I found an oblique fracture at the union of the upper with the middle third of the femur, and another in the lower third, immediately above the condyles. From the increased breadth of the bone at the condyles, and the existing inflammation in the knee-joint, I had no doubt of the existence of vertical fracture between the condyles; but this could not then be certainly detected. The whole limb was swollen and shortened. After making as much traction with the hands, assisted by her son, as the patient could then endure, the leg was still three and a half inches shorter than the sound one. I learned that the patient, previous to the accident, had enjoyed uninterrupted good health for years; in consequence, however, of the protracted confinement, and her sufferings from inflammation and fever, she had become greatly reduced. I applied the retentive apparatus described in the publication of my first case, using adhesive plaster for extension and counter-extension. In this case, however, which required an increased amount of power to overcome the permanent shortening as well as the active contractions of the muscles, I used double adhesive bands, which were fused together by passing them over a heated surface. I also added a horizontal strip which encircled more than half of the pelvis, immediately below the crista ilii, for the purpose of more securely binding the

and counter-extension, at any time. The adhesive strips used for counter-extension have been renewed but once since their first application. This became necessary, in consequence of the want of firmness of the plaster cloth; it would, therefore, be better if this consisted of linen, or some other unyielding material." The fusion of two layers of plaster, as used in the case of Mrs. Van Gunton and others, remedies this defect almost entirely.—D. G.

counter-extending bands to the surface, and increasing the extent of attachment of the counter-extending means. That the manner of applying the adhesive bands may be fully understood, I here furnish a drawing of the apparatus. This conveys a more accurate idea of the plan than mere description. The wadding, the roller for the leg, and the many-tailed bandage for the thigh, are omitted.

Fig. 3.



1. Anterior and posterior counter-extending adhesive bands, two and a half inches wide, crossing each other before they pass through the mortise holes. 2. The same, crossing at the upper part of thigh and perineum. 3. Horizontal pelvic band, which may be three inches wide. 4. Extending bands, receiving strap of tourniquet in the hollow of the foot. 5. Tourniquet.

Having adjusted the apparatus, extension and counter-extension were made by the *tourniquet*, and were increased from day to day until the fractured limb was brought very nearly to the length of the sound one. The tension necessary to overcome the shortening and muscular resistance was, from the peculiar nature of this case, unusually great, *yet no pain was complained of at the seats of extension or counter-extension.* One of the peculiar advantages of the use of adhesive plaster for these purposes is that the extent of its attachment may be increased, if necessary, until the whole pelvis is encased. My experience, however, has satisfied me that the amount of surface covered by the bands, as represented in this drawing, is sufficient to keep the muscular contractions under easy and full control until union takes place in the most unpromising cases.

The patient was placed upon a fracture-bed (I do not remember who first contrived it), which was constructed a few hours afterwards in the neighbourhood. I have found this bed a most valuable assistance in the treatment of all severe injuries, and after large operations, for the last twenty years of my practice, and therefore confidently recommend it to all who have similar cases to treat. It consists of a frame three and a half feet wide and six feet long, made of $1\frac{1}{4}$ or $1\frac{1}{2}$ inch plank, 4 inches wide, joined by mortise flatwise. Over this, sacking or strong canvas is tightly drawn, and secured by tacks. A hole is made in the centre, of a convenient size for the passage of the alvine evacuations. A sheet is thrown over the bed, with an opening to correspond; pillows are placed upon its upper end, and the bed is fully furnished. This is preferable to any of the complicated and expensive beds in use, because it possesses all the properties required in a

fracture-bed, and yet is so cheap and simple as to place it within the reach of every one, in any locality. It imparts the evenness and firmness of a mattress to the softest bed of down or feathers; the patient can have his evacuations, without the least disturbance of the fracture, by raising the frame and resting it upon stools or chairs; during this time the bed upon which it was placed may be changed and made up, and, if it is desired, the patient may be carried safely from room to room, and, in pleasant weather, out into the open air of a piazza or back yard, upon this fracture-bed.

When the apparatus was applied, and the patient placed upon her bed, she declared herself to be much more comfortable than she had been since the occurrence of the fracture. The tension of the extending and counter-extending bands did not occasion any suffering at the seats of their application, whilst the irritated tissues at the points of fracture, though still painful, were relieved in consequence of the more favourable position of the fractured ends of the bone, as regarded the soft parts. Her constitutional symptoms were those of irritative fever, caused and kept up by local suffering. Her tongue being coated, and her bowels not having been moved for several days, a pill, composed of muriate of morphia gr. $\frac{1}{4}$, and calomel grs. v, was ordered to be given at bedtime, and to be followed in the morning by a tablespoonful of castor oil. To the knee, which was inflamed and greatly swollen, fifty leeches were ordered to be applied; after which warm fomentations, to encourage the bleeding, were to be used.

January 22. The patient passed a comfortable night. The knee is less swollen and painful, in consequence of the leeching and fomentations, and the calomel and oil had operated twice freely. She relished some gruel for breakfast. The extension and counter-extension were increased by means of the tourniquet, without pain at the seats of their application, and but slightly at the knee and seats of fracture. Fever having abated very considerably, ordered: ℞.—Quiniæ sulph. ℈j; tinct. ferri muriat. fʒij; syrup, simpl., f℥ij. S.—A teaspoonful every two hours during the day, and tinct. opii acet. gtt. xxx at bedtime. Locally, flaxseed-meal poultice to the knee.

From this time forward the case progressed favourably. After a few days the fever subsided entirely, and, under the use of tonics and an improved diet thereafter, her strength gradually improved. Sleep was provided for during the night by the exhibition of the black drop, and oil was given occasionally to remove costiveness. The leeching was repeated once, in consequence of an aggravation of the swelling, from cold contracted one night, when a great and sudden change of the weather had occurred. After the poultices were discontinued, tincture of iodine was applied around the joint, to reduce the remaining tumefaction. At the end of seven weeks the anterior counter-extending strips became loose, and pain and slight abrasion resulted in one night from their pressure and friction, as if loose bandages had been used. These were removed and new ones applied, and all local irritation immediately subsided. When eleven weeks had elapsed, the splints

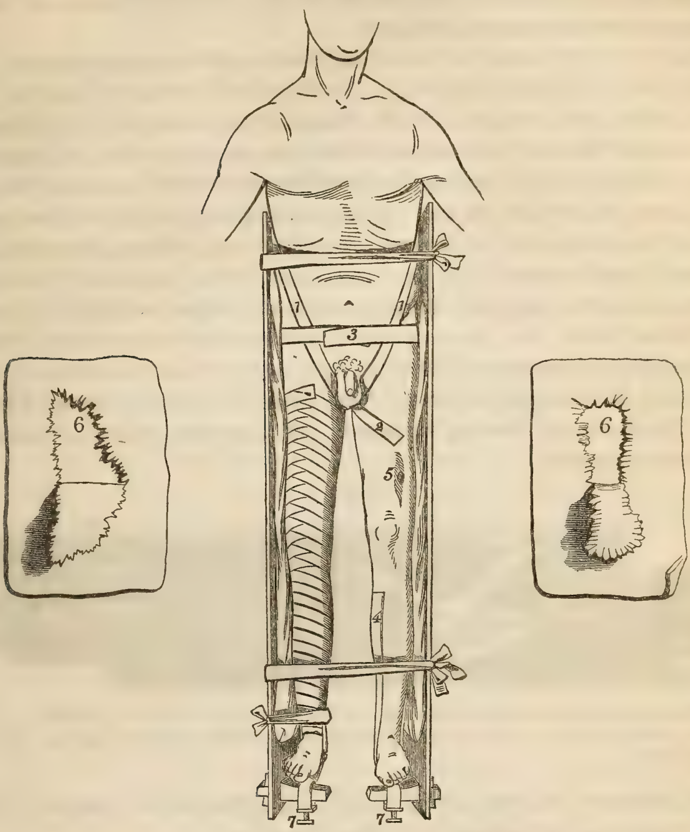
and bandages were all removed for the first time since their application, and the fractures seemed to be united. Enlargement of the knee, without pain, however, was still present. On the following day it was ascertained that the union, especially at the upper seat of fracture, was not sufficiently firm, there being outward deformity and shortening, produced by muscular contraction. The retentive apparatus was again applied, after bringing the femur as nearly as possible into its normal line, and allowed to remain four weeks longer. After this removal, being seventeen weeks from the application of this apparatus, and twenty-four weeks after the occurrence of the fracture, the union was considered sufficiently consolidated, and the apparatus was not again applied. By very careful measurement, at this time, the fractured limb was found to be scarcely one inch shorter than its fellow. In consequence of her protracted confinement, at such an advanced age, she was not able to leave her bed, however, until a week afterwards. Gradually she gained strength, and, by the aid of crutches, became able, at the end of the second week, to move about the room. The union, however, could not have been as perfectly consolidated as was supposed when the final removal of the apparatus took place, since, by a recent measurement, made January 14, the shortening has increased to 1½ inch. She is, however, fully restored in every other respect, and is able to attend to her ordinary household duties. Notwithstanding the unusual amount of power which it became necessary to apply by the tourniquet, for so protracted a period, no complaint was made of pain at the seats of the application of this power, except when the anterior adhesive counter-extending bands became detached from the surface, and began to act as ordinary loose bands, this being promptly remedied by the application of new strips.

2. The next case is that of Thomas Stokely, aged 11½ years, whom I attended, as consulting surgeon, at the request of Dr. Theophilus E. Beesley, the family physician. In this there was compound oblique fracture of both thighs, the bones piercing the pantaloons as well as the muscles and integument. Whilst I leave it to Dr. Beesley to present a full account of this case to the College, I furnish the accompanying drawing (Fig. 4, p. 421), which was very accurately taken from life by Kielman. The roller applied to the leg, and many-tailed bandages to the thigh, are shown as applied to the right limb only.

In the treatment of oblique fractures of the bones of the leg, requiring permanent extension and counter-extension, the adhesive plaster bands are quite as valuable as in fracture of the thigh. For simple oblique fracture, two splints, six inches wide, reaching from above the knee to about six inches below the foot, are sufficient (Hutchinson's modified). Each splint should have two holes at its upper and one mortise hole at its lower extremity. Those at the upper extremity are provided for the passage of the counter-extending adhesive bands, and the mortise below is for the reception of a cross-piece upon which the frame of the tourniquet rests. The tour-

niquet in this, as in fracture of the thigh, furnishes the most convenient and efficient extending and counter-extending power. Junk bags, or wadding, to fill up the inequalities of, and give support to, the leg, adhesive plaster

Fig. 4.



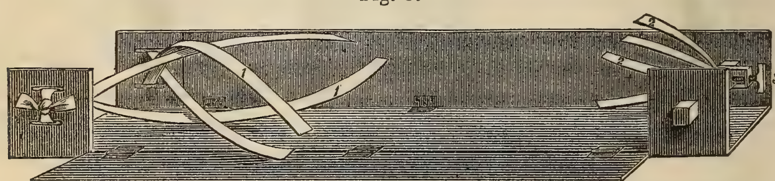
1, 1. Anterior adhesive counter-extending strips. 2. Distal extremity of posterior adhesive strip of left side. 3. Adhesive strip surrounding pelvis, binding the anterior and posterior strips to pelvis. 4. Inner extremity of the extending adhesive strip, forming stirrup under the foot to receive the strap of the tourniquet. 5. Cicatrix of left thigh. 6, 6. Holes made in the pantaloons by the protruding fragments of bone, full size. 7, 7. The common tourniquet, by which the power was applied.

bands, about two inches wide, and the roller, or the many-tailed bandage, complete the apparatus. For counter-extension four adhesive plaster bands are necessary, and for extension two are ordinarily sufficient. The former are applied spirally, so as to cross each other, the two anterior just below the tubercle of the tibia, and the two posterior at a point directly opposite. These, in their spiral course upwards, severally cross each other, externally and internally, opposite the joint; their proximate extremities are then passed through the holes in the splint, and securely tied outside. The

latter are applied to the foot and ankle so as to cross each other at the hollow of the foot, then over the tarsus anteriorly, and the upper part of the tendo-Achilles posteriorly. The strap of the tourniquet passes between the sole of the foot and the strips where they cross each other. After the extending and counter-extending adhesive bands are applied, they are bound to the surface by common bandages.

In cases of compound fracture a modification of the common fracture-box may be used very advantageously. In this the foot-board is omitted, and a cross-bar for the reception of the frame of the tourniquet is substituted. The sides of the box each consist of three separate segments. Of these the upper and lower are permanently screwed to the bottom-board, and the central one is attached by hinges. By this arrangement there is full access to the wound, which may be dressed from day to day without disturbing the extension and counter-extension maintained by the permanently attached upper and lower segments. This apparatus was used successfully in the case of Michael Gillis, who had compound comminuted fracture of both bones of the leg, in the winter of 1852–53, and is the sixth of the series of cases published in the *Amer. Journal of the Medical Sciences*, already alluded to. The following drawing represents this apparatus, omitting bandages and side-compresses.

Fig. 5.



1. The four counter-extending adhesive strips, as if encircling the knee and upper part of leg. 2. The two extending adhesive strips crossing at the bottom of the foot, ready to be applied to the foot. 3. Tourniquet.

Dr. BEESLEY then read the following note of the case of compound fracture of both thighs, referred to by Dr. Gilbert:—

In the afternoon of the 27th of 10th mo. (October), 1858, I was called, as the family physician, to Thomas, son of Wm. S. Stokely, No. 58 N. Eighth St., and met Dr. David Gilbert at the bedside of the sufferer. He was a lad of 11½ years of age, who had a compound fracture of both thighs, of the right a little below, and of the left a little above the middle of the femur, occasioned by his falling from a height of about 20 feet directly upon his feet. Subsequent examination of his dress showed that the upper fragment of one thigh, and the lower of the other, had protruded sufficiently to penetrate through his pantaloons in front. My friend Dr. Griscom had first seen the patient, and attended to the reduction of the bones. I assisted Dr. Gilbert in the application of such temporary splints as we had, and of the proper dressings. As a counter-extending band to each thigh,

two strips of adhesive plaster, each an inch and a half wide and two feet long, were so placed as to cross each other at the portion of the thigh where it joins the perineum, extending, the one in front and the other behind, to the upper part of the splint, and were there passed through two holes and were firmly fastened together, an outside splint only being applied to each limb. A broad adhesive strip was applied around the pelvis and over the counter-extending bands, so as to give them additional support. For the extending bands two broad strips of plaster were applied, from a little below the knees, along the legs, sufficiently long to leave a loop below the hollow of each foot for the strap of a tourniquet to pass through. Extension and counter-extension being then made, and the fractures properly adjusted, a common roller-bandage was bound on the adhesive strips from the ankles to the knees, and the many-tailed bandage was applied over the thighs. Between the splints and the thighs cotton wadding, folded in muslin, was used as padding. A compress and cold water dressing were kept to the wounds.

No complaint of pain at the seats of extension and counter-extension was made by our patient. The following day the temporary splints were laid aside, and a narrow board splint for each limb was substituted, extending along the outside from below the arm-pit opposite the nipple to about six inches below the foot. Extension was made and kept up by means of common tourniquets, the frames of which rested on blocks projecting inwardly from the lower extremities of the splints.

The wounds were inspected at the end of the third day, and were found sealed up by coagulated lymph, a slight oozing of bloody serum alone appearing. Dry compresses were now applied, and retained by the bandage of strips, and a flat bottle of ice-water was laid between the thighs, opposite the injured parts, and continued for a fortnight or more, to the apparent comfort of the patient.

After the first day there had been a considerable amount of swelling, which seemed more the result of effusion and congestion than inflammation. The general excitement was at no time great. It was highest about the fourth day, and subsided to a very moderate degree about the close of the first week. Anodynes were given from the commencement, about every four hours, to allay the spasms of pain which occasionally darted through the limbs, and to obtain sleep; they were found useful, in moderate doses, throughout the case. The neutral mixture, with a little tincture of rad. aconite, was administered, whilst there was febrile excitement; also occasionally, when needful, a cooling laxative or an enema. On the complete subsidence of the febrile symptoms, tonics were given, principally sulphate of quinia, with the tincture of the chloride of iron. The patient was placed from the commencement on a fracture-bed, consisting of a simple frame, about 4 inches deep, 5 feet long, and $2\frac{1}{2}$ feet wide, with strong ticking stretched tightly over it and firmly nailed to it; the ticking was furnished with a hole in the centre for the evacuations, and under this hole was placed

a cushion or pillow. On this fracture-bed he could be lifted, without pain, to attend to his evacuations. The fracture-bed rested on a common mattress, and was supported, when desirable, by stools at the head and foot.

The first complaint he made of pain at the seat of the counter-extending bands was on the morning of the fourteenth day. On examination, it was found that, in order to relieve itching under the plaster, he had separated it for some distance from the skin on the previous evening, and that then this detached portion had acted as an ordinary loose counter-extending band in producing pressure and excoriation; to relieve this, his mother had stuffed cotton under the bands, but still the pain continued. We renewed the loosened portions of the bands, and little complaint was made during the remainder of the treatment until the fourth week, when it became necessary to apply fresh anterior and posterior counter-extending bands. The wound of the left thigh was fully cicatrized on the eighteenth day after the accident; that of the right not until the sixth week. After the first few days the discharge from each wound was very trifling in amount, proceeding only from the granulating surfaces.

We had reason to believe that firm union had taken place at the end of the fifth week; but, at the urgent request of the father of the patient, the splints were continued until the middle of the seventh week, or the forty-sixth day from the accident, when all retentive apparatus was removed. The thighs have their natural form and length. The patient was free, during the entire period of treatment, from the usual suffering experienced at the seats of extension and counter-extension; the only exception to this was the very slight uneasiness above mentioned, induced by the patient's interference with the bands at and near the perineum. The strips of adhesive plaster, by which extension was made, remained without removal from the day they were applied to the end of the treatment.

In writing an account of this interesting case, I have drawn freely from the notes of it kept by Dr. Gilbert, to whose skill, under Providence, I attribute its completely successful result, the more remarkable from the age, nervous constitution, and active character of the lad. Of all the cases of fracture of the thigh which have come under my notice in the course of a pretty extensive practice of more than forty years, there has been none where the apparatus made use of was so simple, so painless, and yet so thoroughly efficient in retaining the injured limbs in their natural position. The fracture-bed, by its cheapness, lightness, and convenience for moving the patient without pain and allowing the necessary evacuations of the bowels, seemed almost all that was to be desired in such a case. At first there was some difficulty in urinating, and the catheter was used a few times; after that, however, a large-mouthed flattish phial was placed so as to receive the urine, when the patient desired to void it. In conclusion I may add, that now, at the end of three months from the accident, the boy walks with facility and without limping, there only appearing some stiffness in his knees.

REVIEWS.

Art. XII.—*The Transactions of the American Medical Association.* Instituted 1847. Vol. XI. Philadelphia, 1858. 8vo., pp. 1027.

The work before us belongs to that class of publications the contents of which demand most imperiously the exercise of a searching, discriminating, and fearless criticism. The very fact of the several papers embraced in these *Transactions* being published under the sanction of an institution which we have claimed as our National Medical Congress, representing, at once, the mind and will of the entire profession of these United States, gives to them an importance and influence far beyond what even the best of them would have commanded had they made their appearance under less lofty auspices.

It is in vain that the Association annually disclaims all responsibility for whatever may be contained in the reports and communications embraced in their printed *Transactions*. So far as relates to the accuracy of the facts, the correctness of the observations, and the legitimacy of the deductions set forth in such reports and communications, such disclaimer may with great propriety and truthfulness be made. Nevertheless, while the Association is unquestionably entitled to the credit that is due for whatever actual reforms it may have effected, and for whatever valuable contributions to the general fund of medical information it has been the means of eliciting and widely circulating, it is, also, in some degree at least, amenable to censure, when it gives currency to any report or essay which, in matter or manner, is calculated to bring into discredit the professional or literary character of the great body of American physicians. Do what we will, say what we will, the *Transactions of the American Medical Association* will not the less be viewed abroad as the exponent of the actual state of medical knowledge and education in the United States. It is fair to presume that, in the nomination of the committees charged to report on special subjects, the Association always makes choice of such of its members as are presumed to be best acquainted with the subjects referred to them, or to be possessed, at least, of the talents and skill requisite for their faithful and satisfactory investigation,—that its prizes are always bestowed upon such essays as are, in its estimation, worthy of them—and that, in accepting and referring for publication any voluntary communication, it believes it to contain either new facts and observations or original and valuable deductions from facts and observations already known. Among the professed objects of the Association a leading one is the elevation of the standard of medical education and the cultivation and diffusion of medical knowledge. Now, we apprehend that this object is not to be attained by allowing every paper which is read or offered to be read at its meetings, whether by a committee of its own appointment or presented by one of its members as a voluntary contribution to be inserted in its printed *Transactions* as a matter of course, without any regard to its matter or its manner.

We freely admit that, so far, the Association has kept prominently in

view the leading objects contemplated in its organization, and that it has already made some progress towards their achievement. We are fully aware that it has done much good, more, perhaps, than could have been expected from the lax character of its organization and the many difficulties which presented themselves as impediments to its successful working. Still, what it has done is but as nothing when compared with the important labour it has still to accomplish before its mission can be said to be fulfilled.

In the eleven volumes of *Transactions* published by the Association there is contained a large amount of valuable matter, comprising reports, essays, and communications of unquestionable merit and deep interest, which confer much credit upon their respective authors, as well as upon the Association, upon whose movement they have been elicited. There may, it is true, be pointed out, amid the many contributions of sterling worth which form the contents of these *Transactions*, a few which, in respect to matter, are of very doubtful character, or particularly exceptionable in respect to manner—papers which ought not to have received the *imprimatur* of the Association, and which would not have gained a place in its *Transactions* had they been submitted, before they were referred for publication, to a proper committee in order that their true value should be carefully tested. It was the earnest desire of the late Dr. Daniel Drake that this should be the course pursued in regard to every paper read or presented at the sessions of the Association, and he actually withheld an important communication of his own because the Association refused to allow it to take this course. We feel persuaded that, unless the plan suggested by Dr. Drake be speedily adopted, the volume of *Transactions* annually issued by the Association, while it continually and rapidly increases in bulk, will as certainly and rapidly deteriorate in the value of its contents.

We believe that the error of the Association is not that it has published too little, but rather that it has published too much. A volume one-half the size of that before us, containing only a few well-digested and carefully prepared original reports and essays, would do more for the promotion of medical knowledge, and for the establishment of a national medical literature, than can be accomplished by the ponderous tomes yearly issued by the Association, made up in their present very loose manner.

The address delivered by Dr. Paul F. Eve, the retiring president, at the formal opening of the session of 1858, is one of a very high order. Its theme is the objects of the Association, the extent to which these have been attained, and the probability of their final accomplishment through its agency. These subjects are rapidly, but very ably discussed. The address shows clearly that thus far the Association has been no failure, and that, provided the delegates who shall hereafter compose it, shall equal in ability and zeal those who have preceded them, little doubt need be entertained of the permanently beneficial influence it is destined to exercise over the best interests of the medical profession throughout the United States.

However far, it must, in truth, be admitted to have fallen short of the accomplishment of the great work for which it was brought into being, it is nevertheless very certain that, in the moral influence it has exercised, the emulation and concert of action it has encouraged, and the friendly intercourse it has fostered among physicians, and the amount of talent it has succeeded in enlisting for the examination of important questions in nearly every department of medical science, it has done a good work, for the achievement of which no other association that could be organized would perhaps have been equally competent.

In carrying out the important measures it was designed to effect, embracing as they do the promotion of medical science, the perfection of medical education, and the enforcement of a correct code of medical ethics, no one supposes that the Association could have accomplished everything demanded for the good of the profession, or that all the great designs of its founders were attainable in the brief space of eleven years.

The first of the reports in the present volume are those on Medical Topography and Epidemics. Of these reports, but three were received at the session of 1858, viz., from the States of Kentucky, New Jersey, and Ohio.

That from Kentucky is drawn up by Dr. W. L. Sutton. It is one of great interest, especially that part which treats of the early settlement of the State, and the perils and privations of the white men who first made choice of it for their abode; though all this has but little direct connection with any of the questions relating to the medical topography of the State and the hygienic condition of its population. The next portion of the report, which describes the character, occupations, habits, and diet of the people, the climate, with a short notice of the mineral springs, is more *germain* to the leading objects to be had in view in the preparation of a report such as that which was expected of Dr. Sutton.

Two diseases only are noticed in this report, namely, the jerks, or, as it has been denominated, epidemic epilepsy, and the so-called milk-sickness.

The "jerks," or "bodily exercises," as they were popularly termed, consisted in various spasmodic seizures and contortions of the muscles of the trunk and limbs observed in persons, who had been brought into a high state of excitement in the large assemblages that were drawn together during a period of religious enthusiasm, kept up by the exhortations and fearful appeals of popular preachers, more noted for zeal than wisdom. This religious excitement continued, with more or less intensity, from 1799 to 1805, and gave rise to convulsive affections which may be said to have constituted a true epidemic, "remarkable alike for the extent of country overrun, the number of persons affected, and the strangeness of the phenomena exhibited;" resembling in nearly all its features, making allowance for modifications necessarily produced by differences in the habits, opinions, position in society, and education of the patients, certain of the strange convulsive epidemics which prevailed in Europe during the middle ages. We fully concur with Dr. S. "that the history of these things is equally interesting," and we would say instructive, also, "to the physician and psychologist."

The account of "milk-sickness" was furnished by Dr. James H. Barbour, of Falmouth, Ky.; some additional remarks being added by Dr. Sutton, derived from reading and oral communications.

The disease known by the popular appellation of milk-sickness has prevailed, at different periods, as an epidemic, over a considerable portion of the Western States. It commences nearly in the same manner as an ordinary remittent. Its leading symptoms are muscular soreness, headache, with slight rigors or distinct chills, succeeded by reaction, loss of appetite, pasty tongue, oppression at the stomach, soon followed by burning, nausea, and vomiting, with intense thirst, the fluid vomited being large in quantity and of a green colour; constipated bowels; pulse small and generally diminished in frequency; hurried respiration, with frequent sighing; great restlessness, early drowsiness, merging into deep coma, with dilated pupils; face of a red and bloated or stupid, dusky appearance; nose and ears, as well as general surface, cool; extremities often cold, at the same time, the patient complaining of an acrid or burning sense of heat.

By many it is believed that this disease is produced in the human subject by partaking of the flesh and milk of cattle affected with the staggers or trembles; but Dr. S. believes that such is not the case, and that what has been denominated milk-sickness is one of a strictly malarial origin. His reasons for such belief are, 1st. In every location where the disease prevails, multitudes escape who partake of meat and of milk. 2d. It has occasionally attacked such as have been very careful to abstain from these articles. 3d. It is especially rare in young children, although, in Kentucky, milk forms a large portion of their diet. 4th. It prevails during the same season of the year as bilious fevers. 5th. It occurs in the same regions in which malarious fevers prevail, and contemporaneously with these in the same family; and, further, in the same individual an attack may commence as sick stomach and end as bilious fever. 6th. It occasionally happens that persistent vomiting and obstinate constipation, the prominent symptoms of the so-called milk-sickness, are the most constant and distressing symptoms of autumnal fevers. 7th. It has, in a few instances, like malarial fever, recurred for two or three years at the season in which it began without any known reapplication of the supposed remote cause. 8th. The most successful treatment is that which has been found best adapted to the cure of remittent fever.

To the report under consideration are appended a series of interesting tables showing, 1st. The population of each county in Kentucky; the number of acres it embraces; the proportion of its improved and unimproved territory; the amount of land to each inhabitant; the average number of deaths annually from cholera, dysentery, fevers, scarlatina, and zymotics, and the proportion of such deaths, in each disease, to the population of the respective counties in which they occur. 2d. The general average mortality annually of the limestone regions of the State, and that from each of the diseases above enumerated, and the proportion of such deaths to the entire mortality from known causes. 3d. The same series of facts in reference to the coal regions; and 4th. The colour, sex, and ages of those who died from cholera, croup, dysentery, continued fevers, fever generally, scarlatina, and hooping-cough, respectively.

The report on the Topography and Epidemic Diseases of New Jersey, is by Dr. Lyndon A. Smith. It presents a very general outline of the medical topography of the State, and of the most prevalent diseases within its limits from 1855 to 1857 inclusive. The most interesting portion of the report are the conclusions of its author in relation to the protective powers of vaccination, based on observations collected during an experience of thirty-seven years as a practitioner. With the first, second, and third, of Dr. Smith's conclusions, we entirely coincide. These aver the permanency throughout life, in every instance, of *perfect* vaccination—but that to effect this perfect vaccination in some subjects requires repeated insertions of matter in the arm, even though each may be attended with all the indications of a genuine vaccination—a resort, in short, to revaccination, again and again, until no longer any impression is made by the virus, and, finally, that even in those who have been exposed to the contagion of smallpox, a prompt resort to vaccination will often greatly mitigate the variolous attack and save the life of the patient. The correctness of the sixth and seventh of his conclusions is also borne out by our experience, they are—

"That the virus should never be taken from any but a healthy infant of healthy parents, and then there is little or no danger of propagating any other disease, as many think may be and often is done.

"That eruptions often occur after the most careful vaccination, and are owing to some peculiar idiosyncrasy, although the friends of the parents attribute it to the virus."

The fourth conclusion which Dr. S. has derived from his experience, that, namely, "a *very small* pustule, which goes through the regular stages and produces a constitutional effect about the ninth day, is more generally perfect than one that produces great inflammation, pain, and swelling, and affects the glands," conveys, certainly, an important truth, but it is expressed in language that will be apt to mislead the inexperienced practitioner. We cannot admit that the slighter the impression produced by the virus at the place of insertion after the proper period of incubation, and the less the impression it produces upon the system, the more genuine and perfect the vaccination. Experience has taught us that those of the European practitioners are perfectly right, who insist that in vaccination, the virus must affect the organism with a certain amount of intensity, or it will fail in affording to it a sufficient, much less permanent protection from variolous infection; and that when, in order to produce the necessary vaccine impression, they insert the virus at different parts of the same arm, or in both arms, they are justified in the propriety of the practice. The truth of the theory upon which that practice is based would seem to be established, by the fact that the subjects of their vaccinations have been shown to be much less liable to smallpox in its genuine or modified form, than those protected by a single insertion of matter.

The report on the Epidemics of Ohio, by Dr. George Mendenhall, is confined pretty much to the diseases prevalent during the year 1855. In reference to these it presents but a few general remarks. With the exception of a very general prevalence of intermittent fever, and diseases of a kindred character, during July, August, September, and October of that year, we are assured that the three years immediately preceding 1858, were unusually healthy. The epidemic choleraic disposition which showed itself in the latter part of the year 1848, and continued, with varying degrees of intensity until 1854, had completely subsided. The ordinary diseases incident to the country no longer exhibited this peculiar influence, and with the exception of the intermittent and remittent diseases of 1855, were diminished in degree.

As just remarked, during several months of the year just referred to, periodical fever prevailed to a very great extent, throughout nearly every portion of the State. Few localities were exempt—high and low grounds were alike visited by the disease. "In places where it had usually prevailed very few escaped; while in districts where it had been an entire stranger, numerous cases occurred. In short, the influences producing this class of affections seemed to be uniformly present in every locality." These influences Dr. M. considers to have been, "the character of the season in reference to moisture, warmth, and a profusion of immature vegetable growth." He is not aware that the periodical fevers of 1855, presented any unusual characteristics or complications—the tendency to diseases of an intermittent character undoubtedly predominated, the influence of which was sensibly felt in the modification of other diseases.

The next report is that of the Committee on Medical Literature, prepared by Dr. A. B. Palmer, of Chicago, Illinois. An annual report embracing a candid, unbiased, and, withal, critical examination of the actual condition of the Medical Literature of the United States; one that would exhibit a fair exposition of its leading faults and excellencies, the improvements it

has undergone, and a judicious exhibit of the amount of influence it has exerted in the advancement of medical knowledge in our midst, would no doubt be a valuable document, and if executed with ability, have no little tendency to elevate the character, improve the style, and increase the value and influence of the professional literature of our country. Let us not, year after year, content ourselves with drawing up disparaging pictures of the matter and manner of the contributions made by American physicians to the general fund of medical literature, and in uttering doleful complaints of the smallness of these contributions and their many imperfections. Let us rather examine what we have done, not by comparing this with the contributions, for which we are indebted to the labours of the learned and experienced professional writers of Europe, with ample opportunities at their command for making extended series of comparative observations in every department of medicine and its collateral sciences; but by comparing what American physicians have done in the department of medical literature with the peculiar circumstances in which they were placed, and the demands constantly made upon their time and energies in a practice, so far as relates to all of them located beyond our larger cities, amid a population sparsely distributed over a great extent of country, leaving little time, and less inclination, for that close application demanded for the careful collation of their observations, and the deduction from them of those general facts and principles adapted to the instruction of others. With such an examination, it will be found that the physicians of this country have, in times past, contributed as much, and that in as unexceptionable a form, as could reasonably have been expected of them, towards the literature of our profession, and that annually their contributions in this department, are becoming more copious and valuable. The pages of our medical periodicals bear ample testimony to this fact; nor are we deficient in independent works, systematic as well as monographic, to refer to in proof of the rapid progress and improvement medical literature is making in the United States.

Much has been said of the necessity of some measures being taken to promote in our midst, a national medical literature, though as to what those measures should be, there seems to be but little unanimity of opinion. All the schemes that have been formally suggested have appeared to be either merely temporary and doubtful expedients, or of a more or less impracticable character. We have no idea that an American medical literature is to be forced or coaxed into existence. With the improvement already commenced in medical education, throughout our country, and the certainty that this improvement will go on increasing from year to year, there will necessarily take place a corresponding increase of medical readers and medical authors, or at least of those competent to record and arrange their observations, and embody in correct language the principles they have evolved from the facts they have accurately studied and carefully compared.

Even now, no work in any of the branches of medicine, issued by an American writer, fails to receive an ample support from American physicians, provided it is one adapted to their wants. The great body of the profession with us, are still, in a great measure, mere "working physicians," —laborious practitioners, and such publications only will meet their wants as have a direct practical tendency. The mass of the profession is not yet prepared by education, or by habits of close, protracted, and laborious investigation to appreciate works appertaining to the higher branches of medical philosophy. It has not, as yet, been taught the applicability of purely philosophical deductions to the improvement of medical knowledge and skill.

It must be some time, therefore, before American medical literature will be able to boast of many meritorious works of this latter class.

The report of Dr. Palmer treats the subject of medical literature with a good degree of justness; his conclusions are, in general, correct, and his suggestions for the most part wise and practicable. In bringing his labours to a close, he presents the following as a very brief *résumé* of some of the leading positions assumed in the report.

"The periodical literature of the United States is regarded as possessing great abundance, variety, richness, and general excellence, and, though still possessing defects, is constantly improving. Many of the contributions are of great weight and value, indicating an enterprising and industrious profession. Serious defects are regarded as existing in the review department, arising mainly from the fact that the income of the journals will not justify pecuniary disbursements for literary labour; and editors necessarily engaged in other pursuits cannot command the time, if all possessed the ability to do the work thoroughly and well. A few well-supported journals in place of the many but illy sustained might tend to correct this evil; but the multiplicity of local journals is considered as peculiarly beneficial, by collecting from a greater variety of sources a larger number of facts and developing the powers of a larger number of writers. The interests of this part of our literature demand a more prompt and liberal pecuniary support.

"The number of original American medical works is increasing and their character is improving, and, in some respects, particularly in practical utility, they will not suffer in comparison with those of Europe; yet serious imperfections exist and decided improvements are demanded. Great and permanent improvements in medical, as in general literature, must be gradual, depending more upon the advancement of education, of taste, and intelligence, than upon any specific measures which may be adopted. Still, various particular measures, such as the frequent writing of medical theses during pupilage, and keeping systematic records of cases when in practice, would do very much in hastening on improvement. But, for the greatest perfection of our literature, we must wait the fuller development of our country, and for those changes of time and circumstances which shall produce a larger number of devoted savans and scholars, placing them in situations where a variety of absorbing pursuits shall not prevent the concentration of great talents upon a comparatively limited range of subjects.

"Respecting the reprint of foreign works, it is held that, while the free circulation of the best class of those works among us increases the knowledge and improves the taste of the masses of the profession, it does not interfere with the production of the higher order of original works, and that the moral obligation of our government to join with Great Britain in the enactment of an international copyright law, is by no means clearly established."

The report of the Committee on Medical Literature is followed by the report of a special committee, charged with the duty of devising a system of medical education for the consideration of the Association.

Among the prominent, and, certainly, most important objects of the American Medical Association, is to be ranked the elevation of the standard of medical education in this country, and the establishment of a proper and uniform criterion, by which to determine the qualifications of such as offer themselves for admission into the ranks of the profession. Unfortunately, however, in the attainment of these objects the Association has accomplished less than in respect to any of the others had in view in its organization. It may, perhaps, be affirmed, and certainly not without some foundation in truth, that the Association has not, however, failed altogether to exert an influence, even in respect to medical education, as is shown by the general evidence of a better preparation and higher attainments in those who now enter the profession, than was formerly the case; still it must be

admitted, that the Association has not succeeded in establishing, in any portion of the country, a course of medical instruction, which makes even an approach towards what it has itself set up as the correct standard. The subject is certainly one surrounded with many and serious difficulties, to overcome which, will demand much wisdom and patient waiting. A mistake, we think, has been made by the Association, in acknowledging, from the very outset, that a diploma conferred by any legally constituted medical college, no matter how deficient its curriculum of instruction, or loose its examination of candidates for the doctorate, is of itself a warrant for the admission of its holder into our ranks, and in applying, consequently, to the schools for that reform in the education of physicians which the Association, as the accredited representative of the entire profession, should have itself taken in hand. We agree with Dr. Eve, in the firm belief that the Association has reached a period in its history, when it is entirely qualified to determine what medical organizations, be they State, county, or city societies, hospitals, boards, or schools, are entitled to be represented in its sessions, and to prescribe the qualification of such as shall be received as delegates.

"If created to improve and advance medical education, and this is in accordance with its own expressed declarations, then it is quite certain the schools must be controlled. It has but to speak on this point and it will be obeyed; for it is now too late for any physician to oppose, or any medical college to set at defiance, the moral power of the Association."

Let it place at the proper height the qualifications demanded of its own delegates, and require the evidence necessary to prove the possession of these qualifications on the part of those who are admitted to participate in the business of its sessions, and it will soon render its diploma of membership so certain a criterion of medical character and proficiency, that there will be excited a generous emulation among physicians, in the effort to render themselves qualified for its attainment, and such a demand will be created for augmented facilities for medical instruction, as must necessarily force the schools to bring up their courses fully to the standard prescribed by the Association.

The special committee whose report is before us, after a brief review of the several propositions submitted to their consideration, arrive at the following conclusions :—

"*First.* Primary medical schools should be encouraged, but, as office instruction will continue to be sought by students, practitioners should either give them the necessary advantages of demonstrations, illustrations, and recitations, or, if not prepared to do so, they should refer them to such primary schools or medical men as will give them proper instruction.

"*Second.* The number of professorships should not be less than seven, viz., a professor of anatomy and microscopy, physiology and pathology, chemistry, surgery, practical medicine, obstetrics, and materia medica.

"*Third.* There should be but one term annually, which should commence about the first of October and close with the March following, thus lengthening the term to six months. The commencement of the term in October should be uniform in all the colleges throughout the country. During the session, there should never be more than four lectures given daily.

"*Fourth.* The qualifications for graduation, in addition to those now required by the schools, should be a liberal primary education and attendance upon a course of clinical instruction in a regularly organized hospital."

The committee recommend that, in order to give our medical colleges an opportunity to consider the above recommendations, and that the Asso-

ciation may have the advantage of their wisdom and their mature views, a convention of delegates from the several medical colleges be held previously to the next session of the Association, to devise a uniform system of medical education.

The report which follows is on Spontaneous Umbilical Hemorrhage of the New-born, by Dr. J. Foster Jenkins, of New York State.

The particular form of hemorrhage treated of in this report is, it is true, of comparatively unfrequent occurrence—it is, nevertheless, met with sufficiently often to demand the attention of every practitioner. When the true character of spontaneous umbilical hemorrhage is overlooked, and inefficient measures are relied on for the arrest of the bleeding, its termination is almost invariably fatal.

Dr. Jenkins has, with commendable industry, collected whatever facts were to be found upon record, and to these has added some additional ones, the result of his own immediate observation, or communicated to him by his medical friends. All of which he has carefully analyzed, and presented in a tabular form.

Dr. J. describes two leading forms or varieties of umbilical hemorrhage in the young infant, as follows:—

"*First*, and most common, that depending on a depraved condition of the blood; the spanæmia resulting sometimes from jaundice, through malformation or deranged function of the liver, sometimes from an inherited scrofulous or syphilitic taint, and probably not unfrequently from privation and despondency in the mother during gestation, or, during the same period, an excessive use of alkalies or diluent fluids.

"*Second.* Independently of any dyscrasia of the blood, umbilical hemorrhage seems to arise by reason of an unusual patency of the umbilical vessels in otherwise apparently healthy children."

This abnormal patency of vessels Dr. J. believes may exist in the first form of umbilical hemorrhage, as well as be itself the apparently exclusive cause of the bleeding.

In addition to the ordinary causes of umbilical hemorrhage enumerated above, Dr. J. refers also to a diseased condition of the umbilical vessels, the result of inflammation.

The facts embraced in the report in question are derived from one hundred and seventy-eight cases. In only *nine* of these could the hereditary transmission of the hemorrhagic tendency be inferred, but there is not a single instance on record of its occurrence in the offspring of families in the individuals of which there existed a liability to hemorrhage generally from the slightest injuries. In by far the larger number of the cases in which the sex of the patient is given, hemorrhage from the umbilicus occurred in males. Climate or race would appear to exert very little if any influence in the production of the hemorrhage. In the majority of cases it is impossible to trace it to parental disease, and there is no reason for supposing that a tedious or difficult labour has any agency in causing it. External violence may give rise to hemorrhage from the navel by displacing the provisional clot, before the occlusion of the umbilical arteries.

The hemorrhage may be preceded by the indications of a deranged condition of the liver, more especially by jaundice, less frequently by purpura. The bleeding may take place before or after the separation of the cord.

"The average duration of the disease, after the appearance of the hemorrhage, in eighty-two fatal cases, was three and a half days. Death has occurred as early as three hours from the beginning of the hemorrhage, while life has been pro-

longed thirty-eight days thereafter. When speedily fatal, death seems most often to result from exhaustion from loss of blood; or the fatal issue may be deferred, to be at a later period attended by purpura, œdema, diarrhœa, *muguet*, or other signs of exhaustion. It is not strange that coma or convulsion complicated six cases."

From a *resumé* given by Dr. J. of the pathological appearances in those fatal cases of umbilical hemorrhage which have come under notice, he infers, 1st, the great frequency of hepatic malformation or derangement connected with the disease; and 2dly, a similar frequency of perviousness in some of the umbilical vessels, or other fœtal openings. The relation to each other of these two conditions we have not a sufficient number of pathological facts to decide. In seven of the cases in which there was absence or obstruction of the ducts, all the umbilical vessels were open in three, the vein only in one, in three the condition of the vessels is not noticed. In one case the biliary ducts were free, with patency of the fœtal openings. How far a deficient secretion of bile may be a cause has not been determined. No microscopic examination of the liver as to the extent of diminution of its secreting cells is known to have been made.

In the treatment of umbilical hemorrhage Dr. J. prefers the application of "the ligature *en masse*" as recommended by Paul Dubois. It is applied by transfixing the integuments at the base of the umbilicus, with two large hair-lip needles; the one being passed horizontally, and the second perpendicularly to and beneath this; waxed thread being then passed several times around each of the needles, in the figure of 8. The needles may be removed at the end of the fourth or fifth day, but the eschar produced by the ligature should be allowed to separate spontaneously, no attempt being made to hasten its removal.

In cases unattended by any evident malformation of liver, an attempt should be made to promote its functions. In cases where there had been *any* secretion of bile since birth, Dr. J. would not hesitate to employ mercurials despite their antiplastic influence on the blood. The mineral acids, the *tr. ferri chloridi* and the sulphate of quinine, anodynes to allay muscular spasm, and a nourishing diet, with stimulants to sustain the strength, would seem in many cases to be also indicated.

In regard to prophylaxis, a prohibition of alkalies and the use of the mineral acids during pregnancy is suggested.

We come next to a report on the Influence of Marriages of Consanguinity upon Offspring, by Dr. S. M. Bemiss, of Louisville, Ky.

It had long been asserted that the offspring of parents closely connected by consanguinity were far more liable to present physical and mental imperfections—various congenital abnormities—a strong proclivity to scrofulous and tubercular diseases, to idiocy and insanity, and to various affections eventuating in a deprivation of the sense of sight and hearing, and of speech, than the offspring of parents not connected by ties of blood—both classes being supposed to be similarly circumstanced in respect to all other causes affecting the integrity of their issue. In support of the position here assumed, many facts have been adduced of a very striking character; it is but recently, however, that the question has been made one of special inquiry, with the view to its solution by well authenticated and properly arranged statistics.

In whatever point of view the subject may be examined, whether in its bearings upon certain points in physiology, etiology, or pathology generally, or in reference to the regulation of marriage through an enlighten-

ment of public opinion or by legislative enactments, it is one of deep interest and importance. We feel grateful to Dr. Bemiss for the zeal and industry he has exhibited in the collection and arrangement of so large a body of facts, in illustration of the influence of the marriage of relations by blood upon offspring, as those he has embodied in the report now under consideration. The tables given by him exhibit the results of near nine hundred such marriages; a sufficient number, he believes, to warrant the belief that any additions thereto, if procured in the same manner, would not materially affect the ultimate result.

In the attempt of Dr. B. to collect a sufficient series of observations in respect to the physical and mental condition of the offspring of marriages between parties neither themselves related, nor the descendants of blood relations, as a standard by which to compare and determine the proper value of the results furnished by the first table, he has not been so successful as could be desired—and he has been unable to learn the existence of any researches, which establish the average fecundity and vital statistics of marriage in the United States.

Of the 833 marriages of consanguinity tabulated by Dr. B., there were born 3942 children—1509 males and 144 females. Of the 3942 children, 1134 were defective, 145 deaf and dumb, 85 blind, 308 idiotic, 38 insane, 60 epileptic, 300 scrofulous, 98 deformed, 883 of them died young, and 53 were sterile. Thus showing only 838 or 21 per cent. who were without defect or disease.

One hundred and twenty-five marriages among those not related or known to be the immediate descendants of relations, gave 837 children—444 males and 380 females. Of the 837 children, 18 were defective, 3 deaf and dumb, 1 blind, 6 idiotic, 1 insane, 3 epileptic, 1 scrofulous; 134 died young; leaving 58, or 46.4 per cent. without defect or disease.

The report next in order is on the Function of the Cerebellum, by Dr. E. Andrews, of Chicago, Ill. The view advocated by the reporter is that the lateral, which are strictly speaking the posterior, lobes of the cerebellum exert their influence over the posterior group of muscles, while the median lobe presides over the anterior group. This view, which at best must be considered as rather inferred than proved, is based upon the following two propositions, the truth of which Dr. A. endeavours to establish by an anatomical examination of the brain in different warm-blooded animals.

"1. In the warm-blooded animals, the median lobe, or vermiform process of the cerebellum, varies in size directly as the bulk and power of the anterior group of muscles.

"2. The lateral lobes vary in like manner as the power of the posterior group of muscles, subject, however, to certain variations, hereafter to be mentioned."

In reference to these exceptions, Dr. A. remarks that, upon comparison it will be found that the lateral lobes, while they have a certain correspondence with the posterior group of muscles, have one equally striking with the size of the cerebral hemispheres, and the rank of the animal in the scale of intelligence.

"There are, therefore, two conditions governing the lateral lobes as to size, which may be illustrated by three propositions :—

"1. If the animal have large complex hemispheres of the brain, and, at the same time, very powerful posterior muscles, then both these conditions unite in demanding large lateral lobes of the cerebellum, and these organs, in fact, attain, under such circumstances, their maximum development.

"2. If the hemispheres of the brain be of a low grade, but the posterior

extremities still powerful, then the deficiency in one condition is balanced by the excess in the other, and the animal will have lateral lobes bearing a moderate ratio of size with the median.

"If the animal have cerebral hemispheres of decidedly inferior development, and, at the same time, posterior limbs which are feebler than the anterior, then both conditions will coincide to reduce the lateral lobes to a bulk less than the median."

The tendency of all the facts adduced by Dr. A. leads, he believes, to the conclusion that the three lobes of the cerebellum have some function connected with voluntary motion; the influence of the median lobe being expended on the muscles of the anterior, and that of the lateral lobes on those of the posterior half of the body—the latter being likewise in some way connected with the mental functions, their development bearing, in some measure, a direct ratio with the intelligence of the animal.

"The *nature* of the influence exerted by the cerebellum upon the muscles is not," Dr. A. remarks, "very clear. Although it may be true that, through it, the mind co-ordinates the muscular action, yet *that* is not true which is stated by Carpenter and others upon the subject, viz., that the size of the cerebellum is in a direct ratio with the number and variety of co-ordinated movements which the animal is capable of exercising.

"Within the limits already explained the size of the cerebellum is *directly as the quantity and power of muscular fibre to be moved*, with no regard whatever to the simplicity or complexity of their combinations. Thus, the co-ordinated actions of a squirrel are far more numerous and varied than those of a dolphin. A mouse co-ordinates more than a bat; a rat more than a bird; a cat more than a seal; a sloth more than a sheep; yet in these and in a hundred other instances the animal which co-ordinates least has the largest and most complex cerebellum, the ratio being as the bulk of muscle to be used, and in the case of the lateral lobes, partly as the grade of the intelligence of the creature. It seems to me, therefore, that, while it may be true that the mind, through the cerebellum, co-ordinates motions, it does not do so because it possesses a specific function of co-ordination, but simply because its action is directly excito-motor, and the mind through it can select any muscle or set of muscles it may choose for action."

Dr. A. does not present his conclusions as absolute truths—they require, he admits, confirmation by a new and fuller examination of the facts derivable from experiment and from pathology—they at present merely show the *direction* in which the facts of comparative anatomy point.

The very interesting report by Dr. C. B. Coventry, of Utica, N. Y., on the Medical Jurisprudence of Insanity, embraces a general sketch of the pathology, the classification and the tests of insanity—the plea of insanity in criminal cases—the kind and degree of insanity that should deprive an individual of his liberty, or of the control of his property—invalidate a contract or a will, or avoid responsibility for crime—feigned insanity and the testimony of skilled witnesses in courts of justice.

Each of these questions are every day acquiring increased importance in reference to the faithful administration of justice and the protection of persons and property with a due regard to the safety and humane treatment of such individuals as are unquestionably irresponsible agents by reason of their mental alienation. These several questions, though treated in a concise manner, the remarks of the reporter in regard to them are replete with instruction.

Dr. C. lays down the following propositions as established in this country, by various judicial decisions, in respect to the plea of insanity as a defence in criminal prosecutions:—

"1. Was the prisoner, at the time of committing the act, capable of distin-

guishing between right and wrong, in regard to the particular act?—*i. e.*, was he capable of appreciating the criminality of the act, and the consequences which would result, and the penalty for its commission? If so, he is considered responsible. Or,

"2. Was the prisoner, at the time of committing the act, labouring under an insane delusion? The mere fact that the prisoner was labouring under a delusion is not considered a justification, unless there is a direct connection between the delusion and the criminal act. Or,

"3. Was the prisoner impelled to the commission of the act by an insane impulse which he could not control? By an insane impulse is not meant a mere burst of rage, or passion, or excitement, which he may not control, but an impulse resulting from cerebro-mental disease; consequently it is necessary to show some other evidence of disease than the act itself—as a change in his character; its incompatibility with his former character and conduct; the absence of all motive, or of motives, which could influence a sane man; the existence of insanity in his family, or a hereditary predisposition; a previous injury, or ill health, showing evidence of disease. In the absence of such collateral evidence, the prisoner would usually be held responsible.

"The question to the jury should be simply, Is he proved to be insane? not whether he can distinguish between right and wrong, or any other particular test. The divisions into intellectual and moral insanity, into general and partial, though appropriate in a scientific work on insanity, are not recognized by the courts of justice."

The rules and regulations which Dr. C. has laid down for the conduct of a professional witness, in cases in which the plea of insanity is raised, are well worth the consideration of every physician.

The volume contains another able report, closely connected with the foregoing; we allude to that on Moral Insanity, in its Relations to Medical Jurisprudence, by Dr. D. Meredith Reese, of New York.

That the moral feelings, the affections, and the sentiments, "the love of family and friends, the love of justice, and the feelings of veneration and of conscientiousness," may become deranged, perverted, or destroyed, there can be no doubt. But with this admission the all-important question arises, is this derangement the result of causes different from those which give rise to intellectual insanity? Can moral insanity exist independently of the latter? or is it not rather one of the indications—often the first—of the presence of that disease of the brain productive of general mania—that is, of perversion or disturbance of all the mental functions, in reference alike to the moral feelings, the affections and sentiments, and to the purely intellectual powers? Can the mere fact of an asserted perversion of the feelings, inclinations, and propensities—of the existence of simply moral insanity—while the individual possesses sufficient intelligence to judge of the character and consequences of his actions, is labouring under no delusion, and is, consequently possessed of the ability to control his acts, be a sufficient cause for declaring him irresponsible for his deeds? Would the ends of justice be likely to be satisfied, and a proper protection thrown around the life and property of the citizen, were such a foundation for irresponsibility in those accused of crime to be entertained by our courts?

Dr. Reese very clearly shows that simple moral insanity has no existence. We commend the conclusions of the author to attention.

Turning back, we find a report by Dr. Edward Jarvis, of Massachsetts, on the Law of Registration of Births, Marriages, and Deaths, which is replete with judicious and instructive remarks on the subject of registration. To render this measure productive of all the advantages it is calculated to yield, it must be evident that some degree of uniformity is demanded in the manner

of registration everywhere pursued; that the same facts, similarly authenticated, and arranged in the same manner, the same nomenclature of diseases, and the same systematic indication of the actual cause of death in the mortuary department of the registration, should be presented, to enable us to compare the value of life in different localities, and to determine, as far as possible, the causes which lower or increase its value, as the case may be, in each. Let the great events of life, with their attendant circumstances, be fully and accurately recorded and reported, so that their lessons may be correctly taught to the world, and we agree with Dr. Jarvis that "it matters not by what means they are ascertained."

The next report is on the Nervous System in Febrile Diseases, and the Classification of Fevers by the Nervous System, by Dr. Henry F. Campbell, of Georgia.

The author of this report has shown himself to be an industrious, able, and successful investigator of the nervous system in certain of its physiological and pathological relations. He is already well known to our readers by his publications in respect to the secretory and excito-secretory system of nerves, and by his suggestions, in a former volume of the *Transactions of the American Medical Association* (vol. x.), presented in the form of general propositions, in regard to the agency of the nervous system in the production of febrile diseases.

Fully admitting the fact that in the production of disease there are certain morbific influences which affect the solid tissues through the medium of the blood, producing in the latter some primary modification either in its constitution or elements, Dr. C. nevertheless insists upon the positive agency of the nervous system in the production of diseased action, in causing the presence or absence of certain phenomena, and in impressing their peculiar characteristics upon the different forms of febrile maladies; thus combining in one system "the inevitable truths of humouralism with the equally inevitable and undeniable truths of neuropathism."

Assuming, then, that all morbid actions are more or less influenced in their manifestations by aberrated nervous action, he insists that as the normal actions of the *cerebro-spinal system of nerves* are subject to cessation and interruption, so those fevers, in the production of which this system is chiefly concerned, are necessarily of a *paroxysmal* character; while inasmuch as the normal actions of the *ganglionic system of nerves* are of a continued or uninterrupted character, so diseases, the morbid phenomena of which are chiefly caused by abnormal action of these nerves, have a character of *continuousness*. The *paroxysmal fevers* appertain, therefore, to *lesions of the cerebro-spinal system of nerves*, and the *continued fevers* to the *ganglionic*. Again, Dr. C. teaches that as the normal action of the *cerebro-spinal* nerves pertains almost exclusively to sensation and motion, with only a secondary and comparatively remote influence—the excito-secretory—upon nutrition and secretion, while the entire office of the *ganglionic* system, in health, is to preside over these functions, therefore in *paroxysmal* fevers we have intense pain, modified sensation, and symptoms allying them to neuralgic and convulsive diseases, but in *continued fevers* the most prominent characteristics are modified nutrition and altered secretions.

In the report before us these several propositions are separately examined, and the process by which they have been arrived at concisely developed. In his exposition of the leading data upon which he has attempted to base a true neuropathic system of pathology, and by which he has constructed a rational physiological classification of febrile diseases, Dr. C. has certainly

exhibited no little ingenuity, and has succeeded admirably in giving to both an air, at least, of truthfulness. To say the least of them, his labours are well timed, and are invested with an additional importance from the fact that in our modern doctrines explanatory of diseased action and morbid phenomena, and in all our nosological systems, there has been evidently a constantly increasing tendency to the adoption of views borrowed almost exclusively from the teachings of the physiological and pathological chemists, ignoring in a great measure the agency of nerve-action as a pathological agent, debasing the living organism, with all its vital endowments, to a mere machine, of which all the phenomena, normal and abnormal, if not solely, are at least mainly, due to a simple action and reaction between the constituent elements of the blood, to unhealthy secretions, and to a consequent defect in the construction and condition of the solid tissues.

Whether or not the views of Dr. C. shall stand the test of a more close investigation; whether or not they shall be sustained by the facts hereafter to be developed and by the results of more extended clinical observations or direct experiments, they will have, at least, the effect of directing renewed attention to a subject the investigation of which cannot fail to be productive of profitable results.

We have next a report on Stomatitis Materna, by Dr. D. L. McGugin, of Keokuk, Iowa. The disease thus named has of late years prevailed, in certain portions of the United States, to a considerable extent. By many writers it is considered to be a disease of modern origin and of a specific character, occurring only in females during the period of pregnancy or lactation, especially the latter, being restricted to particular localities, and described solely by medical writers of a recent date.

Dr. McGugin has, however, shown that in medical writings dating as far back as the times of Hippocrates, a notice, and even accurate description, of an affection similar in all respects to stomatitis materna, as it now prevails, is to be met with. He insists, however, that the disease is one *sui generis*, peculiar to females during gestation and lactation, and occurring more frequently in some localities than in others, while in a few it has not as yet been observed; that its production cannot be ascribed to any peculiarity of climate, locality, season, meteorological conditions, or geological formations. He most positively denies its identity, in either its etiology or pathology, with ordinary chronic stomatitis, with which it has been classed by some writers, or with land scurvy, of which it has been supposed to be a form by others.

The only constant predisposing cause observed by Dr. McG. was a strumous, tubercular, or scrofulous diathesis. He, at the same time, however, would seem to admit that intense cold, excessive atmospheric moisture, insufficient or innutritive diet, want of due exercise, and malarious influences may, possibly, act also as predisposing causes, by contributing to a diminution of the vital forces, by rendering digestion imperfect, nutrition defective, and the blood deficient in its proper constituents.

With Dr. McG. stomatitis materna consists essentially in a perversion of digestion and assimilation. Gestation and lactation produce it by their interference with the regular and complete performance of those functions.

We cannot say that, after a careful perusal of the report before us, we have become converts to the views set forth by Dr. McG., in regard either to the specific character, properly speaking, of the disease of which he treats, or to its being one that is met with only in the pregnant and suckling female. We cannot perceive that he has adduced a particle of evidence in proof of

either position. That the disease is one having its origin in a defect of constitutional vigour and an impoverished state of the blood, caused by depressing meteorological causes, bad or insufficient food, want of exercise, grief, anxiety, despondency, &c., combined, in the majority of cases, with the new demands upon the weak, delicate, and chlorotic female, during the process of gestation or of lactation, we think there can be little doubt; but we do not believe it to be confined to the female; the whole history of the disease, its causes, phenomena, course, and terminations, would seem to point strongly to the fact of its identity with ordinary *follicular stomatitis*, which we know to be a common consequence of deficient hæmatosis and depraved nutrition in both sexes.

The treatment which Dr. McG. has found most successful in cases of stomatitis materna is by tonics, especially such of the metallic tonics which possess alterative properties. Nitrate of silver and iodide of iron he recommends, if they are tolerated by the stomach. The "hydriodate and chlorate of potassa," either alone or combined with vegetable tonic extracts or infusions, will also, he informs us, be found highly beneficial. What he most relies upon is the chlorate of potassa, internally, in the form of a solution, one drachm to a pint of distilled water, in the dose of a tablespoonful three times a day. Pastes, saturated with the solution he directs, to be allowed to soften in the mouth and mingle with the saliva. He recommends, also, the subnitrate of bismuth, made into pills with the extract of hop. Malt liquors with a large amount of malt, and given in an active state of effervescence, he has found particularly beneficial. To insure the cure of the disease, regular exercise in the open air, with tepid bathing, are essential.

"If, however, all measures fail to bring relief, and the patient is, day after day, wasting away under the exhausting influence of the disease, then, in order to save the life of the mother, the dernier alternative of weaning the child must be determined upon and fulfilled. This, however, will not always arrest the progress of the disease, when it has become chronic in its character, and has continued through several pregnancies."

Dr. J. B. Flint, of Louisville, Ky., furnishes an interesting and most truthful report on the True Position and Value of Operative Surgery as a Therapeutic Agent.

Surgery, even in the limited sphere to which it must be restricted, if we insist upon a close adherence to the true etymological meaning of the term, as simply the art of employing manual or mechanical appliances for the prevention or relief of deformity, the excision of diseased structures, and the removal of morbid accumulations or foreign bodies, accidentally introduced, from one or other of the cavities, passages, organs, or tissues of the body, the presence of which either interferes with the healthful performance of the functions of the organ, or threatens the extinction of life, is certainly an art from which the human race has derived a large amount of positive good; and though it can scarcely be denominated, with strict propriety, a therapeutic agent, it is nevertheless one that is capable of the most important preventive and remedial application.

Mere operative surgery, however, the certain result of which is, in the majority of cases, more or less of deformity or decrepitude, must be regarded as among "the last resorts" of practice; as a means to be brought into service only after all other available means of saving limb or life have been exhausted. To conservative surgery is to be assigned, most certainly, a far higher rank than to mere operative surgery. It is unquestionable that the acknowledged masters of the art have become so, and secured their im-

perishable renown, not by their daring and successful use of the knife, but by their careful observation and study of the nature and phenomena of disease, and of the tolerating and recuperative powers of the living organism, and by their discriminating and cautious rather than heroic methods of treatment.

In the report before us, after a most scathing exposure of the rank charlatanism to which a love for operative surgery so commonly leads—of the unworthy and even dishonest tricks that are too often practised in order to attain that *éclat* which is almost always attendant upon him who prefers "the shedding of blood" to the nobler and more scientific plan of saving life and limb without a resort to the knife, and to secure that higher estimate which the public voice awards to the surgeon who is noted for the frequency and boldness of his operations, without much regard to their necessity on the one hand, or their successful termination on the other, than to him who, without parade, prefers to cure his patients by therapeutical means of which the knife forms no part—Dr. Flint examines, with great calmness and sound sense, the true value of operative surgery as a curative measure. In deprecating a frequent resort, or in any instance, without the most urgent and evident necessity, to any of what are termed the "major operations," Dr. Flint would not be thought to entertain any disposition to undervalue operative surgery, either as to the frequency or importance of its interpositions in the treatment of disease. While he would include very few of the so-called major operations among "the legitimate resources of the healing art," he would have us bear in mind "that no such doubt or reservation applies to that great class of professional services unfortunately denominated '*minor surgery.*'"

The more prominent points in the report before us are thus recapitulated in the form of concise propositions, all of which are deserving of a serious consideration on the part of every member of the medical profession, but more especially of those who have but recently entered upon the practice of the healing art, and who are among those most liable to be misled by what Dr. F. denominates "dramatic surgery:"—

"1. The true position and real dignity of all therapeutic agencies is the same, determined by intrinsic and not accidental considerations; and operative surgery has no rightful pre-eminence, in this respect, over other sections of practical medicine.

"2. There are *pseudo-surgical* as well as *vero-surgical* operations, the latter being such only as are undertaken with a therapeutic purpose and *probability;* and if any of these confer more distinction upon him who performs them than others, they are such as are most eminently beneficial to the subject of them.

"3. Of this character are not, in general, those which constitute the staple of what we have termed 'dramatic surgery,' denominated also 'heroic' or 'exploitive' surgery, and these performances, therefore, should be scrutinized very carefully before they are reckoned among the legitimate agencies of our art, or allowed to become the criteria of professional ability or merit.

"4. The fascinations of 'dramatic surgery' are dangerous to professional morality and mischievous to society, and we should endeavour to replace them, in ourselves and the public, by just and rational views of the operative proceedings of our art.

"5. Under the designation of 'conservative surgery,' there is at present an endeavour at reforms in operative surgery, which recommends itself most urgently to the co-operation of all wise and conscientious practitioners."

Passing by a short letter addressed by Dr. E. D. Fenner, of New Orleans, to the President of the Association, and which we do not think was intended for publication as a part of the transactions of the session, we come to the

prize essay on the Clinical Study of the Heart-Sounds in Health and Disease, by Dr. Austin Flint, of Buffalo, N.Y. Like every other production of its talented author, it bears throughout testimony to his habits of close observation and cautious induction. It constitutes a valuable contribution towards increasing our acquaintance with the character of the cardiac sounds in health, and the lesions by which a deviation from these sounds is caused. We regret that a due regard to that economy of space which it is necessary for us to observe, in view of the many works which crowd our table, waiting to be noticed, prevents us from giving even the interesting summary of conclusions, but we hope to lay them before our readers. D. F. C.

Two papers in this volume, both on the same speciality, remain to be noticed. The first of these is a

"Report on the Treatment best adapted to each Variety of Cataract. With drawings illustrating a case. By Mark Stephenson, M. D., Surgeon to the New York Ophthalmic Hospital."

The second is the "Prize Essay," entitled "Vision, and some of its Anomalies, as revealed by the Ophthalmoscope. By Montrose A. Pallen, M. D., of St. Louis, Missouri."

Questions in ophthalmology are so rare among the various inquiries which have hitherto claimed the consideration of the American Medical Association, that the appearance of the two papers above presented will invest the new volume with unusual attractions, for a very respectable number of its readers. The interest, moreover, which belongs to the nature of their topics is necessarily increased by the influence, inseparably connected with the official positions of these papers, one being a special report and the other a prize essay. The topics themselves are certainly not inferior in importance to very many of those which are allowed to swell the too often overgrown dimensions of the annual offspring of the labours of the Association. It is strange, indeed, that what has occupied the elaborate attention of an European Congress, recently assembled for the sole purpose of discussing questions in ophthalmology, and composed of the ablest professional experts in the world, has so long escaped the attention of the great representative body of the profession in this country.

This oversight cannot have resulted from want either of materials or of workmen, or of general interest among actual practitioners.

Under these circumstances we are disposed to hold the Association to a double responsibility on this occasion; and we purpose, briefly, to inquire whether, after neglecting for ten years to call forth anything of moment on diseases of the eye, the Association has succeeded in presenting, in the first place, a report on cataract and its treatment worthy of the standing which, in spite of its "conspicuous" disclaimer, it has conferred upon that report by its acceptance and publication; and whether, in the second place, its protracted silence on such topics has been adequately broken in the originality, novelty, and superior practical value of the essay on vision and its anomalies, as revealed by the ophthalmoscope, upon which it has set the seal of its highest approbation in the award of an annual prize.

We fear that the expectations naturally aroused in both cases, are destined to a general disappointment.

The author of the report on the Treatment best adapted to each Variety of Cataract, was authorized to prepare his "Essay on the Subject of Cataract," as he terms it, in May, 1856. He appears to have entered upon his task under a due impression of its "interesting and momentous" character,

and to have been duly surprised that it had not already been accomplished. Notwithstanding the two years allowed him, and his full appreciation of the importance and difficulty of his undertaking, we regret to find that, while he has not methodically treated the points which were specially allotted to him, he has succeeded no better in producing anything which deserves to be regarded as an original or practical report. He tells us nothing which is not presented with equal, if not greater clearness, force, and fulness in the standard text-books on surgery, and which is not decidedly meagre and common-place in comparison with the contents of the chapters on the same subject in each of the standard works upon diseases of the eye. In short, we have looked in vain throughout the report for something in the shape of cases, statistics, observations, or suggestions, derived from research or clinical investigation, that is sufficiently novel, or interesting in any respect, to justify the space occupied in the volume, or the prominence conferred by the impress of the Association.

The only two clinical cases referred to in illustration, are inconclusive and unsatisfactory to a remarkable degree. The first of these was that of a "child between three and four years of age," upon whom he divided the lens with a curved needle by the "anterior operation" for *traumatic* cataract. "There being no haste in this case, *one eye being good*, I determined to test what the efforts of nature with time would accomplish, without a repetition of the operation." At the expiration of one year, some little absorption had actually taken place! A drawing "of the lens, as it then appeared," was taken at this time, and a second drawing was made "at the expiration of six months more," showing that the "work of diminution was steadily yet slowly progressing"! In order to illustrate this precious instance of the result of a single operation for the absorption of a cataractous lens, the paper has been illuminated with five coloured figures, which serve only to show the *failure* of the operation in the small amount of the absorption depicted, and to suggest a doubt, even as to this amount, from the want of correspondence between the form of the unremoved cataract, in the two representations of the same eye, one of the natural size and the other a magnified view of the same eye. Compare Figs. 4 and 5, and it will be observed that the shape of the unabsorbed cataract is entirely different in the two, though the latter purports to be a magnified view of the former; this is also the case, though less conspicuously, in Figs. 2 and 3.

The termination of the history is equally characteristic and instructive.

"Soon after the second drawing was taken," says he, "to my great regret, the parents of the child removed to California, and (although they promised to write to me) *I have never heard from them since*, but I have no doubt as to the final result." "This case," he continues, "is interesting, and proves to my mind a practical fact which I have long entertained, that the frequent repetition of the operation is unjustifiable."

The second case is related as one in which he had an opportunity, a few years since, of testing the question of double vision, as a consequence of the removal of one lens only in case of single cataract. The subject of this case, a "beautiful young woman, was brought to the institution by the matron of the Magdalene Asylum, who had a cataract in one eye, while the other was perfect. She could not endure the thought of having such a blemish in one eye," and therefore "she wished the cataract removed." Here, again, he performed the anterior operation for solution; but, naturally enough, with much more gratifying success.

"In eight weeks the cataract was entirely removed; and, what was still more pleasing, she could see with that eye as perfectly as the other, there being no confusion of vision whatever. The matron of the institution—a Christian lady in the fullest sense of that term, and in whose veracity I have the strongest confidence—informed me that this young woman could, without the aid of glasses, sew the finest linen cambric muslin with that eye. From what I saw of her, when she returned to the hospital the last time, I am satisfied the statements of the matron were, in every particular, strictly correct."

His readers will, doubtless, readily believe in the escape from double vision; but it will require more than the veracity of the exemplary matron to convince them that the patient could sew so wonderfully "with that eye"! The author hints that he can only account for this "strange" result of eight weeks' work, by the supposition "that a new lens, or something of the same consistency or translucency, is reproduced or secreted, which answers the purpose of the original one"!

The only other remark we have to notice is one in reference to displacement, in which he says, after stating in detail the injurious effects of the operation, and the consequent objection to it: "Notwithstanding these detracting evidences, *displacement* is the operation generally preferred by American surgeons, while in England and Germany it is in disrepute." We must take the liberty of expressing our doubts in regard to the accuracy of this opinion as to the general preference of American surgeons. We know of no evidence to support it, and we have too much faith in the knowledge and judgment of the ophthalmic surgeons of our country to believe anything so discreditable to either. Whatever may be the practice of some few surgeons of standing, who still resort to a procedure which, on the reporter's own showing, is a bad one as it is usually performed, and in spite of the same assumption on the part of a recent writer in a distant State, we cannot believe that it is ever willingly performed by experienced operators in this country. It certainly is not, to any extent, in Philadelphia; where, unless Prof. Pancoast's operation, which is not the ordinary method of displacement as he describes it, be included, the couching and depressing methods are condemned, and that of lateral displacement is not generally adopted. We have no hesitation in expressing the opinion that the "Report," as it is called, "on the Treatment best adapted to each Variety of Cataract," is a misnomer, and the attempt thus to determine very important questions, a miscarriage.

Whatever doubt there may be, however, in regard to the liability of the Association for the character of ordinary papers and reports, is, of course, removed so far as prize essays are concerned. Here, at all events, there can be no mistake as to the position of the ruling power. It has intrusted its reputation, not to the authors of the essays, but to the Standing Committee on Prize Essays, and must hold the latter, not the former, to accountability. Whatever may be overlooked in an ordinary report, for which the Association refuses to become responsible although the publication occurs under its own authority, we have a right to expect in an essay, crowned with the title of pre-eminent merit by the assembled medical wisdom and learning of the country, something more than a useful compilation, however well arranged, well expressed, and accurate and comprehensive it may be. Now, this is what we do not find in the otherwise very creditable thesis of Dr. Pallen. As a report on the ophthalmoscope, and its use in the exploration of the eye, and divested of its preliminary matter, it might be regarded as a desirable contribution to the *Transactions* of the Association, and, as such,

might be entitled to a respectable position in the volume; but as a prize essay on vision, &c. &c., it is surely out of place, and neither receives justice itself as a volunteer communication, nor does justice to the Association as a professional and scientific body. There is nothing in the rudimentary account of the anatomy and physiology of the eye, with its diagrams and zoological introduction, which prefaces the somewhat more elaborate sketch of what is known of colour-blindness and some other anomalies of vision, or even in all of this together, that need have led the prize committee to mistake a praiseworthy exposition for an original production, or even a professor's *thèse de concours*. The interesting account of the ophthalmoscope, its employment, and the pathological changes of the internal eye which may be observed by it, as derived especially from French and German teaching, was cleverly drawn up, and justly elicited the approbation of the committee; still, they could hardly have accepted it as new, or as the result, beyond the mere act of arrangement at second hand, of *native* investigation; nor could they have considered the intelligent rehearsal of European experience, as such, entitled to the doubtful advantage of *national* supremacy in a professedly American work. We are at a loss, therefore, to comprehend the *rationale* of the process by which they reached a conclusion which must compromise the Association with all readers, at home and abroad, who know anything of the progress of the science of ophthalmology in the last five years. Happily the number of those who are thus informed in this country is by no means small; and enough has been done by American writers on the ophthalmoscope to redeem the profession of the country from the mortifying imputation to which the hasty action of the prize committee has subjected our national congress.

Perhaps, since there are no marks of original investigation, no statistics, no records of observation or experiment, nothing, in short, but the literary and critical, if not practical, merit which should characterize every paper admitted into the published *Transactions*, the hearts of our respected umpires were won, or their own visions confused, by the imposing array of illustrations which are appended to the essay. Here, however, the committee is again unfortunate, inasmuch as the pictures, although excellent in themselves, are nearly all "verified and coloured" copies from Jaeger, and present faces which had already become familiar to the student as well as the practitioner in many parts of the United States, to say nothing of British and continental readers. Before closing our remarks, we desire distinctly to say that it is the prize committee, and not the essayist upon whom the responsibility in this case rests. We would under no other circumstances be willing to notice this essay except in terms of commendation, as creditable to its youthful author. Although in some respects it is not the kind of dissertation on such a subject which additional experience would most probably induce him to prepare, it yet affords abundant evidence of well-trained industry, talent, and ability, which cannot fail to reap more substantial and permanent rewards than the too often equivocal, and in this instance doubtless unexpected, distinction of a prize. E. H.

ART. XIII.—*A Treatise on Human Physiology, designed for the use of Students and Practitioners of Medicine.* By JOHN C. DALTON, Jr., M. D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, New York, &c. &c. Blanchard & Lea: Philadelphia, 1859. 8vo., pp. 608.

A NEW TEXT-BOOK on physiology every two or three years has become a necessity. The numerous contributions constantly being made to that science require frequently to pass under the eyes of able and experienced men in order that they may be accepted, modified, or rejected, and, if found worthy of being incorporated into the mass of facts we possess, placed before the profession in a form accessible to all, and adapted to the comprehension and wants of the student.

It requires no ordinary mind to do this well. We know it is the generally expressed opinion that any one can write a text-book; this may be true, but few, notwithstanding, can write a *good* text-book—one which, while concise, is not meagre; full, but not diffuse; exact, yet not tediously minute; systematically arranged, but not fettered to classification—such a book requires something more in its author than a mere acquaintance with the science he teaches; it necessitates a logical and well-balanced mind, not, perhaps, genius, or even the highest order of intellect, but, nevertheless, those best of all substitutes, good common sense and a well regulated understanding.

Of all sciences, none have probably made more progress within the last few years than physiology. Hundreds of facts have been added, old theories have been disproved, and new ones have taken their place, until hardly a vestige remains of physiology as it was twenty years since. The sciences of physics and chemistry have, to a certain extent, become tributary to physiology, and to physicists and chemists we owe a large number of those valuable contributions which have almost rendered it a positive science.

So long as physiologists were content to explain all functional phenomena by designating them as manifestations of vital force, but little real advancement was to be expected. Investigation was crushed by an abstract idea which signified nothing, and the application of the laws which govern the natural phenomena of the universe to the human body when hinted at, was smothered in the torrent of denunciation which followed.

Gradually, however, things changed as one function after another was ascertained to be due to simple and known causes, until now, when, though we must all recognize the modifying influence of vitality over every function, we can explain none by it alone. Heat is produced by the combination of carbon and oxygen, and follows the union as surely out of the body as in it; food is acted upon by saliva and the gastric, intestinal, and pancreatic juices, as effectually in a porcelain capsule at 100° Fahrenheit as within the alimentary canal, and absorption takes place through living and dead animal membranes equally well. Within the body, however, there are so many disturbing causes resulting from vitality, or, as Beclard designates it, "organization in action," that all these processes, together with others we have not mentioned, are so palpably modified that we cannot, in wisdom, deny its influence. Thus far we may safely go; but those who would degrade the human soul to the position of a vital principle, as they understand it, have neither religion nor science to sustain them, and they who

reject the influence of vitality over the functions of the body have no just appreciation of the delicacy, the beauty, and the completeness of the great organic kingdom established by nature's Architect.

From the known character of Dr. Dalton as an original observer, we had every reason to expect that a treatise on physiology from his pen would not only exhibit a satisfactory view of the actual condition of the science, but would, at the same time, be marked by that independence of thought and confidence of expression which are not to be obtained by merely reviewing the labours of others. These expectations have, in a great measure, been realized in the volume before us, but, at the same time, we are compelled to confess having experienced a feeling of disappointment that Dr. Dalton should have omitted some important subjects, the want of reference to which impairs its completeness as a text-book for students. Nevertheless, there is so much original matter, so much that is doubly valuable because confirmed by observation and experiment, and so true an idea of the manner in which physiology should be studied expressed upon almost every page, that we cannot but admire it, and console ourselves with the hope that the omissions will be supplied in a future edition, which we feel sure will be demanded. If, therefore, in the course of our remarks, we should have occasion to differ with the author, and to state what we consider to be the deficiencies of the volume, we shall not be chargeable with a desire to detract from its real value, a value which, founded as it is upon beauty and fulness of illustration, elegance of diction, and originality of thought, will long cause it to retain an elevated position in the ranks of physiological literature.

In the review of a volume of the character of that under consideration, it would be inexpedient to notice in detail all the subjects treated by the author, for, being a text-book, it must of course contain a large amount of matter found in every other treatise of the kind, and therefore perfectly familiar to most of our readers. We shall, accordingly, state briefly the general plan and arrangement of the work, and then remark upon such points as, in our survey, we may deem of most importance either from their value, their novelty, or from the general interest which attaches to them.

In the introduction physiology is defined, and divided and subdivided, according as it relates to vegetable or animal structures, till human physiology, to which the attention is mainly to be directed, is reached. In the opinion of the author, that this latter division cannot be properly studied or comprehended, without an acquaintance with the vital phenomena occurring in vegetables and animals, generally, we heartily agree. It is too true that at the present day many calling themselves physiologists are disposed not only to overlook, but even to decry, the advantages to be derived from experiments instituted upon the lower forms of organic beings, forgetting, apparently, that to investigations of this character, we owe the greater portion of our present physiological knowledge, and that from them we have every hope of future progress.

Vital phenomena are referred to under the following heads: 1st. Those of a physical character, such as the formation of the voice, the motions of the heart, &c. 2d. Those of a chemical character, embracing the transformation of food, &c. 3d. Those connected with reproduction and development; and, 4th, those which belong to the nervous system. The views advanced by Dr. Dalton, in connection with these points, show that he fully recognizes the influence of vitality, without ascribing more power to it than it really possesses.

In relation to the manner in which it is proposed to study physiology, we cannot do better than quote the author's own words.

"The study of physiology is naturally divided into three distinct sections:—

"The first of these includes everything which relates to the nutrition of the body, in its widest sense. It comprises the history of the proximate principles, their source, the manner of their production, the proportions in which they exist in different kinds of food and drink, the processes of digestion and absorption, and the constitution of the circulating fluids; then the physical phenomena of the circulation, and the forces by which it is accomplished; the changes which the blood undergoes in different parts of the body; all the phenomena, both physical and chemical, of respiration; those of secretion and excretion, and the character and destination of the secreted and excreted fluids. All these processes have reference to a common object, viz., the preservation of the internal structure and organization of the individual. With certain modifications, they take place in vegetables as well as in animals, and are consequently known by the name of the *vegetative formations*.

"The second section, in the natural order of study, is devoted to the phenomena of the NERVOUS SYSTEM. These phenomena are not exhibited by vegetables, but belong exclusively to animal organizations. They bring the animal body into relation with the external world, and preserve it from external dangers, through the means of sensation, movement, consciousness, and volition. They are more particularly distinguished by the name of the *animal functions*.

"Lastly, comes the study of the entire process of REPRODUCTION. Its phenomena, again, with certain modifications, are met with in both animals and vegetables, and might, therefore, with some propriety, be included under the head of vegetative functions. But their distinguishing peculiarity is, that they have for their object the production of new organisms, which take the place of the old, and remain after they have disappeared. These phenomena do not, therefore, relate to the preservation of the individual, but to that of the species; and any study which concerns the species, comes properly after we have finished everything relating to the individual."

In the first chapter of the section devoted to nutrition, the proximate principles are treated of in a general manner, and after Robin and Verdeil, they are defined to be those substances which, simple or compound, chemically speaking, exist under their own forms in the animal tissues or fluids. Under this head, we meet with various substances, which, adopting Robin and Verdeil's classification, are arranged as follows, into three divisions:—

1st. The INORGANIC, as water, chloride of sodium and potassium, carbonate and phosphate of lime, &c.

2d. The CRYSTALLIZABLE SUBSTANCES OF ORGANIC ORIGIN, as the different kinds of sugar, fat, and starch.

3d. ORGANIC SUBSTANCES proper, as albumen, fibrin, casein, globulin, &c.

In the three following chapters, these principles are considered in detail, concisely, clearly, and, in general, fully and correctly; we say in general, because we think that in one or two instances the descriptions have not been so exact as they might have been, and occasionally an important substance has been altogether overlooked. Thus no reference is made to the property which globulin possesses of being transformed, under certain circumstances, into hæmato-crystallin—the crystallizable albuminoid body of the blood. We are aware that Robin and Verdeil deny the existence of any proximate crystallizable organic principle existing in the blood, regarding the crystals met with in this fluid, as consisting of phosphate of soda, which, in crystallizing, abstract a little albumen and coloring matter from the blood in which they are formed; but such an opinion is not tenable for

a moment, and has been completely disproved by the analyses of Lehmann and others.

A few other organic principles which might very properly have been enumerated here, are described in detail in other parts of the work, under their appropriate heads.

The fifth chapter treats of *food.* Dr. Dalton very properly discards Liebig's division of aliments into combustible and histogenetic—a division which cannot be sustained either by argument or experiment, and which only retains a place in physiological literature, from an overweening respect for the eminent ability and high scientific character of its proposer. No one elementary article of food can be regarded as exclusively serving for the maintenance of heat, or as contributing entirely to the nutrition of the tissues. Without fat, there is every reason to believe a single organic cell could not be formed, and under a regimen of pure albumen alone, it has been shown that the animal heat can be sustained. The composition of several of the more important articles of food is given, and it is shown that the nutritive value of any one of these does not depend upon its containing a large amount of any individual alimentary principle, but upon the presence of all these principles in such proportions as the system requires.

From original observations, Prof. Dalton found that an adult man, in full health, and taking free exercise in the open air, required about two and a half pounds of solid food, and three pints of liquid food, divided as follows:—

Meat	16 ounces.
Bread	19 "
Butter	$3\frac{1}{2}$ "
Fluids (coffee and water)	52 "

The nutritive value of any article of food cannot be positively inferred from its chemical constitution, but must be determined from actual experiment. If all substances were equally digestible, we should be able at once to state their value in the economy—but this is not so; bone and cartilage, though capable of being digested to a certain extent, by carnivorous animals, are not sensibly acted upon by the human gastric juice, and therefore, though possessing all the elementary principles required by the organism, they cannot contribute to its nutrition. The same may be said of gum, which differs but slightly in composition from starch, but which is not so acted upon by the digestive fluids as to fit it for absorption.

With regard to the action of the *saliva,* Prof. Dalton adopts the view that, although this secretion possesses the power of changing amylaceous substances into sugar, this faculty is not practically exercised, inasmuch as the gastric juice entirely prevents the metamorphosis. There is no doubt, we think, that the gastric juice very materially retards the conversion, but that it entirely prevents it is not so certain. Prof. F. G. Smith, of this city, from his experiments upon Alexis St. Martin, was led to the conclusion that the change is not arrested in the stomach, but that glucose can readily be detected in the contents of this viscus after the ingestion of amylaceous food. Whatever objections may be urged against these experiments cannot apply to a second series instituted by the same observer upon M. Brown-Séquard (who possesses the faculty of emptying the stomach at will), from which it was clearly shown that sugar was present in the contents of the stomach after amylaceous food had been taken which certainly did not contain it. The point is one, however, upon which physiologists will continue to differ till it is more satisfactorily determined.

The *composition, action, &c., of the gastric juice* are next reviewed. In stating the numerous experiments instituted with reference to the nature of the free acid existing in this secretion, no mention is made of Prof. F. G. Smith's observations, which seem to show the presence of lactic acid in this secretion.

The remarks upon the action of the gastric juice are full and judicious, and the whole subject is presented to the reader in the clearest possible manner.

The *intestinal* and *pancreatic juices* are then considered, and their special properties in reference to the digestion of sugar, starch, and fat, amply pointed out. Dr. Dalton shows very clearly that no secretion found in the intestinal canal possesses the property of emulsifying fat in so eminent a degree as the pancreatic juice. But, after Colin's experiments, in which the pancreas was extirpated from the horse and the fat still absorbed, we will have to give up the theory which ascribes the agency to this gland alone, and admit that the intestinal juice and the bile are also capable of effecting that minute division of oily matter necessary to its absorption.

Passing over the seventh chapter, which relates to absorption, and which is written in the usual clear and concise manner of the author, we come next to the eighth chapter, which treats of the *bile*. Here we have a number of original observations which are highly important, but which have already been laid before our readers in Dr. Dalton's paper published in a former number of this journal. The various tests for bile are mentioned, and the fallacies of those which depend upon oxidation of the colouring matter and consequent change of hue are indicated. Pettenkofer's test, the only reliable one we at present possess, is described in detail.

The variations in quantity and functions of the bile are next considered. From original experiments, Dr. Dalton makes it appear that the discharge of bile into the intestine is greatest immediately after eating and within the first hour. He also determined that bile, or, at least, a substance answering to its reactions, was constantly present in the small intestine, but could not be detected in the large intestine. These results agree perfectly with those we have obtained from similar experiments.

In common with most physiologists of the present day, Dr. Dalton does not ascribe a high digestive power to the bile, neither does he regard it as excrementitious. In fact, there is but little doubt that the bile has not completed its function when it is discharged into the intestine, for if, as determined by Bidder and Schmidt, it be diverted from the intestine by the creation of a biliary fistula, the animal perishes in a short time with all the symptoms of inanition. It cannot, therefore, be considered an excrement, and, though Dr. Dalton and others have failed to detect it in the blood, we agree with him and with Bidder and Schmidt in regarding its passage into this fluid in a decomposed state as certain.

The researches of Marcet,[1] published since Dr. Dalton's volume was given to the press, go far to show that the fat taken as food into the stomach is transformed into fat acids, and, but for the action of the bile, would be excreted as such with the feces. He invariably found that, after the ingestion of fat, the oleic and margaric acids were found in the stomach, and that, if the bile was prevented passing into the intestine, these acids were constantly found in a free state in the alvine dejection. As if, how-

[1] Medical Times and Gazette, August 28, 1858. Also Brown-Séquard's Journal de la Physiologie, October, 1858, p. 806.

ever to add to the confusion already existing, M. Brown-Séquard[1] publishes the result of an experiment performed by him several years since, from which it appears that fat was absorbed from the large intestines without having been acted upon either by the bile, pancreatic juice, or juice secreted by the glands of the small intestine.

In the ninth chapter, the *formation of sugar in the liver* is considered. Nothing is said of Sanson's experiments, which demonstrate the presence of glycogenic matter in the blood of the portal vein, in both herbivorous and carnivorous animals, and, in fact, such presence, so far, at least, as the latter are concerned, is denied by Dr. Dalton. A commission of the French Academy of Medicine, appointed to report upon Sanson's *Memoirs*, confirmed his results, so far as they related to the presence of glycogenic matter in the portal vein of the herbivora, but contradicted them as regarded the carnivora. In his investigations, Sanson made use of Bernard's method for the separation of the glycogenic matter by caustic potash, and also employed a method of his own, free from the objections urged against the use of the potash solution. By both methods, he demonstrated the existence of glycogenic matter in the portal vein of a dog, fed for several days on flesh alone. The experiments were frequently repeated, and with uniform results. In the report to which we have referred, the commission objects to the use of potash, on the ground that by its action on albuminoid bodies, a small quantity of a substance is sometimes produced, capable of being converted into sugar, in presence of sulphuric acid, and therefore the results of Sanson's experiments on dogs are set aside. However correct the commission may be as regards the action of the potash, nothing is adduced against the other process employed by Sanson, and we cannot, therefore, at present, invalidate his conclusion. Colin's experiments, which show that sugar is found in the chyle of both herbivorous and carnivorous animals, also go far in support of Sanson's views of the external origin of this substance. The views of Bernard in relation to the sugar-forming function of the liver are however adopted without reserve, although other experiments to which we might refer, instituted long before the publication of Dr. Dalton's volume, have shown indubitably that sugar clearly derived from the food will enter the blood, and even be excreted as a component constituent of the urine. Pavy's experiments[2] also demonstrate conclusively, in opposition to Bernard's theory, that sugar does not exist during life as a constituent of the liver, but is generated by fermentation after death, or removal of this viscus, from the glycogenic matter or hepatine existing in it. He has also shown that the quantity of hepatine varies according to the nature of the food. These researches had not, however, been published when Dr. Dalton's volume issued from the press.

Chapter tenth treats of the *spleen*. Nothing in addition to our present knowledge is stated relative to the functions of this organ. Dr. Dalton regards it as constituting, with the mesenteric and lymphatic glands, a system having a common function, connected with certain changes going on in the blood. We may add that, in all probability, the thyroid body and supra-renal capsules have also an identity of function with the spleen, as well as with those organs mentioned by Dr. Dalton.

The four following chapters relate to the blood, respiration, animal heat, and the circulation.

In the chapter relating to the *blood*, the crystallizable substance of this

[1] Op. cit., p. 808 (note).

[2] Guy's Hospital Reports, Oct., 1858.

fluid is again entirely overlooked. We do not find the slightest reference to it, which is certainly surprising in view of the great attention physiologists have recently given to the matter, and the importance which attaches to it on account of its medico-legal relations. There are several points in this chapter wherein we are constrained to differ with Dr. Dalton. Were we alone in this respect, we should feel a hesitancy in putting our opinion in opposition to that of so generally correct an observer as the author, but in the matters to which we refer we think we are sustained by the great majority of physiologists.

In speaking of the red blood-corpuscles, Dr. Dalton says :—

> "In structure, the blood-globules are homogeneous. They have been sometimes erroneously described as consisting of a closed vesicle, or cell-wall, containing in its cavity some fluid or semi-fluid substance, of a different character from that composing the wall of the vesicle itself. No such structure, however, is really to be seen in them. Each blood-globule consists of a mass of organized animal substance, perfectly or nearly homogeneous in appearance, and of the same colour, consistency, and composition throughout. In some of the lower animals (birds, reptiles, fish) it contains also a granular nucleus imbedded in the substance of the globule; but in no instance is there any distinction to be made out between an external cell-wall and an internal cavity."

We might quote from Todd and Bowman, Carpenter, Bennett, Milne-Edwards, Lehmann, Müller, Draper, and many other high authorities, against Dr. Dalton in the view which he takes of the histology of the red corpuscle, whilst, with the exception of Valentin, Donné, and Robin, we know of no recent observers holding the theory which he advocates. The existence of a cell-wall is not only logically inferred from the action of liquids upon the corpuscle through endosmosis, but may frequently actually be demonstrated. For instance, if water be added to the blood of certain fish, as the gold-fish (*Cyprinus auratus*), the red corpuscle will be seen to become spherical, swell up, and finally to burst, giving exit to the nucleus, and leaving nothing else visible under the microscope but a broken and shrivelled membrane. The same liberation of the nucleus follows the addition of urea and ether, but in these cases the membrane is dissolved. In frogs and birds, it is almost impossible, owing to its great density, to cause the rupture of the cell-wall of the red corpuscle, and consequently we find it exceedingly difficult to obtain blood-crystals (which are formed from the contents of the corpuscles) from the blood of these animals.

In relation to the white corpuscle, we are again forced to disagree with Dr. Dalton. He entirely denies the existence of a nucleus in this body, and regards the appearance of such after the addition of acetic acid as being due to the coagulating and disintegrating action of the acid upon the substance of the white corpuscle. Now, by carefully focussing the object-glass of a microscope, and by the aid of oblique light, the presence of a nucleus in the white corpuscle can be clearly established without the addition of acetic acid. Frequently, also, by the action of reagents, the white corpuscle can be made to burst and allow the nucleus to escape. The action of acetic acid is not altogether that of coagulation and disintegration. It is mainly by its dissolving effect upon the free granular matter of the corpuscle that we are enabled to distinguish the nucleus so readily after its addition to the blood.

In relation to the destruction of the red corpuscle, Dr. Dalton expresses the opinion that no such action takes place within the organism, but that, being once formed, the red corpuscle continues to exist whilst life remains in the individual. He says :—

* * * * "But there is no reason for believing that the red globules of the blood are any less permanent, as anatomical forms, than the muscular fibres or nervous filaments. They undergo, it is true, like all the constituent parts of the body, a constant interstitial metamorphosis. They absorb incessantly nutritious materials from the blood, and give up to the circulating fluid, at the same time, other substances, which result from their internal waste and disintegration. But they do not, so far as we know, perish bodily in any part of the circulation. It is not the *anatomical forms* anywhere which undergo destruction and renovation in the nutritive process; but only the *proximate principles of which they are composed.* The effect of this interstitial nutrition, therefore, in the blood-globules, as in the various solid tissues, is merely to maintain them in a natural and healthy condition of integrity."

When we call to mind that, in certain anemic conditions of the system, as chlorosis, scorbutus, &c., we not only obtain a less number of red corpuscles by chemical analysis, but can actually count them under the microscope, and, therefore, positively demonstrate their paucity, we are at a loss to conceive why Dr. Dalton should entertain the opinion that they are never destroyed. Every pathologist is aware of their deficiency in the diseases we have mentioned, as well by external indications as by chemical and microscopical examination. But aside from the evidence afforded by pathology, there are many strong reasons for believing that the red corpuscles undergo disintegration as a natural and healthy process.

From what we have said it will be perceived that we do not regard the chapter on the blood as fully representing the present state of our knowledge on the subject.

In the chapter on *respiration* this function is treated of in a systematic and perspicuous manner, and the views expressed are such as may be considered well established. The process is considered in the inferior orders of animals, and then detailed descriptions of the respiratory movements of the chest and glottis, and of the changes in the air and blood, are given.

In relation to *animal heat*, Dr. Dalton's opinions are those which are beginning to be held by all physiologists who are not adherents of a particular school. We quote the following passages as containing the conclusions arrived at in relation to this subject—conclusions in which we must express our entire concurrence.

"Animal heat, then, is a phenomenon which results from the simultaneous activity of many different processes, taking place in many different organs, and dependent, undoubtedly, on different chemical changes in each one. The introduction of oxygen and the exhalation of carbonic acid have no direct connection with each other, but are only the beginning and the end of a long series of continuous changes, in which all the tissues of the body successively take a part. Their relation is precisely that which exists between the food introduced through the stomach and the urinary ingredients eliminated by the kidneys. The tissues require for their nutrition a constant supply of solid and liquid food, which is introduced through the stomach, and of oxygen, which is introduced through the lungs. The disintegration and decomposition give rise, on the one hand, to urea, uric acid, &c., which are discharged with the urine, and, on the other hand, to carbonic acid, which is exhaled from the lungs. But the oxygen is not directly converted into carbonic acid, any more than the food is directly converted into urea and urates.

"Animal heat is not to be regarded, therefore, as the result of a combustive process. There is no reason for believing that the greater part of the food is 'burned' in the circulation. It is, on the contrary, assimilated by the substance of the tissues; and these, in their subsequent disintegration, give rise to several excretory products, one of which is carbonic acid.

"The numerous combinations and decompositions which follow each other

incessantly during the nutritive process, result in the production of an internal or vital heat, which is present in both animals and vegetables, and which varies in amount in different species, in the same individual at different times, and even in different parts and organs of the same body."

Nothing could be more lucid, concise, and at the same time more comprehensive than Dr. Dalton's description of the heart and circulation of the blood. The arterial, venous, and capillary circulations are considered under separate heads, and the general view presented is such as will convey to the student an adequate idea of the subject.

The fifteenth and sixteenth chapters treat of *secretion* and *excretion.* Every point of importance connected with these processes is touched upon, and the various products of secretion and excretion well described, with the very notable exception that no mention is made of the character, constituents, &c., of the feces, except incidentally when treating of the bile. Nor would we otherwise know from a perusal of the volume that any such thing as intestinal excrement was to be met with, as a consequence of the ingestion of food and deposit in the intestines of certain effete matters of the organism. Aside from the fact that some reference should have been made to this excretion, we think the results of the researches of Marcet, Wehsarg, Ihring, and others, sufficiently important to require that they should be brought within the immediate reach of the student of physiology. The subject to which we refer would, perhaps, have been more appropriately considered in an extension of the sixth chapter, which relates to the function of digestion. But having brought the food to the termination of the small intestine, it is left. Under no other head could the matter have been properly brought than the one we are now considering—excretion; but as we have seen, the author is here also silent upon the point, and we are forced to infer that the large intestine, and the very important part which it performs in the economy, have inadvertently escaped his notice.

These chapters finish the first section, and bring us to the middle of the book. With the exceptions to which we have referred, we think it well adapted, by its terseness, perspicuity, and general accuracy, to convey correct ideas of the great subject of nutrition. We should, perhaps, have preferred that some points considered should have been more fully dwelt upon; but we are aware that fulness of detail is in many cases properly sacrificed to conciseness and clearness.

The *nervous system* forms the subject considered in the second section. The first chapter of this section relates to the general structure and functions of the nervous system. The histology of the nerves first engages our attention. Then, in illustration of the progressive development of the nervous system, we have numerous examples from the lower animals, commencing with the radiata, through the mollusca and articulata, till the vertebrata, at the head of which stands man, are reached.

The second chapter relates to *nervous irritability* and its mode of action. All the principles enunciated in this chapter are carefully and accurately stated, with all the elaborateness necessary in a work constructed on the plan of that under our notice. The peculiarities of nervous action are clearly stated; and we presume the following remarks, which conclude this portion of the subject, will meet with the unqualified approval of a majority of the physiologists of the present day:—

"The nervous force, therefore, while it has some points of resemblance with electricity, presents also certain features of dissimilarity which are equally im-

portant. It must be regarded accordingly as distinct in its nature from other known physical forces, and as altogether peculiar to the nervous tissue in which it originates."

In chapter third the *spinal cord* is considered. The various important functions exercised by this organ are detailed with Dr. Dalton's accustomed perspicuity, and several experiments illustrative of these functions are given in detail. The ensuing paragraph, ending the chapter, expresses in a few words the varied character of the influence exercised by the spinal cord:—

"We find, therefore, that the spinal cord, in its character of a nervous centre, exerts a general protective action over the whole body. It presides over the involuntary movements of the limbs and trunk; it regulates the action of the sphincters, the rectum, and the bladder; while, at the same time, it exerts an indirect influence on the nutritive changes in those parts which it supplies with nerves."

In chapter fourth the various parts composing the *encephalon* are considered, and the peculiar office of each pointed out. In treating of the hemispheres, Dr. Dalton shows the direct relation existing between the size of the cerebrum and the degree of intellectual capacity. Nevertheless, there is, as he thinks, little doubt that other circumstances, in addition to size, may modify its functional activity. We quote the following passage in illustration of his views:—

* * * "The functional activity of the brain is modified, no doubt, by its texture as well as by its size; and an increased excitability may compensate partially or wholly for a deficiency in bulk. This fact is sometimes illustrated in the case of idiots. There are instances where idiotic children with small brains are less imbecile and helpless than others with a larger development, owing to a certain vivacity and impressibility of organization which take the place, to a certain extent, of the purely intellectual faculties."

The cases of the two dwarfed and idiotic Central American children, exhibited a few years since under the name of the Aztec children, are adduced in support of this view. These children, as many of our readers will doubtless recollect, were possessed of exceedingly small heads, and yet were extremely excitable and vivacious; and though incapable of continuous articulation, readily understood gestures, and knew the meaning of the different tones of the voice.

Chapter fifth relates to the *cranial nerves*. Here, although we have descriptions of the nerves, their origins, courses, &c., Dr. Dalton has entirely omitted to describe the arrangements by which the nerves of special sense receive impressions made upon them. In fact, we have nothing in relation to the physiology of the special senses, but only anatomical descriptions of the nerves themselves.

Thus, in relation to the sense of smell, we are told that the olfactory nerves pass through the cribriform plate of the ethmoid bone, and are distributed to the Schneiderian membranes; and then we are informed that these nerves convey the special sensation of smell. Nothing is stated in regard to the minute division of matter necessary in order that it may be conveyed to the termination of the nerves in question; nothing as to the way in which smelling is performed, and nothing in relation to the construction of the olfactory apparatus.

And so with the sense of sight. The whole theory of vision is entirely unnoticed, and no mention whatever is made of the eye.

The philosophy of sound, the physiology of audition, and the construction of the auditory apparatus, are also entirely ignored.

The physiology of taste, and the peculiar manner by which this sensation is received by the tongue, are likewise passed over in silence; and, finally, we have no reference whatever to the sense of touch.

We cannot avoid expressing the opinion that these deficiencies impair the completeness of Dr. Dalton's treatise as a text-book for students. All that he has stated in reference to the senses—those agencies by which man is placed in communion with the external world—can be found in any text-book of anatomy; and if the student does not find them described and explained in his physiological text-book, he will not be likely to have them elucidated in any other work ordinarily recommended for his study.

In the sixth chapter the *system of the great sympathetic* is considered. This chapter concludes the section devoted to the nervous system.

We hope that in a future edition Dr. Dalton will add to the usefulness of his volume, by supplying the deficiencies in this section which we have felt it our duty to notice. To do this would not add very materially to the bulk of the work, whilst it would greatly improve it both as a volume for reference and as a text-book.

The third section is devoted to the consideration of the subject of reproduction.

In the first chapter the *nature of reproduction* and the origin of plants and animals are considered. Prof. Dalton argues strongly against the theory of spontaneous generation—a theory, however, which cannot be said to receive the countenance of biologists of the present day. Many interesting facts in connection with infusorial and parasitic organisms are detailed, and a summary of the very important results of Siebold's and Küchenmeister's investigations with reference to certain entozoa is given.

In the second chapter *sexual generation*, as it takes place in plants and the different classes of animals, is briefly considered; and in the third chapter the *egg and the female organs of generation* are brought under notice.

Next the *spermatic fluid and male generative organs* are described. In chapter fifth *periodical ovulation* and the function of menstruation claim attention; and chapter sixth treats of the *corpus luteum* of menstruation and pregnancy.

These portions of the work are really models of their kind, both in style and matter, and are marked by so much originality as to convey a high idea of Dr. Dalton's ability as an observer. The remarks in relation to the corpus luteum—as might have been expected from Dr. Dalton's known interest in the subject—are full and to the point. As our readers are aware, he has by his investigations materially added to our means of distinguishing the corpus luteum of menstruation from that of pregnancy, and his views have met with the general acceptance of physiologists.

Chapters seventh to eighteenth treat of the *development of the egg* and appendages, of the whole process of *utero-gestation*, of the *development of the several parts and organs of the embryo*, and, finally, of *development after birth*.

In treating of the formation of the chorion, we have a much clearer, more exact, and more modern view presented, than that usually given in British and American text-books. The opinion of Valentin[1] and Von Baer[2], that this membrane is produced from the albuminous material which the ovum receives as a coating, during its passage through the Fallopian tube, is, we

[1] Handbuch der Entwickelungsgeschichte des Menschen, &c., pp. 38, 39.

[2] Beobachtungen und Reflexionen über Entwickelungsgeschichte, &c., 2d Abtheil, p. 185.

think, very properly—in accordance with the results of more thorough investigation—discarded, and the more philosophical view of Coste[1] adopted. According to this investigator, there are three chorions; the *first* is the vitelline membrane, which, after the ovum has entered the uterus, becomes covered with vegetations, and establishes the first relation of the ovum with the matter surrounding it. The *second* chorion is the external layer of the blastodermic membrane, which, being pushed back against the vitelline membrane, causes the atrophy and disappearance of the latter. The *third* chorion is formed from the allantois, which, being applied to the whole surface of the ovum, becomes covered with villosities, and causes the atrophy of the membrane surrounding it. This chorion is highly vascular, and is persistent to the end of gestation.

This theory of the formation of the chorion is that adopted by Robin, Longet, and other continental physiologists; and we are glad to see that it has become incorporated into a text-book of physiology in the English language.

The albuminous covering which the ovum receives in its transit through the Fallopian tube, probably serves a purpose as important as that ascribed to it by the physiologists we have mentioned, and that is, the nutrition of the ovum, prior to its arrival in the uterus. This view is sustained by Longet,[2] and derives additional support from the fact, also mentioned by this author, that the albuminous material in question entirely disappears before the ovum reaches the cavity of the uterus.

We can do no more than express our high appreciation of the manner in which Dr. Dalton has treated these portions of his subject. In fact, in reference to the whole of the third section, we should do violence to our own feelings were we to withhold our meed of praise for the truly admirable manner in which it is conceived and brought to its termination. As an essay on reproduction we know of nothing which excels it, and we regret that the length to which we have already extended this review forbids our noticing it more in detail.

In our remarks upon Dr. Dalton's volume we have endeavoured to render justice to its merits, and at the same time point out what we consider to be its deficiencies. Its great recommendation consists in its containing, to a great extent, the results of the author's own observations. There is, therefore, a freshness about it which those accustomed to refer to books made up entirely from the labours of others can scarcely fail to appreciate. Experimental results from frequent citation become hackneyed, and if we would preserve their value it is necessary that an observer like Dr. Dalton should now and then work the ground over again.

Conciseness and exactitude of expression are also prominent features in Dr. Dalton's treatise. No one can be at a loss to understand his meaning. There is never a word too much or too little; but what it is necessary to say, is expressed in as clear a manner as the English language allows.

We have as yet said nothing in regard to the illustrations, and the manner in which the publishers have performed their part. There cannot, we think, be two opinions on these points. The illustrations are so admirable, the type so bold and clear, the paper so excellent, that no one with the slightest perception of the elements necessary to make an elegant volume, can fail to recognize them in the work before us. Of the two hundred and

[1] Histoire Gén. et part. du Développ. des Corps Organisés, p. 82.
[2] Traité de Physiologie, tome 2d, de la Génération, p. 148.

fifty-four cuts, all are original but eleven. Besides being well executed, they are, with scarcely an exception, true to nature; and the diagrams are so clearly and faithfully drawn, as to aid materially in the elucidation of the subjects to which they refer.

As to what we consider to be the deficiencies of the volume, we trust we have said nothing uncalled for, or calculated to convey an erroneous opinion of its character. As our readers have perceived, the defects are chiefly those of omission, and such as the author can with a little labour—irksome, perhaps, to an original mind—rectify without doing violence to his opinions. When this is done, we will have a text-book, not only useful as a work of reference, but one that can be conscientiously recommended as complete to the student.

To physicians, Dr. Dalton's treatise, even in its present form, can scarcely fail to be of service. Those who have in a measure kept pace with the advance of physiology, will find an epitome of its progress, and, perhaps, also some things with which they were not previously acquainted; whilst those who have not studied it since their pupilage, will find a mine of information, capable of being turned to practical advantage. To both these classes we recommend the work in question, confident that they will rise from its perusal with an exalted opinion of Dr. Dalton's literary and scientific abilities. W. A. H.

ART. XIV.—*Report on the Vital Statistics of the United States, made to the Mutual Insurance Company of New York.* By JAMES WYNNE, M. D., &c. &c. &c. New York: H. Bailliere, 1857. 4to. pp. 214.

THIS is a work prepared, as stated in the preface, for the convenience and advantage of life insurance companies. It is intended to gather all the recorded facts in respect to life and mortality, and to show the progress of population, the conditions of vitality, and the circumstances that increase or diminish it.

Dr. Wynne, with good reason, begins with complaining of the want of facts of this nature. He is not alone in his sorrow. Every investigator of this subject, from the beginning until now, has felt the same want, and mourned the deficiency of records in respect to human life and death, the circumstances that surround them, and the influences that bear upon them.

This subject, that should be the nearest to men's hearts, and familiar to all, which should demand the attention of legislators and statesmen more than all others, has yet attracted but little notice from the public, and is looked upon as hardly more than a mere speculation, fit for dreamy philanthropists rather than a matter worthy of the consideration of practical men. While other things of inferior importance to man have been watched, studied, and guarded, with careful vigilance, this has found few to care for it, and lend it an earnest encouragement.

The records of estates, the registries of lands, houses, barns, &c., are well provided for in almost every civilized country, and no real estate is transferred without the recognition of public authority; yet but few nations or states have established any law for the registration of the great events of humanity—birth, marriage, and death—within their borders. This is done by some of the nations of Europe, and a few of the States of our

Union. England has published nineteen large annual volumes containing the record of life and mortality for the year, and rich with mathematical calculation and philosophical deduction in respect to vitality and the circumstances that affect it for good or for evil. Scotland has published quarterly reports for four years. In this country, Massachusetts took the lead in 1842, and has sent forth sixteen annual reports of the births, marriages, and deaths. Rhode Island has published five, and Kentucky six reports. All these are large and full of matter of great value to the world, and especially to the student of vital statistics. Connecticut has published nine annual reports; New Jersey eight; Virginia one, including the facts of two years; South Carolina four; and New York two. All these are smaller than those of the three States first mentioned, and yet they contain facts of great importance to be known. Some other States are following in the track of these, and, we trust, will in due time make full revelations of life, its progress and its dangers, among their people. Vermont has already established a very satisfactory law for this purpose. Under its operation, the returns have been made to the government, and the facts are now in competent hands to digest them and make the requisite report. California has established a similar law, and we may now look for its first fruits.

It is deeply to be regretted, that some other States have begun this work, but have faltered and gone back. New York published reports for 1847 and 1848, but since that time its law has slept the sleep of death, and no further cognizance has been taken of it by the government or people. That so intelligent and enterprising a community, as that of the Empire State, should stop in such a work as this is a matter of wonder, at least to all who know their great and liberal doings in other things, and is a deep disappointment to the friends of this science of humanity everywhere. It is to be hoped, however, that the law is only resting temporarily, to rise again in a better form, and enter upon a noble and vigorous course through all coming years.

Pennsylvania once had a law, which not being satisfactory to those who were most interested in it or affected by it, was strangled almost at its birth, and of course produced no fruit. It is gratifying to be able to state, that the friends of this matter there have it still at heart, and hope, ere long, to procure the adoption of a law that will be acceptable to all, and produce its proper results.

New Hampshire a few years ago passed a law, but it was stillborn. It had no life, and no regard was paid to it by either rulers or people.

Several cities, Charleston, Washington, Baltimore, Philadelphia, Brooklyn, New York, Buffalo, Providence, Boston, and Lowell, and for ought we know, others, for a varied length of years, and some of them for a very long period, have published the annual reports of their mortality.

We have also occasional reports of the mortality of New Orleans, Memphis, Tenn., Troy, and Rochester, N. Y., and of some others, published for special purposes, on account of some epidemic or other cause that made it important that their sanitary condition should then be investigated and made known.

The mortality statistics of the United States for the year ending June 1st, 1850, published in connection with the last census, embraces the deaths, within that year, in the several States. This is the first attempt of our National Government to inquire into and to show the sanitary condition of the people; but we trust it is but the first of a series of decennial volumes of this nature, to be issued with the reports of every future census.

This is not a perfect work, nor does it pretend to be; but it is as complete as the facts that were gathered would allow, and these were probably as complete as the nature and character of the law that required them, and the means provided for their collection would permit. The facts were gathered by the marshals and their deputies, who were not presumed to be familiar with vital statistics, and the causes of death, and upon whom, for the first time, this new burden was placed, in addition to the usual and ordinary duties of their special office. Moreover, Congress unfortunately, without considering the nature and bearing of this work, the amount of the facts that were gathered, and their importance to humanity, ordered the superintendent of census to digest the facts, and print them in a volume of four hundred pages, and granted only a limited sum for this purpose. The superintendent of census, Mr. Dé Bow, therefore, was compelled to omit much that should have been spread before the world, and to curtail many others that would have been worth far more than their cost to the nation, if they could have been expanded to their due extent.

Although abbreviated and reduced, it is yet a work of great value, for it contains the record of a class of facts the like of which has not been given to the public to read in this country.

No inquiry was made by the marshals for the births, and no returns of them were made to the central office. The marriages and deaths were not all ascertained and reported. Some whole counties were entirely omitted in respect to these events, and in many others the inquiry was but partial, and the returns imperfect.

The incompleteness of the reports of these great facts in the census, is acknowledged by the superintendent, and it is shown by Dr. Wynne in the volume now under review. But the subsidiary facts that are stated: the causes of death, the age and sex of those who died, the comparative prevalence of the several fatal diseases, as to each other, and the seasons in which they occurred, these are reliable and important, and may be turned to good account in the investigations of life and mortality.

Dr. Wynne first describes the geographical territory of the United States, showing its physical condition, and points out the various sanitary and morbific influences. He gives a good account of the population, and their distribution as to age and locality in the several States, and of the widely different characteristics of the people. The increase of inhabitants from natural causes and from immigration, and the probable future of the people of this country, are ably discussed and clearly set forth.

Dr. Wynne indorses the opinion of Prof. Tucker, "that the increase of population is inversely as the density in the States." This is a general truth, especially when applied to agricultural States. But there are many circumstances and conditions beside unoccupied land that affect the growth of population. When the inhabitants of a country, exclusively agricultural, reach a certain density, and when all the land, that can be profitably cultivated, is so appropriated, then the surplus population go elsewhere for occupation and support, and growth is suspended. But if any other employments, as manufacturing, commerce, &c., are introduced, or if new methods of agriculture are adopted, and more labour can be expended upon the soil with advantage, and larger crops are obtained, then a new element is at work, more people are wanted, and the natives are retained at home, and if these are not sufficient to meet the demand for labour, others are brought in from abroad in proportion to the increase of the means of occupation. Thus the growth of population continues with various degrees of rapidity.

This is shown in the comparative increase of the population of Massachusetts and North Carolina, and some other States, as in the following table:—

	Population to sq. mile, 1840.	Rate of increase, 1840 to 1850.		Population to sq. mile, 1840.	Rate of increase, 1840 to 1850.
Massachusetts	94.58	34.81 pr. ct.	North Carolina	14.80	15.35 pr. ct.
New York	51.68	27.52 "	South Carolina	20.23	12.47 "
New Jersey	44.87	31.64 "	Virginia	20.21	14.67 "
Pennsylvania	37.48	34.09 "	Kentucky	20.76	25.98 "

The same is shown in different counties of Massachusetts. The counties of Berkshire, Franklin, and Hampshire had forty-nine inhabitants to the square mile in 1840. These were devoted mainly to agriculture. The counties of Essex and Middlesex had one hundred and ninety-four to the square mile, and these largely engaged in manufactures and commerce. During the ten years from 1840 to 1850, the former gained 15 per cent., while the latter gained 45 per cent. in population.

The difference in the rate of growth, in the same State, at different periods, is shown in the progress of Massachusetts in the two decades following 1830. In the first—1830 to 1840—starting with 78 to the mile, there was an increase of 20 per cent. But in the next, beginning with 94.58 to the mile, there was an increase of 34 per cent. This was due to the great increase of manufactures and trade and improvements in agriculture, which gave employment to a greater number of people.

If there were neither immigration nor emigration, if none come into the State except by birth, and none go out except by death, the increase or decrease of population is precisely the difference between these two classes of facts; and as, almost universally, the births exceed the deaths, there is a proportionate increase of population from that cause.

It is rare that any State, town, or district, however small, is unaffected by migration outwards or inwards. Almost everywhere, one or the other of these events is going on, and, most commonly, both. This acts as a disturbing element in all the calculations of the progress of population founded solely on the operation of natural causes. This difficulty would be obviated, if it were possible to determine, not only the number, but also the sex and ages of both the incomers and the outgoers.

The recent immigration from Europe, and especially from Germany and Ireland, into the United States, is greater than any movement of population from country to country within the reach of history. In 1850, there were in this Union 2,210,839, who were natives of other lands. Of these, 961,719 were born in Ireland, 379,093 in Great Britain, 147,711 in the British Provinces; making 67 per cent. from the British dominions. 618,299 were born in Germany, Prussia, and the Low Countries; being 27.9 per cent. of the Teutonic race. 20,299 were from Sweden, Norway, Denmark, and Russia; and 61,101 from France and the South of Europe.

These foreigners are not divided between the sexes, nor distributed among the several ages, in the same proportions as the people either of the country from which they came, or of that in which they now live. Those who leave their homes, and try their fortunes in new regions, are mostly males, and in youth and early manhood. The national reports of immigration found in the annual executive documents, at first gave only the numbers of

passengers from abroad in gross; but in later years they have distinguished the sexes, and the ages in quinquennial periods up to forty. These reports, for several years, are rendered almost useless by the want of summaries of the facts. The numbers of immigrants of each sex, and in each quinquennial period of life, are stated separately for each port of entry and each quarter of the year. Every reader, therefore, before he can use these facts, is obliged to add up these manifold items, running through thirty to fifty pages of the national report. In the last six years, this work is done by the Department of State, and the facts are easily understood and applied.

We have obtained these results for the thirty-seven and a quarter years ending with December, 1856, during which 4,364,652 alien passengers arrived by sea from foreign countries, whose sexes and ages are specified. 2,600,926 of these were males, and 1,763,726 females; being in the proportion of 147 males to 100 females, or an excess of 47 per cent. of the stronger sex. The native white population of the United States were in the proportion of 1029 males to 1000 females, or an excess of almost 3 per cent. of the former in 1850. Of course, the excess of males in the United States is increased by this large disproportion of that sex among the foreign immigrants. In Europe, there is an excess of females, being in Great Britain, Ireland, France and Germany about 3.5 per cent., and in Belgium, 6 per cent. We have no means of explaining this great difference of the distribution of the sexes in Europe and among the natives of this country.

The proportions of the sexes are far from being the same in all parts of this country. In New England, there is a slight excess—about 1 per cent.—of females; but in all the other sections the other sex predominates. In the new States, beyond the Alleghanies, there are about 8 per cent. more males, and in the territories this excess amounts to 172 per cent. This large proportion of males in the new States and the territories, is unquestionably due to immigration, which brings more of that sex than of the other.

No distinction is made, in the census of 1850, between the natives and foreigners in the United States, when classified according to ages. We have, therefore, no direct means of comparing them in this respect. But as, among the immigrants, there is a smaller proportion in the early and late periods, and, of course, a larger proportion in the middle period of life, than is found among the whole body of white population here, it necessarily follows that this disproportion of maturity above childhood and youth must increase with the lapse of time after their arrival here.

We find in the census of Ireland for 1851, the record of the ages of all who left the island, intending not to return, in the four years and nine months ending with 1855. With this, and with the custom-house records of all the immigrants into the United States through thirty-seven years and three months, from October 1, 1819, and with the facts of the seventh census, we are able to compare the ages of the new and the fixed population of this country.

Ratio per cent. in several Periods of Life.

Period of life.	White population in United States, 1850.	Emigrants from Ireland to all countries.	Immigrants in United States from all countries.
Under 10 years	28.64	12.90	15.49
10 to 20	23.17	25.97	20.81
20 to 40	30.91	50.93	53.33
Over 40	17.23	10.20	10.35

The great body of the immigrants are of the poor and labouring classes. There are a few adventurers, a few refugees from governments unsuited to their ideas, and some fugitives from justice. Some came to escape from distressing poverty, and very few from starvation. But the great majority were simply poor. They left their native lands to amend their low condition. Some merely hoped to get a better living; some hoped to gain a competence in a land of plenty, and a few came with the expectation of making their fortunes in this land of promise.

In 1850, the greater part—65 per cent.—of the foreigners, were in the old States, and only 35 per cent. in the new States and territories of the west. They prefer the regions which are the most densely peopled. In New York, New Jersey, Pennsylvania, Connecticut, Rhode Island, and Massachusetts —which had fifty to one hundred and twenty-seven inhabitants to the square mile—there were 1,224,000 foreigners; and in Michigan, Wisconsin, Iowa, California, and the territories—which had less than eight to the square mile—there were only 210,000. The Irish and Germans are especially disposed to congregate and live in masses, where they find their own countrymen, and in crowded places, where much business is carried on. They fear that they may not be able to obtain employment in the sparsely settled country, and prefer the more certain chance of obtaining it among a denser population. Consequently, only about a tenth of the Irish, and a little more than a quarter of the Germans, were in the new States of the northwest, and in California. Moreover, they seem to have a strong dread of the country even of the old States; about two-fifths of those foreigners were in the large cities, and no small part of the others in the smaller cities and the villages throughout the land.

This is a remarkable fact, seeing that the Irish certainly, and probably the Germans, lived in the country at home, and were accustomed to its employments only, and unused to the labour and habits of cities. According to the last census of Ireland there was a decrease of 1,695,950 in the rural districts, and an increase of 83,211 in the civic districts, between the years 1841 and 1851. In 1841 there were 66.1 per cent. of the total population engaged in agricultural pursuits, but in 1851 these constituted only 52.6 per cent. of the whole, showing a diminution of 13.5 per cent. in ten years. The proportion of those otherwise occupied increased in that time. In the four years and nine months ending with 1855, 288,191 males, and 292,092 females, over 15 years old, left Ireland. Of these 232,874, or 80 per cent. of the males, and 121,682, or 41 per cent. of the females, were farmers or farm labourers. From June 1, 1841, to December 31, 1855, 2,087,856 left Ireland, of whom 1,600,753 were destined for the United States.

The Irish immigrants, at least, are mostly of the poorer and the labouring classes. In 1841, 62.9 per cent. of the people of Ireland were dependent on their own manual, unskilled labour for support. In the ten next succeeding years these had decreased to 52 per cent. of all. The operation of the encumbered estates law consolidated a great many large farms, which had been divided and subdivided. These had been let to middle-men, and sub-let, in mere patches of an acre or more, to labourers, and gave a meagre subsistence to the most miserable of the peasantry. By this consolidation of estates, their houses were taken from them, and their cabins were destroyed, and they were compelled to find shelter and employment elsewhere. There was, therefore, a decrease of 355,689 of the fourth-class houses, mere "mud cabins, having only one room," within the ten years, 1841 to 1851,

being a loss of 72 per cent. of houses of the lowest class; while, on the other hand, there was an increase of 73,073 houses of the first, second, and third classes, or 8.7 per cent of the whole. The families which occupied the fourth-class houses were 35.1 per cent. of the whole nation in 1841, and only 12.2 per cent. ten years later.

Taking all of these independent facts together—that the great loss of population of Ireland, from 1841 to 1851, was of the agricultural class—that the great majority of those who left, in 1841 to 1851, were farmers and farm labourers—that the inhabitants of the rural districts only decreased—that nearly three-quarters of the poorest houses were destroyed—that there was a great decrease of those who depended on their manual labour for sustenance—and that 1,600,753, or four-fifths of all the emigrants from Ireland in sixteen years, 1841 to 1856, left with the intention of going to the United States—it is extremely probable, that the great majority of the Irish immigrants were poor, that they were occupied in cultivating the earth, and lived in the country districts, before they left their native land.

We have no means of knowing the original residence or social and pecuniary condition of the immigrants that come from other countries. But it is probable, that they were of a class similar to the Irish. This is corroborated by the immigration reports, which state that the largest part of all who arrive from abroad are farmers and labourers.

It would be naturally expected, that these foreigners would, immediately on their arrival, go to the country and seek such employment there as they were accustomed to at home. They seem to be the very men to go into the wilderness, to take up and break up the new lands, and convert them into farms for themselves, and find and establish homes of their own. Especially those whose whole capital is their physical strength and their capacity of labour, should do this, for it may be accomplished without cost. The national domain is open to whomsoever will take possession of and cultivate it, and no return is demanded until the government orders the sale, which may not be until the occupant shall have tilled it for some years, and earned sufficient to pay the small price that is required. Thus, in course of years, they could secure not only homes and subsistence, but a competence and even wealth for themselves and their families.

This, however, is done by but few except the Swedes and Norwegians, most of whom become independent farmers in the new States. But the Irish are not made for pioneers. Whatever may be their physical force, they have not the moral qualities of energy, enterprise, and self-reliance necessary to carry them beyond the pale of society, nor the requisite force to subdue the unbroken earth, and establish civilization in the savage regions. Their social nature and their want of courage keep them in the midst of numbers. They prefer, therefore, to occupy the subordinate positions of serving others, who will give them employment, superintendence and wages, in the cities and villages, to the solitary and self-directing independence of farming on their own account in the new territories. A large proportion of the Germans seem to have the same aversion to the unbroken wilderness, or even the country anywhere, and are to be found in the cities, towns, and densely peopled States.

Beside the disturbance in the law of population by the introduction of the foreign element into the United States, the natives are said to be very migratory, and move easily and frequently from State to State. There are no adults in the newest States that were born within their borders, and a part of all the people of every State were born elsewhere in the Union. In Iowa

and Wisconsin 70 per cent., in Illinois 53 per cent., in Connecticut 12 per cent., and in North Carolina 3.7 per cent., of their American population were born in other States; and 23.18 per cent. of all the natives of the country were living in States in which they were not born. This is supposed to be sufficient to justify the opinion that we are a restless people, without local attachments, and willing to change our homes for smaller motives, and more frequently, than the inhabitants of other countries. Yet this is not true in comparison with England and Wales, where in 1851, according to the census of that year, 24.87 per cent. of all the population were living in counties where they were not born. The Irish, although so ready to leave their country, are yet more fixed while they remain at home, for in 1841, only 4.96 per cent., and in 1851, only 8.92 per cent. of all were residing out of the counties which gave them birth. We have no means of knowing the condition of the people of the continental nations, but presume, that they are more fixed in their native homes than the Americans or the English.

The population is younger in the United States than in England and the middle and northern nations of Europe. A new people, or one that is growing, by immigration or natural increase, has a larger proportion of the young and fewer of the aged than one that is old or stationary.

Average Age of the Population.

Scotland, 1841	27.73 years.	Prussia, 1838	27.25 years.
England, 1841	25.41 "	United States, 1840	22.71 "
" 1851	26.40 "	" 1850	22.89 "

Similar differences, varying with the progressive nature of the population, exist between the old and the new States of this country. Emigration takes away the young and middle aged from their native districts, and leaves the more advanced in age behind. It carries to the new States the young and middle aged, who, at first alone, and afterward with their children, constitute the whole population. In New Hampshire and Vermont 32 and 35 per cent., and in Iowa and Minnesota 20 and 24 per cent., of all the people are over 60 years old. For every 1000 children under 15, there are over 60, in New Hampshire, 768; in Illinois, 73; in North Carolina, 153; and in Mississippi, 74.

The available and real power or effective force of any people depends on the proportion between the productive and the unproductive—the sustaining and the dependent classes. It may be assumed as a general law, that between 15 and 60 men and women are able to support themselves, and that before and after that period, they must depend on capital already accumulated, or now creating by others. That is, the labour of the forty-five years, from 15 to 60, must find bread and sustenance not only for that period, but for the preceding childhood and the subsequent old age. It follows that in any community, which has a larger proportion of children and of the old, the dependent bears more heavily upon the sustaining class. This varies very materially in different countries and States.

For every 1000 between 15 and 60, there were older and younger—

In Great Britain	729	United States	842
In France	598	Arkansas, Illinois, Michigan, Missouri	945
		New Hampshire, Connecticut, Vermont, North Carolina	816

In Arkansas, Illinois, Michigan, and Missouri, which in ten years, 1841 to 1851, had gained 494 per cent., the dependent were to the sustaining class as 945 to 1000. But in Connecticut, New Hampshire, North Carolina, and Vermont, which had gained only 18 per cent., these proportions were as 816 to 1000.

No inquiry was made by the marshals, who enumerated the people of the United States in 1850, as to the births, and of course they made no report of these facts to the government. The statement of the births in the volumes of the seventh census include, not the actual number of those who were born in the year, but only those who were alive and under one year old, on the 1st of June, 1850. By a singular assumption of legislative power, Congress ordered that these should be taken as the births during the year ending May 31, 1850. As this period, from birth to the end of the twelfth month, is the most fatal of human existence, except that of extreme old age, it is manifest that this number must fall far short of that which the law requires it to represent. Having no means of determining the ratio of mortality, at this age, in the several States, the error cannot be corrected, nor even an approximation made to the truth in this matter, except in regard to Massachusetts.

According to the National Census (Compendium, page 111), there were 23,805; but according to the State Registries, there were 25,651 born in Massachusetts in the year. Even the State returns fall short of the truth, for a few towns made no report, and others made reports that were evidently incomplete. The census gives 2.33 per cent. as the rate of births to the population of Massachusetts, and the State reports show, that, through the seven years, 1850 to 1856, the average rate was at least 2.9 per cent. The ratio of children under one year, by the law called the births, given in the census, varied from .29 per cent. in California, to 3.80 per cent. in Utah. Omitting these, which, from their peculiar arrangement of society, cannot be considered as representatives of others, the proportion ranges from 1.92 in New Hampshire, to 3.41 in Wisconsin, and that of the whole nation was 2.75 per cent. In all the new States of the northwest the ratios were over, and in all the States east of the Alleghanies, they were under 3 per cent., and in New England none exceeded 2.45. The proportion of births depends upon the proportion of females of the productive age in the population; as the last is not a constant fact, the other will vary accordingly.

Ratio of Births to Average Annual Population.

COUNTRY.	PERIOD.	ONE BIRTH TO—	
		Females, 20 to 40 yrs.	Total population.
England and Wales	1841-1851	4.4	30.7
France	1841-1856	5.7	36.6
Massachusetts, native . . .	1849-1856	9.2	51.5
" foreign . . .	1849-1856	3.9	17.7

Upon either basis of calculation, the ratio is larger in England than in France, larger in France than among the Americans in Massachusetts, and very much larger among the foreigners in that State than among the natives. Complete returns in Massachusetts would increase the ratio in native families, but probably not to the extent of England or France. The great

excess in foreign families in Massachusetts is due in part to the excess of females of the productive age, and in part to their more frequent marriage.

The females of the marriageable and productive age, 20 to 40 years, constituted 18.52 per cent. of all the immigrants in thirty-seven and a quarter years. They were 14.7 per cent. of all the whites in the United States, and 18 per cent. of those in Massachusetts, including the foreigners, in 1850.

In Massachusetts the Irish constitute 72 per cent. of all the aliens. They seem to be much more disposed to marry than the natives. In the three years 1854 to 1856, the marriages were one in 114.6 of the Americans, and one in 48.5 of the foreigners of all ages. They were one in 35.5 of those who were from 20 to 40 years old among the natives, and one in 25.1 of the same among the aliens.

The excess of births, in the Irish families, is not to be taken as evidence of a corresponding increase of their race in this country, for their infant mortality is also in a disproportionate excess; and it is yet to be seen, whether they will add as much to the effective adult population as the natives whose marriages and births are both in a lower ratio. So far as the records of Boston afford any data, they show that 18 per cent. of the children who were born of American parents, and 22 per cent. of those born of foreign parents, in 1856 and 1857, died within those years. This shows an infant mortality 22 per cent. greater among the strangers.

The connection of the states of prosperity and adversity with marriage and production, is discussed with ability and learning by Dr. Wynne. Unquestionably this is true as a general principle. In most civilized nations, men and women consider their power and means to sustain any new responsibility, before they assume it. Marriage is a new and fixed obligation. It necessarily involves a large increase of expenditure, with no prospect of diminution, but rather of further increase, through many coming years. When fortune favours, this burden is usually borne without difficulty; but when fortune frowns, there is trouble and sometimes distress. Men, therefore, marry more readily in times of prosperity, than when external circumstances are less promising. Thus, the labourer, when work is abundant and well rewarded, the man of business, when trade is prosperous, the farmer, when lands are easily obtained, and others when their plans of life are encouraging, engage in matrimony without fear; but when trade is dull and labour not easily obtained, or when crops are small and provisions not easily procured, or when land is costly or unproductive, they find reasons for delay of marriage and the assumption of any new burden.

The history of matrimony in some nations, whose records of marriage and of the corn exchange, of trade and finance, have been kept and published, show that these keep pace with each other; that while business is brisk marriage is frequent, and the contrary. These facts are shown by Milne, in his work on annuities, and in several of the reports of the Registrar General of England. The readiness with which employment can be obtained here, the high price of labour and the low price of provisions, induce the Irish in this country to marry at an earlier age than the more cautious and provident natives, or even their own countrymen at home.

The census reports give one birth in every 36.3 white inhabitants in 1850. Dr. Wynne very reasonably supposes, that this is much short of the fact, and that it even exceeds one in 35. The registration reports of Massachusetts show, that in the period from 1849 to 1856, inclusive, the births were one in 51.5 of the native, and one in 17.7 of the foreign population.

It should be considered, in connection with this last statement, that the natives include not only the native families, but also the children of the foreigners born in this country. If we could ascertain the numbers of the latter, and include them with the foreign class, there would be less difference, but yet an excess of the births in alien families. The experience of Boston throws some light on this matter. The average annual births in American families were one in 42.6, and in foreign families one in 20.6 of the average population of native and foreign origin, through the nine years, 1849 to 1857.

Rate of Birth to Population.

Country.	Period.	One birth to	Country.	Period.	One birth to
Austria	1834–37–40	29.0	Saxony	1832–1841	26.7
Bavaria	1836–1839	28.9	Sweden	1805–1835	31.0
England and Wales	1838–1855	30.47	Wirtemberg . . .	1833–1842	23.2
France	1836–1850	36.13	Massachusetts, native	1849–1856	51.5
Hanover	1832–1841	31.4	" foreign	1849–1856	17.7
Prussia	1839–1841	25.0	Boston, native . .	1849–1857	42.6
Russia	1852	22.4	" foreign . .	1849–1857	20.6

Baudin, in his valuable work on *Geographie et de Statistique Médicales*, adds the following to the above—

Baden, one birth in . . .	25.7	Naples, one birth in . .	27.3
Belgium, " " . . .	32.9	Portugal, " " . . .	29.1
Denmark, " " . . .	31.21	Sardinia, " " . . .	31.9
Holland, " " . . .	28.4	Switzerland, " " . . .	32.7
Holstein & Schleswig, one birth in	30.68		

Europe, average of twenty States and countries, one birth in . . . 29.09

These proportions are not constant in any country. "They varied in France, from one in 35.6, in the period from 1840–45, to one in 36.7, between 1845 and 1849. In Prussia, it diminished from 1834 to 1846, and rose again in 1849. A small increase, with various oscillations, is manifested in Hanover, Bavaria, Denmark, and Baden, and a decrease in Wirtemberg and Holstein. In Holland there was a decrease of 10 per cent. from 1840 to 1849; accompanied, however, with a similar diminution of marriages."

There seems to be a universal law that more males than females are born. As far as any records can be obtained in various nations, this is shown to be the fact. The excess of males varies with different people, and so far as known, this difference is the greatest in the United States.

How far this excess of males is due to, or influenced by, appreciable causes, is a question not settled, though one of deep interest. Here are very wide differences of excess. In South Carolina and Kentucky it is more than three times as great as in Massachusetts. It is about twice as large in Russia as in Sweden and England. It is forty per cent. greater in Austria than it is in Prussia. Comparing New England with Kentucky and South Carolina, we might suppose that the warm climate was more favourable to the male conceptions than the cold. But the excess in Virginia is only about half of that of Kentucky and Carolina. The largest excesses are found, so far as we have the record, in the farthest north and the farthest south, Russia and South Carolina. Yet Sweden and Norway are in the same latitude with Northern Russia; Prussia, Hanover, Saxony, and Great

Britain, are in the latitude of Central Russia, and all have a much smaller excess. From the facts here presented, there is no reason to think that climate has any influence in the determination of sex of the human child.

Births.

Country or State.	Period.	Males.	Females.	Total.	Males to 1000 females.	Per cent. excess of males.
Sweden	1796–1805; 1816–1835	1,335,798	1,275,007	2,610,805	1047	4.7
Norway	1801–1835	544,226	518,994	1,063,220	1048	4.8
Russia	1801–1833; 1842	27,087,237	24,774,368	51,861,605	1093	9.3
Prussia	1820–1834; 1839–1841	4,809,050	4,537,721	9,346,771	1051	5.1
Saxony	1832–1841	321,000	304,283	625,283	1059	5.9
Hanover	1832–1842	306,234	290,661	596,895	1053	5.3
Wirtemberg	1833–1842	366,513	343,661	710,174	1066	6.6
Bavaria	1836–1839	311,847	293,256	605,103	1063	6.3
Austria	1834; 1837–1841	3,474,596	3,229,386	6,703,982	1072	7.2
Belgium	1840–1849	670,344	636,165	1,306,509	1053	5.3
Florence	1451–1845	603,708	582,807	1,186,515	1035	3.5
France	1817–1851	17,078,694	16,105,321	33,184,015	1060	6.4
Scotland	1855–1858	185,210	175,463	360,673	1055	5.5
England and Wales	1811–1820; 1837–1856	7,112,849	6,792,455	13,905,304	1047	4.7
Europe	....	64,207,326	59,859,549	124,066,875	1072.6	7.26
Massachusetts	1842–1857	186,229	180,162	366,391	1033.6	3.3
Rhode Island	1853–1857	6,460	6,180	12,640	1045	4.5
Connecticut	1848–1857	39,859	37,174	77,033	1069.5	6.9
New York	1847–1848	35,226	32,503	67,729	1053	5.3
New York City	1853–1856	30,313	28,169	58,482	1076	7.6
New Jersey	1850–1856	44,387	40,789	85,176	1088	8.8
Philadelphia	1825–1855	151,443	139,829	291,272	1083	8.3
Virginia	1853; 1855; 1856	49,982	47,366	97,348	1052	5.2
South Carolina	1853–1854; 1856–1857	6,667	6,036	12,703	1104	10.4
Kentucky	1852–1857	67,871	61,491	129,362	1103	10.3
United States	....	618,437	579,699	1,198,136	1066.8	6.68
COLOURED.						
South Carolina	1853–1854; 1856–1857	20,865	19,651	40,516	1061	6.1
Kentucky	1852–1857	16,209	14,946	31,155	1084	8.4
Coloured	....	37,074	34,597	71,671	1071	7.1
Total	....	64,862,837	60,473,845	125,336,682	1072.5	7.25

Neither can we discover any evidence of the influence of season. In Massachusetts the largest proportion of males was conceived in February, the coldest, and in August, the warmest months; and the smallest in May, October and November. In Kentucky, through five years of record, the largest excess of males, 13 per cent., was conceived in April and June, and 11 per cent. in January, March, and November; the smallest, 2 per cent., was in August, and 6 per cent. in September. In England there is less difference both in the temperature of the seasons and in the excess of males born in different quarters of the year. The English reports state the births as to quarters, not as to months. An analysis of 4,783,441 births through the nine years, 1847 to 1855, shows that for every 10,000 females conceived, in each of the several quarters, and thereafter born, there were in the quarters ending with March, 10,429 males; with June, 10,469; with September, 10,449; and in that ending with December, 10,470. This is a small difference between the two extremes, the 1st and 4th quarters, embracing the colder months, and a still smaller difference between winter and summer. The largest excess is in autumn and spring. These proportions were not constant. The excesses of the males in the four quarters of the three years,

1853 to 1855, were severally 4.49, 4.87, 4.22, and 4.52. In the five years, 1848 to 1852, they were 4.1, 4.5, 4.6, and 4.8, per cent. And in the whole, as stated above, showing a general constancy, yet a variation which seems not to be due to the seasons. It must, however, be borne in mind, that the seasons vary less in Great Britain than in the United States. The mean temperature of the air at Greenwich, England, for the six years, 1849 to 1854, was, in the 1st quarter, 40.6°; 2d, 51.9°; 3d, 60.5°; and in the 4th, 44°; being a difference of only 20° between the winter and summer. The mean temperature of Massachusetts was, in the 1st quarter, 27.34°, and in the 3d quarter, 67.56°, of the same years, showing a difference of 40°.

The English reports include the cholera year of 1849, when, in the whole kingdom, 53,293, and in London, 14,137, died of the cholera, and an unusually large proportion died of diarrhœa. About four-fifths of these died in the third quarter ending with September. A comparison of the conceptions in this quarter, with the same in the years immediately preceding and succeeding, shows, that there was a diminished proportion of males conceived in that time of depression, alarm, and exhaustion. The excess of male conceptions in the healthy summer was in England 4.2, and in London 4 per cent., and in the cholera summer they were 3.9 per cent. in the whole nation, and 2.1 per cent. in the metropolis. The summer of 1847 was very sickly in London. Zymotic diseases, and especially typhus fever, were more prevalent and fatal than in other years. The excess of male conceptions was in that season only 2 per cent., being only one-half of that in other years. These differences are important, and tend to corroborate the doctrine of Villermé, and of Dr. Emerson, that the proportion of the sexes born depends, in great degree, on the health and energy of the people at the period of conception.

The effect of season, on the number and frequency of conceptions, occupies the attention Dr. Wynne, and he devotes to it one of his best chapters. The season of marriage is to be considered in this connection. If the births average about four to a marriage, as they do in England, including the second, third, and fourth marriages, and also the illegitimate births, then about one-fourth of the conceptions would take place within three months of the union. If the marriages are distributed equally through the year, each season would have an equal share of the first conceptions. But if the marriages are distributed unequally, as in Massachusetts, where, during twelve years, the average of November was 62 per cent. greater than that of March, and 53 per cent. above that of the other months; or as in Kentucky, where, in the last quarter, the marriages were 37 per cent. above, and in the second quarter 30 per cent. below the average of the year, we naturally look for a larger proportion of conceptions in the winter months than in the others. The 10,069,116 births in England, of which we have the record of the date, are distributed with singular equality through the four seasons, being in the ratio of 241, 258, 259, and 242, in the several quarters, per 1000, in the whole year. The 2,509,243 marriages were distributed less equally, being in the ratio of 2053, 2532, 2399, and 3016, in the successive quarters, per 10,000 in the whole. Here is an excess of 47 per cent. of the marriages in the fourth over those in the first quarter, and an excess of only 5 per cent. of the conceptions in the first over those of the second quarter. It would seem then, that in England, at least, the first conceptions followed the marriage at such various intervals as to show no necessary and immediate connection, as to time and season, between these two events.

There is more apparent relation between these events in Massachusetts, at least, a greater proportion of the conceptions take place at or near the season of marriage. These unions are distributed more unequally there than in Great Britain. Making correction for the different length of the months, through the recorded years, the marriages in November were to those in March, as 248 to 100; and the order of frequency was 1, November; 2, October; 3, May; 4, September; 5, January; 6, April; 7, June; 8, December; 9, February; 10, July; 11, August; 12, March—the highest exceeding the lowest by 148 per cent. The marriages in the cold season, October to March, exceeded those in the warm season, April to September, by only 11 per cent. With the same correction for the length of the months, the order of conceptions was, 1, December; 2, November; 3, January; 4, October; 5, February; 6, March; 7, June; 8, July; 9, May; 10, September; 11, August; 12, April. The excess for an equal number of days in the cold season over those of the warm season, was 18 per cent. within the period herein quoted.

In England there was an excess of 2 per cent. of marriages in the winter, and an excess of 6 per cent. of the conceptions in the summer. According to the calculation, this excess of marriages in the cold season should have produced two-thirds of 2 per cent. excess of births in the winter half of the year; but on the contrary, there is an excess of 6 per cent. in the warmer half—making practically an excess of 7⅓ per cent. in the summer; whereas Massachusetts has an excess of 10 per cent. of conceptions in the winter, independent of those due to the excess of marriages. Thus the facts we have been able to obtain, give no ground for the theory that season has any influence on conception. We then leave this topic in the trust that further experience and observation will develop and establish some law, if there be any, by which conception is influenced by climate or season.

The order of frequency of marriage in France, in ten years, 1831 to 1840, was: 1, February; 2, November; 3, January; 4, June; 5, October; 6, July; 7, May; 8, September; 9, April; 10, August; 11, March; 12, December. The order in Piedmont, in ten years, 1828 to 1837, was 1, February; 2, January; 3, April; 4, November; 5, May; 6, June; 7, September; 8, October; 9, August; 10, July; 11, March; 12, December. In Milan, Genoa, Naples, and Turin, the largest daily number of marriages was in February, and the lowest in December, in all but Naples, where the lowest was in March. The daily marriages were in France 3 times, in Milan and Genoa 6 times, in Piedmont 7 times, in Naples almost twice, and Turin more than twice as many in the highest as in the lowest months.

In Massachusetts, in the years quoted, there were 117,655 marriages, and 323,488 births, being 2.75 to a marriage of every degree. In England and Wales, in fifteen years, there were recorded 2,144,445 marriages and 3,612,532 births, or 4.015 births to a marriage. These, however, are not to be taken as the exact proportions of fecundity, for, as Massachusetts is more progressive in its population than England, a large proportion of the marriages were recent, and have not produced all their fruits. According to Hain and Moser, quoted by Baudin, the births to marriages were, in—

Prussia	1840 to 1849	4.16	Sweden	1821 to 1826	4.03
Austria	1830 to 1847	4.29	Low Countries	1825 to 1830	4.83
Hanover	1823 to 1843	4.03	Belgium		4.40
Bavaria	1836 to 1844	4.26	Geneva	1814 to 1833	2.75

Dr. Wynne states, p. 97, that marriage takes place earlier at the South than at the North. This corresponds with the records, which show that in

South Carolina 43.24 per cent., in Kentucky 44.7 per cent., in Massachusetts 23.60 per cent., in England 12.07 per cent., and in Ireland 17.4 per cent., of the females, when married, were under 20 years old; and in South Carolina 38.3 per cent., Kentucky 37.4 per cent., Massachusetts 46.98 per cent., England 49.18 per cent., and in Ireland 41.1 per cent., were between 20 and 25. These harmonize with the general law, that heat of climate tends to produce early development of the physical constitution, and hence leads to early marriage.

In discussing the subject of mortality, Dr. Wynne is compelled to feel the same want of records as in regard to births and marriages. Only eight States, and a few more cities, have gathered and published these facts, and none of the State reports are complete. Even in Massachusetts only one hundred and sixty-four towns, about half of all, including, however, about two-thirds of the population, have made so manifestly perfect returns of their deaths as to justify the calculation of the rate of mortality. We are not aware that "Bills of mortality are kept by most of the populous towns in the United States," as our author supposes; and if it be so, they are not published. We have made long and persevering inquiry for these; but except the few, whose annual and full reports are now before us, and some others whose reports are printed weekly or monthly, in detached parts, without summaries, we receive the general answer that no such documents are published in any available form.

The Mortality Statistics of the United States, prepared and published by the National Government for 1855, compressed, by law, into a volume of 400 pages, includes all the reported mortality in the year ending June 1, 1850, and also the causes, place, and time of death, the duration of sickness, and the sex, age, and nativity of the deceased. Mr. DeBow, faithful to the mandate of Congress, did not exceed his limits; yet he packed each of his pages so closely as to rarely have an equal in respect to economy of space. But even then he was obliged to omit much that was desirable for science, and would have been profitable for the people to know.

It was important to have the popular names of diseases, which the marshals collected in every part of the country and returned to Washington. No such opportunity has ever been presented before, and perhaps such will not occur again, of obtaining the common and local names of diseases throughout the United States. When Dr. Farr, by order of the Registrar General, prepared the nomenclature of Great Britain, he gathered all the popular names, amounting to about a thousand, and translated them into a scientific nomenclature, and classified them according to their natural affinities, giving each its appropriate place, and its scientific and intelligible signification.

Thus, he made a dictionary of popular nosology, in which he included all the common terms, and attached to each its proper synonyme. This explains all the common names, many of which are variable and uncertain, and some are merely local, and unknown beyond the narrow districts where they are found. The Registrar General published this most valuable paper in his fourth report in 1842, and again in his seventh report in 1846, and also in a pamphlet of instructions prepared for the local registrars and sent to all in the kingdom.

When the Committee of the American Medical Association, appointed for this purpose, prepared the nomenclature of diseases, they gathered all the common and popular names that were to be found in the registration offices of the States and cities to which they had access. They found eleven hundred and forty-seven of these names. These were arranged and explained

by their scientific synonymes, like Dr. Farr's, and published in the first Report of the *Transactions* of the Association, for 1846 and 1847.

The late census inquiry extended over a much wider field, and reached every State and almost every county and town in the United States, and without doubt every popular and local name of disease that could be supposed or imagined to be fatal was obtained. All of these are now in the files of the Department of the Interior, at Washington, and it is yet to be hoped, that Congress will order their publication, and present them to the world, with the location, State, and county, where they were found.

It is manifest and conceded, by the Superintendent of Census, that these records of mortality do not include all the deaths of the year, covered by the inquiry. In some States, some whole counties made no return of these events. Many of the deputy marshals, having charge of small districts, reported none. It is plain, from the very method of the inquiry, that there must be deficiencies. Many families remove and leave no trace behind. Strangers die and are forgotten. Some children die and are not recorded. In a large proportion of the districts and towns of the United States, no public mortuary records are kept, and family records are far from being general, and, moreover, even those which are kept, usually contain only the name and age of the deceased, and the time of death. Many of the persons who gave the information, however well they may have understood the statistics of the family and their condition at that time, were not familiar with the history of the year, and, therefore, could not give an account of even the deaths which may have occurred in the household. It was not to be expected, then, that this would be a perfect report of the mortality of the United States. In some States, especially in those whose people were accustomed to keep such records, and whose governments are accustomed to gather and publish them, the result of the national inquiry probably approached near to the truth. In Massachusetts, the national officers obtained the report of 19,409 deaths. The State reports, for the same months, give 19,848. Even this State return is incomplete, for several towns, containing about 30,000 inhabitants, made no return, and other towns containing about one-third of the population, made returns that were manifestly imperfect. Making correction for these defects, there should have been at least 21,000 deaths reported. Yet it must be granted that the national authorities came as near the whole facts as ought to be expected, as this was the first time that this information was sought by the Federal Government, and this was done through officers and in States entirely unused to the work.

We have no such means of measuring the probable correctness of the national statements of mortality in other States as we have in Massachusetts. Even Connecticut and New Jersey, whose registration laws were in operation in 1850, did not obtain the reports of their deaths with sufficient fulness and accuracy to afford a standard of comparison.

While it may be reasonably claimed, that all the statements in the volume of mortality statistics are correct in themselves, yet it must not be presumed that they include all the facts of this nature in any State, and they must not be taken as such; nor have they such an appreciable relation to the entire mortality of the year that they could be corrected and the whole number approximated. They must be considered as a collection of individual facts, correct and very important in themselves and in their relation to each other, but not as a whole. No deduction, therefore, as to the rate of mortality to the population in any State or district can be drawn from

these. No comparison of these rates in any one part of the country with that of any other part, or of this with that of any other nation, can be founded on these returns, and all such that have been or may be made only mislead. It was a mistake, therefore, into which the Census Department allowed itself to be drawn, when, in 1851, it calculated the rate of mortality on this basis, and published it in the report of that Department in 1852, and again calculated the rate in six divisions, and published the results in the abstract of the census, pages 12 and 140, printed by order of Congress in 1853. No man, having any knowledge of the law of population and mortality, can for a moment believe that the deaths were one in 100.29 in Vermont, and one in 51.23 in Massachusetts; one in 232.82 in Oregon, and one in 53.19 in New Mexico. These wide differences carry their error on their very face, and should not have been sanctioned by the government. Yet the errors were extensively spread; many newspapers and other journals republished them, and drew conclusions as to the comparative health and mortality of the several States, and of the nation; and some of these statements and conclusions were republished abroad, giving widely erroneous notions of the sanitary condition of this country and of its several parts.

Admitting, then, that this volume throws no light on the rate of mortality in the several States, still there are many other points on which much is to be gained from it. Although the facts which are here presented are only a part of the whole which had happened in the year investigated, yet it is not assuming too much to suppose, that the deficiencies are distributed in a similar proportion in the various classes in the same States, that they fall in a similar ratio on the males and on the females, and on the various ages of the deceased and the various causes of death, and that the ratio of the sexes to each other, of the deaths at any given age, to those at any other age, or of all ages, in any season to those in any other or all seasons, and from any class of disease to those from any other or all diseases, as stated in this volume, are the same as the similar ratios in the actual and total mortality in the same districts, counties, and States, in the year of inquiry.

With these premises we can, by the aid of the facts in the mortality volume, determine the proportion of the whole mortality of the country, or of any of the States or counties herein reported, that fell upon any period of life, or in any season, or was caused by any class of diseases. We may also compare the relative prevalence of any cause of death, and the relative force of mortality at any given age, or in any given season, in any State or district with the same in any other, or of the whole nation with the same in any other country; and if the year of inquiry 1849–50 is a representative year, if the diseases prevailed in the same ratio to each other then as in other years, and if the mortality fell in the same ratio on the several periods of life, or in the various seasons, or on the two sexes, then this volume of mortality statistics, of only a single year, becomes of very great value; and however we may feel the want of more complete information in this volume, we look upon it as a store-house of most important facts, the like of which has not been before offered to our people. Making what use we may of that which we now have, we will hope that at the next census, in 1860, a still further advance will be made, which shall fully represent the sanitary and vital condition of all parts of our country.

The marshals found and reported 323,023 deaths in the year preceding June 1, 1850; of which 172,878 were males, and 150,145 females. Here, as elsewhere, there was a preponderance of early mortality. While the

children under one year were 2.7 per cent., and those between one and five 12.3 per cent., of the whole living population, the deaths in the first age were 16 per cent., and in the second 21 per cent., of all that were reported. This is the general law of mortality, which is corroborated by all other records. In England and Wales, during the ten years 1845 to 1854, the deaths under one, and between one and five, were 22.8 and 16.5 per cent. of all; while the population at those ages were 2.7 and 12 per cent. In France, in the year 1853, the deaths in the first, and between that and the end of the fifth year, were 17.7 and 11.3 per cent. of all; but only 1.8 and 7.4 per cent. of the living were in these ages.

Baudin (*Geog. et Statist. Méd.*, II. 77) says, that in Saxony, Switzerland, Sardinia, Prussia, England, Holland, Sweden, Norway, and France, the average of the mortality was 4524 deaths, under 15 years of age, out of 10,000 of all ages; and of these, 2406 were males, and 2,118 females.

The males begin life with a majority of numbers. They have, at the outset, an excess varying from three to ten per cent. above the females. But their mortality is more than proportionately large, especially in early infancy. Without intending to give any rate of mortality for the United States, yet it may be profitable to compare the proportion of early mortality in the two sexes. The ratio of deaths in the year 1849–50 was—

Under 1 . .	Males	17.1	Females	16.4	per cent. of all.
Over 1 and under 5	"	21.0	"	21.5	"

The ratio of living was—

Under 1 . .	Males	2.68	Females	2.91	per cent. of all.
Over 1 and under 5	"	12.2	"	12.4	"

This shows, in the first period, a considerable difference, and, in the second, a slight difference, in favour of the females.

In England, the rate of mortality among the males was greater in the first five years, and slightly less in the period from five to twenty, than among the females.

The following table, taken and calculated from such of the reports containing the facts as we have been able to obtain, shows the excess of mortality of males and females at the several ages.

Excess of Ratio of Mortality.

Age.	England and Wales.	France.	Massachusetts.	Kentucky.	South Carolina.	United States.	Sweden.
1 year	...	...	Male	Male	Male	...	...
Under 5 . . .	Male	Male	Male	Male	Male	Male	Male
6 to 10 . . .	Male	Female	Male	Male	Male	Male	Male
11 to 15 . . .	Female	Female	Female	Female	Male	Female	Male
16 to 20 . . .	Female	Male	Female	Female	Female		Male
21 to 30 . . .	Female	Male	Female	Female	Female		Male
31 to 40 . . .	Male	Female	Female	Female	Female	Male	Male
41 to 50 . . .	Male	Female	Male	Female	Female		Male
51 to 60 . . .	Male	Male	Male	Female	Male		Male
61 to 70 . . .	Male	Female	Male	Male	Male	Male	Male
71 to 80 . . .	Male	Male	Male	Male	Male		Male
Over 80	Male	Male	Male	Male	Male	Male	Male
Over 90	Male	Male	...	...	...	Male	Male
Total . .	Male	Male	Male	Male	Male	Male	Male

The rate of mortality in England and Wales through seventeen recorded years was—

Under 5	Males	7.356	Females	6.343
5 to 10	"	.916	"	.895

In France, through ten years of record—

Under 5	Males	8.455	Females	7.545
5 to 10	"	1.049	"	1.109

The comparative rate of mortality of the sexes, was against the females from 11 to 30 in England and Wales; from 6 to 15, from 31 to 50, and from 61 to 70 in France; from 11 to 40 in Massachusetts, through thirteen years of experience; and against the males in all the other periods in these countries, and in all periods in Sweden. There was a greater proportion of mortality reported in Kentucky, through five years, among the females from 11 to 60, and among the males at all other ages. The excess of female mortality is not large at any period, so that the total in all States and nations, from which we have the returns, the excess is with the males.

According to Baudin, the record of the mortality in ten European nations shows, "that there are three critical epochs for females, first, that of puberty, from 15 to 20; second, that of maturity, from 30 to 35; and, third, at the termination of fecundity, from 55 to 60." "There are also two critical ages for males, first that of virility, from 20 to 25, and that of the beginning of age, from 50 to 60.

So far, then, as we have been able to obtain the facts, we may infer that, during the years including the development of womanhood and a part of the procreative period, there is a small increase and excess of proportion of female mortality, but the excess of male deaths at the other periods is more than sufficient to counterbalance these; so that, in the whole, the rate is higher in the stronger than in the weaker sex.

This result corresponds with the calculations of the expectation of life. The English life table, prepared by Mr. Farr, and printed in the "Sixth Report of the Registrar General," gives a probability of longer life to the females at every age; and Mr. Neison, in his *Contributions to Vital Statistics*, page 8, gives them a longer probability at every age from 10 to 95, and to the males at the higher ages.

The Irish table quoted by Norton, *On Life Insurance*, gives a longer expectation to males from 20 to 35, and from 62 to 70, and from 72 to 79; but at all the other ages it is in favour of the females. The British government annuitant's table, and all other calculations which distinguish the sexes, show a similar result in favour of the female.

The same result is shown in the tables of the life annuities, and the terms of the companies which dispose of them. They ask a higher premium for annuities granted to females than to males, because the former have a longer life, and must, therefore, be paid through more years.

The specific intensity of life is stated by Mr. Neison, in his "Contributions," to be greater in the male from 10 to 23, from 29 to 38, and from 99 to 105; at 24 it is equal for both sexes; and at all other ages it is greatest for the female, in England and Wales. In Scotland, it is greatest in the female from 26 to 28, and from 39 to 98, at 100 and 104, in the rural districts, and from 15 to 92, at 99 and 100 in the city districts; while at the other ages it is greatest in the male.

Notwithstanding there are universally more males than females at the beginning of life, as already shown, the greater mortality of the former soon removes this excess, so that in all nations whose record is obtainable, except the United States, there are more females living at most ages, and

in all nations whose record we have, except the United States, Tuscany, and Sardinia, there are more females in the total of the population.

At the three censuses of the United States—1830, 1840, and 1850—there were, among the whites, more females between 15 and 20, and over 70, and more males at all other ages. Among the free coloured, there were more females at every age. Among the slaves, the males predominated from 10 to 15, from 20 to 70, and over 100; in all other ages, the females were most numerous. Each of the enumerations of Great Britain, from 1821 to 1851, shows an excess of males in the early periods, and a large excess of females in the subsequent periods of life; and in all the enumerations from 1801 to 1851, there is an excess of females in the total. In France, in 1851, there was an excess or females from 20 to 26, at 28, 30, 33, 35, 40, 44, 48, 50, and 54, and all ages above. At each of the eight enumerations from 1801 to 1851, the females show the largest numbers in the total population. In Ireland, Russia, Germany, Denmark, Hanover, Saxony, Wirtemberg, Switzerland, Italy, Belgium, Norway, and Sweden, the same excess of females is found.

The large excess of males in the United States is due, in part at least, to immigration, which, in thirty-seven and a quarter years, brought 147 males for every 100 females. If the 250,000 aliens, estimated, by Mr. Bromwell, in his excellent *History of Immigration*, to have arrived between the Revolutionary war and October 1, 1819, were in the same proportion, then there was an excess of 533,915 males over the females among the foreign immigrants, between 1783 and 1850. According to the law of mortality, the survivors of these, in 1850, must have been much less than the excess of 489,736 males found among the whites at that time. This large proportion of males among our people cannot be explained by any analogy of the population in other countries.

This proportion of the sexes varies with different countries, and in the same country at different periods. The excess of females in Great Britain was 2.2 per cent. in 1821, 3.7 in 1841, and 3.2 in 1851. In France it was 2.4 in 1801, 4.09 in 1806, 5.9 in 1821, 4.5 in 1831, 2.4 in 1841, 1.8 in 1846, and 1.08 in 1851. On which Boudin remarks, with singular truth, "in proportion to the time elapsed from the wars of the republic and the empire, we see the equilibrium of the sexes is re-established, which is inevitably destroyed, by the great destruction of men in the strife." In Ireland the female excess was 3.38 in 1841, and 5.38 in 1851. This change is doubtless due to the great emigration which carried off a larger proportion of males, and left a larger ratio of females behind. The excess of males over the females in the United States has varied very little in the sixty years from 1790, being at the seven enumerations successively, 3.7, 4.9, 3.9, 3.3, 3.7, and 4.6 per cent. In New England there has been an excess of females, varying from .82 per cent. in 1790 to 3.01 in 1820, and .87 per cent. in 1850. In all other sections, the excess is in the other sex at every decade. This male excess was larger in the early than in the later periods; and when the country or State is peopled exclusively with immigrants, there is necessarily a large preponderance of the stronger sex; but this gradually diminishes with the increase of the natives. Ohio had in 1790 an excess of 18.6 per cent. of males, which gradually decreased to 5.5 per cent. in 1850. The Middle States had an excess of 5.5 per cent. in 1790, and 2.78 per cent. in 1850. The South Atlantic States had an excess of 5.94 per cent. in 1790, and 1.04 per cent. in 1850.

The equation of life, or the period at which half are dead, is shown by Mr. Neison to be in favour of females. In England and Wales, of those

who survived their 10th year, one-half of the females would survive to their 54th, and of the males to their 53d year. In the Friendly Societies, which, Mr. Neison supposes, enjoy a greater longevity than the average of the people of Great Britain, he finds that, calculating from the 20th year, the equation is 1.1 per cent. in favour of males, but calculated from all other decennial periods, it is favour of the females in a much higher degree.

It is safe, then, to assume, from all these facts, both in Europe and in the United States, that females enjoy a greater length of life, and perhaps a greater degree of health, than the males.

There may be differences in the relative value of male and female life, caused by climate and locality, in the north and in the south, in the city and the country, in the mountain regions and the valley; but we have no records of facts to determine these, still less to demonstrate them. We must, therefore, wait for further observations, before any reliable deductions can be drawn as to the comparative effect of climate, and of local and endemic influences on life in the two sexes.

In the wide extent of the United States there is almost every variety of climate. Florida, on the extreme south, with a range of latitude from 24° to 29°, has an annual mean temperature of 67°; and Maine, on the extreme north, between 43° and 47°, has an annual mean heat of 40.99°. These are subject to still further variations of temperature. The average mean of winter in Maine is 18°, and that of summer in Florida is 80°. These are the averages of the observations at the various military posts in these States. At the most southern post in Florida, Key West, the mean of the thermometer was for the year, 76°, and for the summer 82.51°; while at Fort Kent, the farthest north, it was 37° for the year, and 11.36° for the winter. Beside these extremes for the entire seasons and years, there were days and weeks in which the mercury went far above and far below these points, and these were periods when the cold of the arctic regions, and the heat of the tropics, were experienced within the limits of the United States.

There are also all varieties of local influences within this Union—the seashore and the mountain ranges, the elevated, primitive, granitic formation, and the alluvium of the river banks, the arid waste of the desert, and the rich everglades of Florida. Unquestionably these varieties of locality and formation have a certain influence upon the life and health, the vital force, and the physical well-being of those who dwell among them. The effect of climate on the human constitution is everywhere admitted. It is supposed that life is shortened, and the energies materially diminished, by the extremes of heat and of cold. But we have not sufficient data to demonstrate these matters.

It is generally supposed that zymotic diseases, and those of the digestive organs, are more prevalent and fatal in warm countries, and diseases of the respiratory organs are more destructive in the colder regions. Here, as elsewhere in the field of vital statistics, facts are wanting to produce unquestioning faith. We have the Mortality Report of the United States in the year ending with May, 1850, and the Annual Registration Reports of Massachusetts for sixteen years and eight months, Rhode Island for five years, Connecticut for eight years, New York for two years, South Carolina for four years, Kentucky for five years, New Jersey for seven years, and Virginia for two years, all of which, except the last, state the causes of death. These are the only bases we have for even a conjecture of the comparative prevalence of the various diseases, or classes of diseases, at the north and the south. As none of these reports, not even those of Massachusetts, include

the whole mortality of the State or country, no inference can be drawn from their facts as to the rate of total mortality, or of that produced by any specified cause or class of causes. But, as it is presumed that the imperfections are distributed in nearly equal proportion over all the statements, it is reasonable to suppose that the given number of deaths from any specified cause, bears a similar ratio to the total mortality from that cause, that the given number from any other cause does to its total mortality; and that the total numbers destroyed by the various diseases stand in the same proportion as the numbers that are stated.

Hence a table deduced from these reports, showing the ratio of deaths from various causes to the whole mortality from all specified causes, in different regions, may be interesting and instructive. The analysis of the statements, in the National Mortality Report, of the deaths from zymotic, digestive, and respiratory diseases, in South Carolina, Georgia, and the five States bordering on the Gulf of Mexico, and in the ten extreme northern States, including New England, New York, Michigan, Wisconsin, and Iowa, and a digest of the same facts in all the reports of Massachusetts, Rhode Island, Connecticut, and New York, and also of South Carolina and Kentucky, furnish us with the facts in the following table.

According to the National Report there were, in the single year covered by the inquiry, 89,143 deaths from known and stated causes in the ten northern States, and 43,536 in the seven southern States. According to the several State Reports there were, in the four northern States above quoted, 307,070 deaths from given causes, and in the two southern States, 64,176. The eighteenth Report of the Registrar-General shows the number of deaths from the various causes in the seven years, 1848 to 1854, including 2,869,898 from all causes.

Ratio of Mortality per 10,000 of all Deaths, from all Stated Causes.

CAUSES.	NORTHERN STATES.		SOUTHERN STATES.		ENGLAND AND WALES.
	National census.	State reports.	National census.	State reports.	1848–1854.
All stated	10,000	10,000	10,000	10,000	10,000
Zymotic	4,110	2,950	4,220	4,735	...
All respiratory	2,784	3,319	2,192	2,595	2,436
All digestive	2,809	1,731	2,398	2,549	1,097
Consumption	1,799	2,250	441	838	1,235
All respiratory except consumption	985	1,035	1,749	1,757	1,201
Bronchitis, catarrh, influenza, laryngitis	55	57	175	169	505
Pneumonia	401	467	747	713	538
Pleurisy	30	55	69	29	23
Croup	242	260	304	425	98
Hooping-cough	173	110	332	252	213
Asthma	10	16	25	25	108
Cholera, cholera infantum, diarrhœa, dysentery	2,355	1,202	1,532	2,111	745
Dysentery	1,032	692	220	1,115	57
Jaundice, disease of liver	73	87	72	67	158
Teething	91	117	189	119	107
Worms	26	11	321	166	...
Fever, scarlet	304	455	187	304	396
" typhoid	288	598	443	681	435
" all others	460	65	1,487	735	15

For the purpose of showing, at a glance, the comparative prevalence of the diseases of the respiratory and digestive systems, all the diseases of those organs are included under these terms in this table. Croup, hooping-cough and influenza, and diarrhœa, dysentery and cholera, which are first included in the zymotic class, are, therefore, repeated.

Although the national record covers a wider field, seventeen States, it includes only a single year, and is, therefore, a less certain indication of the permanent prevalence of diseases than the State records, which include several years, but only six States. The cholera prevailed in New York and Louisiana, and dysentery in New England and New York, in the year of the census inquiry, and gave a larger proportion of mortality from those causes and from zymotic diseases than is found in those States in the average of years. Consequently there was a larger proportion of deaths from these causes, at the North than at the South, in the year 1849–50, while in the other years, reported by the States, there was a large excess in the warmer regions.

Looking, then, at the State registers, which, for our present purpose, give a more reliable basis of comparison, and taking the classes, we find that the zymotic and digestive diseases are relatively more fatal at the South, and the respiratory disorders more fatal at the North. But an analysis of these classes shows that this relation is very materially changed in regard to many specific disorders. Consumption, that terrible scourge of the colder climate, is not absent in the warmer. It destroys about one-fifth of all who die in these Northern, and one-twelfth of all who die in these Southern States. Taking away this disease, the respiratory class shows a larger proportion of mortality in the warmer section. And pneumonia, croup, and hooping-cough, show a very large excess of mortality at the South, their proportion of the whole being severally 53, 63, and 129 per cent. greater there than at the North. Fevers—continued, remittent, and intermittent—were largely, 1030 per cent., in excess at the South. Scarlet fever prevailed 49 per cent. relatively more in the colder States. The deaths from jaundice and disease of liver were more at the North, from worms fifteen times as prevalent at the South, and from teething about the same in both sections. Not much reliance can be placed on the statements in respect to the last three diseases, because, being generally connected with other disturbances of the digestive organs, they are frequently reported under other heads.

It is generally supposed, that whatever may be the peculiar dangers and diseases of any climate or region, these fall more heavily upon the strangers than upon the natives; that the immigrants are more subject to local and climatic influences, and to the endemic and epidemic disorders, than those who were born among them. But it is also supposed, that the constitutions of the strangers, by protracted residence in a new locality, become gradually accustomed and inured to these dangers that surround them, and so moulded and strengthened by the exposure as to be able to resist them more and more, and consequently their liability to suffer from, and to fall beneath, the attacks of diseases peculiar to their new situation, grows less and less, and ultimately they may be almost, and, perhaps, quite as secure as the natives.

We have very few records to establish or refute this opinion. Those who support it found their faith rather upon general observation than upon actual measurement or enumeration. On the contrary, we have one record of facts, which, so far as it goes, proves that the acclimation is rather ex-

haustive than strengthening to the human constitution. The experience of the East India Company furnishes the only reliable facts we have been able to find in reference to this matter.

From the records of the Bengal Government, copied into the *Journal of the London Statistical Society,* we have the facts of the following table :—

Rate of Mortality of Europeans residing in the East Indies.

Age at death, in years . . .	20 to 25	25 to 30	30 to 35	35 to 40	40 to 45	45 to 50	50 and over
Years of residence in East Indies .	1 to 5	5 to 10	10 to 15	15 to 20	20 to 25	25 to 30	30 and over
Mortality per 1000 of the class . .	19.9	20.8	16.6	23.4	35.4	36.4	48.6

From this, it would seem that the effect of climate is accumulative, and grows more and more destructive with the length of exposure.

The term acclimation is often used in a restricted sense, meaning merely protection from a single prominent and dangerous disease. The immigrants in Louisiana are said to be acclimated when they have passed safely through an attack of yellow fever, that is, if this disorder attacks men only once, they are acclimated so far as danger may threaten them from this cause. Dr. Barton says, "Perfect acclimation is only to be derived from having had the disease." But still the question remains unanswered, whether a man, having had the yellow fever, and thereby secure on that side, is also secured in any degree from the attacks of any other diseases which are peculiar to the same district—whether his constitution, in its power of endurance and resistance, becomes by this means, or by protracted residence, assimilated to that of the native; or does the protection from this fever, like the experience of the smallpox, measles, &c., stand alone, leaving the patient as subject to all other diseases as before?

In connection with acclimation there is another consideration worthy of the attention of the philosopher, that is, the effect of a new climate upon a race, through a succession of generations. How far, how securely, and how soon can a race be established in a climate different from that in which they originally lived and flourished? Are the children of the Northerner, born and raised in Louisiana, or in the Indies, endowed with a Southern constitution, with power to labour and enjoy life, to resist and overcome the attacks of disease, and to escape the peculiar dangers of the climate, like those whose ancestors through many generations have dwelt there?

The experiment of the Mamelukes to perpetuate their race in Egypt was a failure; their children perished, and they were obliged to replace them with new importations from the North.

Mr. Edward Clibborne, in an address before the British Association for the Advancement of Science, in 1856, asserted that the climate of the United States is exhaustive to the European constitution, and although the British and other races may endure our dangers for one, two, or, possibly, more generations, they ultimately die out, and our country must depend on new supplies of European immigrants from generation to generation to sustain its population, for "it is clear, from causes now in operation, that, no matter how favourable the circumstances of the European people in the United States were, their extinction, at no distant period, was certain, provided the connection of America with Europe ceased."

We think Mr. Clibborne is mistaken, both as to his facts and as to his inferences. We have seen no evidence nor record of the facts upon which

he bases his doctrine. But it is not our intention now and here to show his error, that is self-evident to Americans.

Considering how prone the people are to change their residence—to remove from the North to the South, from the East to the West, from the mountain regions to the river bottoms, from the interior to the seaside, from the rural district to the dense city and the reverse, and knowing that there are great differences in the local and climatic influences of these diverse places, it is singular that the effect of change of climate and locality upon the human constitution has received so little attention.

There is no want of observation and record of the physical condition of various places. We have learned, or are taking measures to learn, all the meteorological facts—the state of the air, the temperature, winds, quantity of rain, all that the thermometer, barometer, raingauge, &c., can reveal; but the relations of these facts to man's vital force, his power to labour, his health and energy, his comfort and longevity, these are yet to be studied and known.

Our government has taken great pains to ascertain the cost and profit of introducing animals and vegetables from other countries into the United States.

The government has lately been trying the experiment of using camels from the East, in Texas, New Mexico, &c. The effects of the climate, and the local influence of food and water and labour upon these animals there are carefully watched, recorded, reported, and published by competent officers and superintendents. But no such study, record and report are made of the men who drive them, nor of the people who go from other regions and try the experiment of health and life in that strange country and climate.

By national authority, men of the highest scientific character have been sent abroad to learn the best and most approved means of fortification and war, and of using weapons of destruction; they have been sent to remote rivers to determine their fitness for navigation; to the prairies, the deserts, the mountain passes, the savage wilds of the uninhabited West, to learn their fitness for roadways; even to distant nations, to know their aptness for commerce. Most, if not all of our States, and most civilized nations, expend money freely, through agricultural societies and other associations for the encouragement of the arts, and in the employment of naturalists, philosophers and practical men to ascertain the method and profit of raising various kinds of animals, grains, fruits, vegetables, &c., all for the benefit of the capital and the increase of the income of the people. But nothing, or next to nothing, has been done here to determine the effect of locality, climate, condition, or circumstance, upon the health and life of man, nor whether he can change these without impairing his vital force.

Dr. Wynne discusses the effect of occupation upon human health with much ability, and refers principally to Mr. F. G. P. Neison's late and valuable *Contribution to Vital Statistics*, of which the 3d quarto edition was published in London, in 1857. This is indeed the most important addition to this science. It should be in every public library and accessible to every student of these matters. Mr. Neison's chapters on the Friendly Societies, and their experience in respect to the health and longevity of their members engaged in the various employments, teach important lessons in respect to the value of life among the working mechanics and the better class of labourers, as compared with that of the whole nation collectively, and of the higher classes separately.

The Friendly Societies in Great Britain, like the Odd Fellows' Societies

in America, act as a sort of mutual health and life insurance companies. They, therefore, take cognizance, and keep a record, of the sickness and death of all their members. Mr. Neison, through the government returns, and by his own inquiry, obtained the record of upwards of 2,000,000 of years of life, with the sickness and death; carefully analyzing these records of mortality, and comparing them with the general reports of death, and aided by the experience of the Life Insurance and Annuity Companies, he shows that the members of these Friendly Societies have a higher expectation or probability of life than the average of the people of England, the peerage, or the subjects of Life Assurance. "The healthiest Life Tables have not shown anything so favourable" as these. He, therefore, seems to think there is an error in the opinions of Mr. Chadwick, the Health of Towns Commission, Villermé, Griscom, and many others who have written, to show that poverty and its conditions are attended with a greater amount of sickness and a higher rate of mortality than more comfortable circumstances. We believe Mr. Neison is the first writer of high authority, who has questioned their doctrine; and that he has done so with insufficient reason. His own calculations and results will not here be disputed. It is admitted without qualification, that the expectation of life in these Friendly Societies is superior to that in the other classes named; yet it by no means follows, that there is no difference of vitality in connection with different conditions or that that difference is in favour of the poorer and less comfortable classes. It must be remembered that these Friendly Societies do not include all the poor; they are practically composed of picked men, for Mr. Neison says:—

Although "they are almost exclusively the hard-working members of the community, chiefly occupied in the drudgeries and toils of the mechanical arts, and consequently exposed to the inclemencies of the seasons, excesses of temperature, impure atmospheres, constrained postures, and other conditions usually thought objectionable;" although "their incomes are very limited, affording but the scantiest and simplest means of support, and their habitations are of an inferior order, of the cheapest kind, and in the worst streets;" "yet it is necessary to make a distinction between them and the great bulk of the poorer classes of the country."—*Contributions*, p. 38. "The members, and those likely to be candidates, are intimately known, in their daily habits and ordinary health, to each other, and where evidently bad health exists, admission is refused"—p. 42.

Even after admission is obtained the member is not sure of his position until death, for

"The fact of continuing a member of such a society presupposes great regularity of habits, otherwise difficult circumstances and distress would ensue, and from inability to continue his subscription non-membership would follow"—p. 13.

"Although a few pence is all that is needed for his weekly or monthly contribution, it presumes a certain amount of frugality and industrial habit sufficient to separate him from the reckless and improvident who are more openly exposed to the vicissitudes—poverty, destitution, and distress—incidental to fluctuations in the demand for labour"—p. 38.

Thus, only the best lives are admitted, while the dissolute, the intemperate, and the vagabonds, the lowest health, and the poorest lives, are excluded; and if any, originally good and acceptable, when admitted, afterward fall into bad habits, they are sifted out, and the societies are constantly purged of their unprofitable members, for

"The blessing thus bestowed on the frugal and industrious workmen of the country composing the Friendly Societies, in having granted to them a prolonged duration of life is, no doubt, the result of their simple and uniform habits

of life, and the more regular and natural physical exercises to which they are habituated"—p. 43.

Notwithstanding this unquestionable evidence of Mr. Neison of the superior longevity of the permanent members of these societies, still neither he nor Dr. Wynne has shown that the privations, the narrow streets, the crowded houses, the bad air, and the exposures of even the regular, sober, and industrious poor, are more favourable to health and life than the better conditions and circumstances of the regular, sober, and industrious among the more prosperous classes; nor that the dissolute habits of the irregular, abandoned, and idle, do not waste their vital forces and lead to earlier death.

Mr. Neison's large collection of facts enabled him to calculate the expectation of life of persons engaged in various trades, and in his contributions he shows, that at the age of 20, there is a probability, that the labourers will live 47, bakers and miners 40, painters 36, and clerks 31 years longer.

We have no means of determining accurately the effect of employments on life in this country. The average of longevity of the males, in the various occupations, who died at the age of 20 and over, is given in all the Massachusetts reports of mortality, except the first two, and in all the reports of Rhode Island and New York. These include 56,326 persons, connected with 163 employments. One English report only, the fourteenth, for 1851, states the ages in connection with 359 occupations. This shows not only the number, 93,869, who died, and their ages, in quinquennial and decennial periods, but also the number of the living who were engaged in each of them.

It is not possible here to give the average longevity in each occupation, nor would it be instructive to do so, inasmuch as in many there were so few deaths, that their average would be worthless. But we have arranged them into classes upon the plan described in a former article in this journal, vol. xxix. p. 429, rather according to the degree of exercise required, and their exposure to the elements, and the powers brought into use, than in regard to any chemical or other character and influence of the material on which they operated, with the following result:—

Average Longevity connected with Classes of Occupations in Massachusetts, Rhode Island, and New York.

Classes of occupations.	No. of deaths over 20 years old.	Average age, years.
Cultivators of the earth	18,300	61.89
Active mechanics, working in open air	3,929	50.42
Active mechanics, in shops	5,434	47.60
Inactive mechanics, in shops	5,357	41.64
Mechanics, trades not specified	1,396	44.44
Employed on the ocean	3,214	46.06
Labourers, without special trades	11,023	44.22
Others employed abroad, expressmen, teamsters, &c.	1,064	42.93
Professional men, students, &c., working with the brain	1,982	48.11
Merchants, capitalists, financiers, &c.	4,627	47.34

We add to these a few of the leading employments in which there were deaths sufficient to lead to an approximation of their longevity in the United States and in England.

OCCUPATIONS.	MASSACHUSETTS, RHODE ISLAND, NEW YORK.		ENGLAND AND WALES.	
	Number of deaths.	Average age.	Number of deaths.	Average age.
Farmers	18,300	61.89	9,784	63.30
Ship-carpenters . . .	408	58.22	327	65.34
Coopers	419	57.21	339	56.05
Carpenters	2,261	56.09	2,604	55.39
Clergymen	370	55.92	358	61.45
Lawyers	274	54.58	266	53.28
Physicians and surgeons . .	544	54.31	973	54.13
Blacksmiths	1,011	51.43	1,409	55.26
Tanners and Curriers . .	270	48.13	3,066	56.80
Masons	548	47.45	2,258	53.24
Seamen	3,205	46.01	1,752	47.59
Merchants and traders . .	2,334	45.32	2,301	52.94
Bakers	166	44.49	664	54.85
Labourers	10,407	44.39	1,501	56.88
Butchers	184	43.80	1,054	52.14
Shoemakers	3,458	43.41	3,236	56.88
Tailors	547	40.00	1,931	51.77
Painters	198	38.76	884	46.09
Machinists	552	38.17	329	44.22
Printers	198	32.03	298	46.21
Jewellers	136	30.52	148	50.26

Here is a remarkable difference in the average longevity of these classes in this country. The farmers live 22 per cent. longer than the active mechanics working abroad, 30 per cent. longer than the active mechanics in shops, and 48 per cent. longer than the inactive mechanics working under cover. It must be considered, however, that there has been a great expansion of most of the manufacturing employments, and of some this increase has been very great, within less than a generation. Shoemaking, the manufacture of machinery, printing, and some others, have grown rapidly within that time, and therefore the workmen connected with these constitute a progressive population, and have a larger proportion of youth and middle-aged men among them than the farmers. Labourers also have multiplied greatly, especially in Massachusetts, by the large accession of foreigners, who are in the younger and middle periods of life. Consequently the average age at death would be lower than in the older and less progressive professions, and lower than it will be, when these kinds of business shall be carried on without expansion to the completion of an entire generation. This progressive nature of several of these employments explains in some degree the great discrepancy between the average age in England and in America. There has been no increase among the labourers and shoemakers beyond the average of the nation in Great Britain. The living as well as the deaths in these occupations in 1851 included a due proportion of the oldest there, while here few had been long enough in those employments to become old, and, of course, there were few of the old to die. The longevity in a few of the occupations was nearly similar in both countries, but in most it was greater in England, and in some this excess was largely in favour of the elder nation. This is due, in good measure, to the reason above given, the more progressive nature of our population.

In calculating the average longevity, and comparing one employment with another in this respect, it is important to look at the way in which the professions are filled. Some are exclusively filled with young men, as the brakemen and conductors on railroads, coachmen, &c., who, after a few years, leave these employments and engage in others. Students, clerks, and a large portion of teachers are generally preparing to become professional men, merchants, &c. The average longevity of the few who die in these occupations is necessarily low, while the great majority pass to other pursuits, to die at a more advanced age, and swell the average age there. The average longevity of those who die in these employments is therefore no indication of their sanitary or morbific influence on those engaged in them, nor of their real expectation of life. In preparing the last two tables of the longevity of the professions, we included not only those who were actually engaged in them, but also those who, over twenty years old, were preparing to enter them; as with lawyers, physicians, &c., are included law and medical students, and with merchants are included clerks. Some other classes are made up, in great measure, of the aged and those who are beyond the middle of life. A large part of the paupers enter that class only through the door of age. They were self-supporters as long as their strength lasted; and when this failed, in declining years, their resources were cut off, and they were thrown on the public for sustenance. Many of the class of gentlemen, or men of independent means, but without special occupations, are those who created their fortunes in some employment, and in their later years retired from business and lived upon their former earnings. Of course both of these classes include a large proportion of the aged, and yet their average age at death is no indication of the healthful nature of pauperism or of wealth.

Since the living population is so differently constituted in various places and employments, the average age at death is often a very doubtful means of determining the value of life, and it should never be used, except with great caution and a careful consideration of the circumstances of the living. If the occupations or the countries which are to be compared on this basis are filled in the same way, and neither is more or less progressive than the other, and if both or all have been filled through an entire generation in which the oldest have had opportunities to try the full experiment of life, then the principle is a good one, and may be used with confidence; but otherwise it is a very loose mode of arriving at conclusions of the worth of life, and often leads to error.

We have not space here to even touch upon the many other important topics connected with human vitality and mortality of which Dr. Wynne treats in his volume. These, as well as those which we have here noticed, are worthy of the careful consideration of the philanthropist and the political economist, and we commend them to their earnest attention.

In reviewing this book, we have had frequent occasion to regret that its author could not have availed himself of all the records of facts in respect to life and death that have now been published. In all the uses of probabilities, especially when these are deduced from averages, it is of the utmost importance that these should rest on as wide a basis as is possible. Therefore we trust, that in another edition of this work, Dr. Wynne will be able to add all that has been observed, recorded, and published, and make his volume the grand store-house of the vital and mortuary statistics that shall then be known to the world. E. J.

ART. XV.—*A Treatise on the Pathology of the Urine, including a Complete Guide to its Analysis.* By J. L. W. THUDICHUM, M. D., with seven plates. London, John Churchill, 1858: pp. 429.

AMONG the special themes of study in the animal economy, none has attracted more attention, or produced a more voluminous literature, than urinary chemistry. It is, indeed, a tempting field of research. The fluid of which it treats is so easily obtained, and its relations to the general system are so obvious and so important, that it could hardly fail to force itself upon the notice of the student of physiology and pathology. There is but little danger that the importance of this study will be underrated. Our apprehensions are all in the other direction. One-sided studies are prone to lead their votaries to give undue prominence to them, to the neglect of other departments of inquiry.

Urinary chemistry has, indeed, specially manifested this tendency. Too exclusive deductions have been drawn from the condition of the discharges from the bladder. We have had brilliant theories built up on the foundation of the proportions of phosphates and chlorides passing away in the urine, and deductions affecting the most general considerations of pathology have been drawn from the determination of these salts. Yet we all know that a great quantity of phosphoric acid passes off through the intestinal canal, and there is no evidence against the supposition of a sort of complementary relation between the urine and feces in reference to the excretion of its salts, so that when deficient in the one they are in excess in the other. Urea, also, has been made the measure of the metamorphosis of nitrogenous matter in the body, and the physiological value of certain beverages has been deduced from the ratio of its diminution under their use. But why assume that urea is the only substance through which this waste gets exit? Why may we not as well infer that the change goes as far as ammonia, which is partly exhaled through the lungs, but chiefly passes off in the feces in the form of ammoniaco-magnesian phosphate. It is impossible to reason accurately with reference to any one function of the living body, until we know the state of all the others; so that it is absurd to attempt to build up a stately theory of diatheses upon the foundation of observations, however numerous and accurate, of the condition of a single secretion.

We are far from underrating the importance of the study of urinary chemistry. All we desire is that it shall be restricted within its proper limits, and that we shall not be led by science back to the point to which ignorance conducted our ancestors—to find all our pathology in a urinal. We hail any advances made in this department as contributions to the better understanding of the entire organism, but we ask that they may be checked by simultaneous observations of other excretions before they are received as interpretations of any general condition of the system.

It cannot be denied that, in spite of all the attention bestowed upon it, urinary pathology is still in a very unsatisfactory state. No one is more sensible of this fact than the careful and assiduous students of that medical speciality. It was, therefore, with no little satisfaction that we received the new treatise now under consideration. We must, however, caution our readers not to be misled by the title page. Those who expect to find an exclusive treatise on pathology, abounding in wire-drawn distinctions and brilliant speculations, are destined to disappointment. The order of the

book is that of a chemist, and it is a very full and satisfactory account of the present state of urinary chemistry and its relations to pathology. It entirely fulfils the author's promise in another clause of the title. It is truly a "Complete Guide" to the analysis of the urine. In this respect, we know of nothing in the language at all comparable to it. It is concise yet not obscure in its language and admirably lucid in its method. The author is manifestly thoroughly acquainted with the literature of his subject, and well versed in the manipulations of which he treats. He is no theorist, and has no hobbies to ride. Consequently, we have, as a result, a minimum of speculation and a maximum of fact.

We shall not attempt an analysis of a book which has so little superfluous matter. A running commentary on those points which particularly arrested our attention during the perusal of the volume may serve to give our readers an idea of its character, and for the rest we must refer them to the work itself.

The author begins with an account of the physical characters of the urine, under which head its clearness or turbidity, its odor, taste, and tints are discussed. For the latter, Vogel's scale is adopted and illustrated by a colored plate. A very useful table is also introduced in which the causes of each hue, and the most expeditious tests for its peculiar coloring matter, are given. In this also are included the tints imparted by the various foreign substances which reach this excretion through the medium of the circulation.

In reference to the vexed question of the acid reaction of the urine, the author is not quite so full as could be wished, and leaves the matter pretty much as he found it, attributing it in one place to the acid phosphates and in another acknowledging the presence of an unknown free acid. Lehmann's experiments with the baryta salts show conclusively that the acid phosphates are not always the sole cause of this reaction, which, he thinks, may also be attributed to hippuric and lactic acids. Our author gives Neubauer's formula for the estimation of this unknown free acid, oxalic acid being taken as the standard of acidity. This portion of the chemistry of the urine is in an extremely unsatisfactory state. The changes of the urine are briefly noticed. Allusions to Scherer's researches on acid reaction are scattered over various chapters, but we were disappointed in finding no continuous discussion of them in this place, where some account of acid fermentation was to be expected.

The question of alkalinity is more fully discussed. This is well known to depend upon fermentation within the bladder, when not caused by alkaline ingesta. The carbonate of ammonia, thus formed, gives rise to all the bad consequences of precipitated phosphates. In a note to this chapter the author gives some excellent practical hints in reference to the selection and preparation of test-paper.

Concerning the collection of the urine, and the determination of its quantity, we have some very sensible remarks. Great stress is laid upon the absolute necessity of accuracy in this particular, and, indeed, without it, the most elaborate analyses are of little value. To determine the pathological and physiological significance of any particular ingredient in the urine, it is imperatively necessary to know, not merely its *per mille* proportion, but the actual amount discharged in a given time. It is to stand as an index of waste or absorption, and any statement of mere proportion gives us no definite information. The relative proportion of water and solids may vary; so, also, there may be a difference in the relations of the latter to one another,

or a particular solid of grave pathological importance may owe its entire significance to its absolute quantity and its constant production.

The rules given for the determination of the solids are excellent. The method of evaporating the urine by heat is highly objectionable, because the acid phosphate of soda constantly decomposes the urea, giving rise to carbonate of ammonia, which passes off with the watery vapour. An accurate determination of the separate solids, therefore, cannot be made to correspond, in its amount, with the total weight as obtained by drying in this manner. The best method is unquestionably to evaporate over some hygroscopic substance *in vacuo*, and, indeed, this is the only plan admissible in any delicate analysis of organic substances. The direction of our author, to keep the vessel containing the solid residue hermetically sealed during the process of weighing, is by no means to be despised, as these organic bodies so rapidly absorb water from the atmosphere, that it is impossible to keep the weight constant long enough to determine it with accuracy.

The attempt to estimate the total weight of solids from the specific gravity of the liquid is utterly futile. Our author very clearly sets forth the amount and cause of the error in this method, even when the greatest precautions are taken. The ordinary mode of taking it renders any approach to accuracy absolutely impossible, the inevitable mistake being multiplied by the unknown quantity, carelessness in the operator. A plan of estimation which is liable to a necessary error of one-tenth of the entire amount of material estimated during health, and one-fourth during disease, has certainly no claims to anything like accuracy. The gravimeter, therefore, must be considered as a very rude and imperfect instrument of research, capable of giving some clinical information, but utterly worthless in the decision of all questions which require delicacy of determination. All inferences as to waste of tissue, which may have been drawn from observations made with it, must be ruthlessly rejected.

The chemical history of the various constituents of the urine commences naturally enough with urea. The instructions for its detection and determination are very explicit. To avoid the disadvantages of coagulating by heat albuminous fluids in which urea is to be sought, the plan of extraction with absolute alcohol is recommended. This agent, at the same time that it coagulates the albumen, dissolves the urea.

The method recommended by our author for the quantitative determination of urea is that originally devised by Liebig. This, as is well known, is accomplished by precipitating the urea with a solution of nitrate of mercury of known strength. Great care is necessary in the preparation of the reagents for this purpose, and the most minute directions are given by our author, guarding the student against every possible source of error.

Bunsen's method of determining the amount of urea, by the quantity of carbonate of baryta formed during its decomposition at the temperature of an oil-bath, is also given. This is the most elegant and accurate of all chemical methods for urea, none of the other constituents of the urine being similarly affected.

Davey's plan, which is also described, is objectionable, because uric acid is liable to the same change with urea. It consists in treating urine in a graduated vessel with hypochlorite of soda, the amount of urea being estimated by the quantity of nitrogen gas evolved. This plan is liable still further to the objection which lies against all volumetrical determinations of gases, as being too delicate for the common use of the profession. So many and so careful corrections must be made for temperature, barometric

pressure, &c., that it is a hopeless task for the medical practitioner to undertake.

The physiology and pathology of urea are very briefly stated. It is assumed to be the measure of "dissimilation," a term coined by the author to express the opposite of assimilation, but not synonymous with the common phrase "retrograde metamorphosis," inasmuch as it implies a change which may be effected in food as well as in tissue. Nothing is said of its relations to uric acid, and no positive theory of its origin is hazarded. In a subsequent chapter, on albumen, Bechamp's statement that urea is formed from albumen, by digestion in hypermanganate of potash, is repeated. Staedeler, however, has recently contested this discovery, insisting that Bechamp's urea is nothing but benzoate of potash.

For the precipitation of uric acid, nitric acid is recommended in preference to hydrochloric, on account of the greater solubility of the substance sought in the latter acid, as well as the increased tendency to the formation of confervoid growths which decompose the acid, a process entirely prevented by the use of nitric acid. For the volumetrical determination of this substance, Scholz's process with permanganate of potash is recommended. This is so uncertain, that no analytical chemist would be satisfied with a process for the determination of the commercial value of an article which was half so inaccurate. Nothing shows more conclusively the unsatisfactory condition of organic analytical chemistry than the expectation expressed by our author that two inevitable errors will balance one another. For the diagnosis of urates, a very simple table is given, perfectly unexceptionable, so long as they are found separately. The caution in reference to the necessity of dissolving the deposit in boiling water and filtering whilst hot, is necessary to be borne in mind when making these comparative tests.

The remarks on deposits of urates and of uric acid are well worthy of attention. They are very full and cover the whole ground. The precise pathological value of these sediments is carefully examined. Uric acid diathesis is rejected as a useless pathological myth, the presence of deposits being attributed to acid fermentation. The development of lactic acid by the action of a mucous ferment on the colouring matter, and the consequent decomposition of the urates, as shown so satisfactorily by Scherer, is adopted by our author, and has, indeed, gained very general acceptance from those who have studied the subject.

Under the head of creatine and creatinine we find nothing new, except some experiments to determine the quantities of these substances excreted.

In the chapter on chlorine we have some sound remarks on the errors inseparable from the method of estimating the salts by evaporation and incineration. In all organic analyses it is a growing conviction among chemists that incineration effects such changes as totally to mislead us in reference to the true constitution of their saline ingredients. Decomposition is inevitable, and no human ingenuity can rearrange the disturbed elements in their original order. In the volumetrical analysis of chlorine, preference is expressed for the method by nitrate of mercury, because the end of the precipitation is indicated by a change of colour, whereas, in the nitrate of silver process, it is only determined by the cessation of a cloudiness. This is easily obviated by adding a chrome salt to the urine, which, by the permanent crimson precipitate, indicates the moment at which all the chlorine is thrown down.

Physiologically, chlorine has a direct relation to vital activity. The greater that is, the larger is the amount of chlorine excreted by the kidneys.

Let the activity be either nervous or vascular, the result is the same. Indeed, copious draughts of water, which, in common parlance, stimulate the kidneys, produce the same result. This, however, looks to us more like a physical than a vital action. If we compare the tissues to a porous filter charged with saline matter, we shall find no cause of surprise in the fact that a large and long-continued current of water, passing through their pores, will wash out more salt than a smaller quantity occupying less time in the transit. Barral's idea of chlorides stimulating the elimination of nitrogen may possibly be accounted for in the same way, since they excite undue thirst and lead to copious draughts of liquids.

As for its pathological significance, the result of numerous observations is that chlorine is always diminished in acute febrile diseases, sometimes falling as low as one-hundredth of the normal quantity. Indeed, so constant is its relation to the pathological state of the system that it may be used as a prognostic, its decrease being an evidence of increasing morbid action, and its progressive augmentation affording a measure of convalescence.

Sulphuric acid has been observed to be increased when an exclusive meat diet has been used. There is no doubt that it arises in great part from the oxidation of albuminous materials, whether existing as tissues or introduced as food. The absence of this acid in muscle-juice seems to imply that the ultimate oxidation takes place either in the course of the circulation, or in the very act of secretion in the kidneys. Here, however, all is yet dark, and many more observations are needed to determine this question.

Phosphoric acid, as every body knows, is a constant and most important ingredient of the urine. The methods given for determining it (by acetate of soda and iron) are not satisfactory, and, indeed, at present there are difficulties surrounding every process for its estimation. The author himself acknowledges that the plan recommended is liable to an error of ten per cent. in the most careful hands, which may rise to twenty or thirty, should the operator be a little careless. Few chemists who have used it to any extent, have much partiality for it. Perhaps Kopp's plan of precipitating as yellow phosphate of uranium may succeed better. The common method of throwing it down as a double salt of ammonia and magnesia cannot be relied upon at all, when delicacy is desirable.

In speaking of the physiological signification of the acid, our author notices its relation to the ingestion of food, but is strangely silent in regard to the influence of the disintegration of nervous matter upon the quantity secreted.

The few paragraphs in which the pathological indications of albumen in the urine are indicated, are very brief but very much to the point. They contain, in a form perhaps too condensed for the ordinary student, a summary of what is known upon the subject, without any admixture of speculation. The general medical reader knows how very little that is.

There is an equal economy of language and speculation in the chapter on sugar, and, perhaps, some readers may complain of the *ex cathedra* manner in which vexed questions are decided upon. For example, Mialhe's notion of the influence of alkaline bicarbonates on the oxidation of sugar, is simply denied, without argument. With equal peremptoriness is Bernard's idea of the formation of sugar from albuminous substances in the liver disposed of; the presence of sugar in that organ, among the carnivora, being attributed to the inosite of the flesh they eat. Singularly enough, this is a question not touched upon in the controversy on glucogenia. The nearest approach to it is Lehmann's search for a glucoside which he con-

ducts with diastase and sulphuric or hydrochloric acid, neither of which acts upon inosite. In any event, however, this does not affect the opinion that the liver is the special organ for the formation of sugar; it only changes the raw material from which the glucose is to be made. Our author suggests that the milk-sugar in carnivora may be formed from inosite. The odour of diabetic urine is attributed to acetone, on the strength of the examination of a case in the hospital, at Prague.

Under the head of uroxanthine, we have great light shed upon a number of obscure cases in which a blue colouring matter, resembling indigo, has been observed in the urine. The observations of Kletzinsky and Schunck are cited to show the identity of indigo blue and indigo red with Heller's uroglaucin and urrhodin; and of indican (the substance which by oxidation yields indigo) with uroxanthine. The result appears to be that indican is an ingredient of normal urine, and the formation of indigo from it is only one of the accidents of oxidation. Virchow was able to make indigo from every specimen of concentrated urine he examined.

In treating of oxalate of lime, our author goes into a very full investigation of the crystallographic forms of that substance, and concludes that it belongs to the quadratic and not to the cubical system. He differs wholly from Golding Bird's second opinion concerning the dumb-bells, and restores them to their original position among the forms of oxalate of lime.

The book concludes with an account of urophanic substances, or those which, when introduced into the body, are expelled through the urine. There are many interesting observations in this chapter, but to discuss them would extend this notice to too great a length. A. S. P.

BIBLIOGRAPHICAL NOTICES.

Art. XVI.—*Reports of American Institutions for the Insane.*

1. *Of the New Hampshire Asylum, for the fiscal year* 1857–58.
2. *Of the Massachusetts State Hospital, Taunton, for the fiscal years* 1856 *and* 1857.
3. *Of the Retreat, at Hartford, Conn., for the fiscal year* 1857–58.
4. *Of the Bloomingdale Asylum, for the year* 1857.
5. *Of the King's County, N. Y., Asylum, for the fiscal year* 1857–58.
6. *Of the Western Pennsylvania Hospital, for the year* 1857.
7. *Of the Maryland Hospital, for the year* 1857.
8. *Of the U. S. Government Hospital, for the fiscal year* 1857–58.
9. *Of the Indiana State Hospital, for* 1857.

1. The *New Hampshire Asylum for the Insane*, during most of the fiscal year terminating with the 31st of May, 1858, was under the superintendence of Dr. Jesse P. Bancroft, the successor of Dr. Tyler. "From the manner in which he has thus far discharged his delicate and onerous trust," says the report of the trustees, "the board feel confident that, under his supervision, the asylum will continue to flourish and make advances in respectability and usefulness.

	Men.	Women.	Total.
Patients in the asylum May 31, 1857	84	86	170
Admitted in course of the year	44	32	76
Whole number	128	118	246
Discharged, including deaths	40	37	77
Remaining May 31, 1858	88	81	169
Of the patients discharged, there were cured .	17	17	34
Died	13	5	18

"Of the deaths, none have resulted from acute or epidemic disease. One occurred from apoplexy, one from epilepsy, one from consumption, two from general paralysis, and one from suicide. The others were the termination of long-continued insanity in various forms."

Aggregate of admissions since the asylum was opened . . .	1,552
Recoveries	696
Deaths	158

The legislature of New Hampshire, in a spirit worthy of imitation in other States, at its session next preceding the issue of this report, appropriated four thousand dollars for the benefit of the indigent insane at the asylum. "This has been applied," says the report of the trustees, "as directed in the act providing the same, and some one hundred and thirty-five indigent patients have, for different periods during the year, received aid therefrom to the amount of from eighty to eighty-five cents each per week. Without this timely assistance very many of them could not have remained at the institution. A larger portion of the insane, from causes readily suggested upon reflection, are in more needy circumstances than is generally supposed, and have not, either in themselves or their friends, resources more than partially sufficient for their support. From facts which have come to the knowledge of this board, they are constrained to believe that the appropriation of last year was insufficient to answer fully the purpose of its intention, and that there is now a large number of insane persons scattered through different sections of the State, and indifferently cared for, who would be sent to the asylum could they be assisted to a like extent by the State."

2. The Third Annual Report, by Dr. Choate, of the *Massachusetts State Lunatic Hospital, at Taunton*, contains the following record of the movements of the patients at that institution in the fiscal year ending November 30, 1856:—

	Men.	Women.	Total.
Patients at the beginning of the year	126	136	262
Admitted in course of the year	82	103	185
Whole number	208	239	447
Discharged, including deaths	68	81	149
Remaining at the end of the year	140	158	298
Of the patients discharged, there were cured	33	29	62
Died	14	26	40

Causes of death.—Phthisis, 11 (all females); maniacal exhaustion, 6; marasmus, 5; softening of the brain, 4; diarrhœa, 3; chronic mania, 3; dysentery, 2; apoplexy, 2; fever, epilepsy, scrofula, and suicide, 1 each.

"Twenty-nine deaths occurred this year from chronic diseases which had been operating for months, some of them for years; and were of a character necessarily fatal in their result. Of the remaining eleven, six were the consequence of that dreadful exhaustion which follows the stage of intense excitement in acute and typho-mania. But one death occurred from fever, and two from dysentery, so that on the whole we may consider that a high degree of health has existed among the inmates of the hospital during the year."

"The excess in the number of female patients, which last year amounted to eight per cent., has increased this year to more than twelve per cent., notwithstanding that the number of females discharged and died has considerably exceeded that of the males."

The extent to which foreign emigration has been directed towards the older States of New England within the last ten or fifteen years, is, perhaps, nowhere more conspicuously or accurately indicated than in the records of the institutions for the insane in that quarter of the Union. In connection with this subject we make the subjoined extracts from Dr. Choate's report.

"During the past year the proportion of foreigners admitted has decidedly increased. Previous to this year they have constituted thirty-two per cent. of the whole number; during the year just closed they have amounted to forty-one per cent. * * * In view of the very large number of foreign insane now supported by the State, and of the evils consequent upon their mixing up in the hospitals with our own people, it seems to be worthy of consideration whether, when the new hospital at Northampton is opened, some plan cannot be devised by which a separation may be effected, based upon this distinction. Under the present arrangement the most serious difficulty to be overcome in this and all other State hospitals with which I am acquainted, is that connected with social classification. * * * We have in the State hospitals people from every walk of life, many of them cultivated and refined, and the associations to which such are necessarily sometimes subjected, are neither agreeable nor useful. The foreign insane have different habits of life, different wants, need different management, and have a much smaller chance of recovery. It is an established rule that the provision made for insane people should be made to conform, as far as possible, to that to which they have been accustomed, and which they would have chosen were they at liberty. In this point of view the native and foreign insane need very different provision in many respects."

"A large portion of the Irish patients, who are nearly all supported by the State, slide after a while, if they do not recover, into an exceedingly sluggish and passive state of dementia, when they are not susceptible of remedial treatment, and can many of them be taken care of with perfect safety and propriety in some ward of the State almshouses devoted to the purpose, and under the care of a special competent attendant."

The most serious, humiliating, and revolting abuse to which the insane are still subjected, is their compulsory association, in many of our State hospitals, with deranged criminals. The old notions of essential affiliation between jails and receptacles for the insane, and of identity of management of the two classes of persons for whom those establishments are created, have been very much

modified within the last twenty years, but they have not, hitherto, been so far destroyed as to enforce a general practical illustration of their destruction.

If the felon be rejected from domestic social intercourse, from the school, the church, the asylum for the deaf, the asylum for the blind, and the general hospital, wherefore, in the name of reason, of propriety, of justice, and of humanity, should he be received into the establishment devoted to the treatment of mental disorders? In all the places of intercourse for the sane, the burglar and the homicide are discarded; but the institution from a liability to the kind and curative offices of which no one of us can claim exemption, still opens its doors to them, still intermixes them with the innocent, the educated, the refined.

With a state of public sentiment such as it ever has been in regard to hospitals for the insane, and such as, to a very great extent, it unfortunately and unjustly continues to be, the thought, to a sensitive mind, of becoming a patient in one of those establishments is of itself sufficiently painful and humiliating, without aggravation by the fact that the position will bring with it inevitable daily intercourse with persons under the ban of the law, and generally regarded as outcasts from society.

These remarks were suggested by a section of Dr. Choate's report, from which a portion is here extracted.

"During the past year two convicted felons have been sent to us from the houses of correction. Both of these men are of the worst and most dangerous class of criminals. They were sent to us, not with the expectation that they could be cured of their insanity, but because they were feared in the prisons. Besides these, two men sent the previous year from the State prison, one of them convicted of manslaughter, the other of highway robbery, still remain with us. * * * Their influence upon the other patients is in all respects bad. Their language is low and profane, and abusive of the officers and attendants. They need a different mode of treatment from the other inmates, which introduces a disturbance in carrying out the regulations of the institution. * * * The security of society, the safety and good of the insane, and the reputation of our lunatic hospital, imperatively demand that they should be kept elsewhere."

Report for 1857.

	Men.	Women.	Total.
Patients in the hospital Nov. 30, 1856	140	158	298
Admitted in ten months	123	84	207
Whole number	263	242	505
Discharged, including deaths	86	92	178
Remaining, September 30, 1857[1]	177	150	327
Of those discharged, there were cured	45	37	82
Died	17	26	43

Deaths from phthisis, 17; diarrhœa, 6; softening of the brain, 5; maniacal exhaustion, 2; fever, 2; disease of the heart, 2; marasmus, 2; chronic mania, 2; dysentery, apoplexy, disease of the liver, paralysis, and suicide, 1 each.

"Twenty-eight per cent. of the whole number of deaths since the opening of the hospital have been from phthisis, of which disease five males and thirty-four females have died. It is hard to account for this difference between the sexes; it is, probably, not accidental. Males, on the other hand, appear to be much more subject to a fatal issue from disease, particularly softening of the brain. Fifty-one per cent. of the deaths were from causes directly connected with the mental disturbance."

"The general health of the inmates of the house, throughout the year, has been good. Typhoid fever, generally of a mild form, prevailed to a limited extent during the winter and spring, and two deaths occurred from that cause. During the summer we have had an almost entire exemption from those acute affections of the bowels which usually prevail at that time. But three deaths from acute diseases have occurred during the year. * * * Next to this (phthisis), as a cause (of death), ranks that chronic diarrhœa which is so fre-

[1] It appears that the termination of the fiscal year has been changed from Nov. 30, to Sept. 30. The term embraced by this table is, therefore, only ten months.

quent and troublesome in old cases of dementia. This arises partly, probably, from the effect upon the digestive organs of the impairment of the nervous energy and vital force; partly from the manner of eating of such patients; and in some cases is undoubtedly aggravated, and even induced, by their habits of introducing into their stomach a variety of filthy and indigestible substances. * * * The patients received from Boston are mostly Irish. As a nation, at least as we see them here, they do not bear disease well. They have not that vital force which offers resistance to the last to the assaults of a deadly disease. They have not that *vis medicatrix naturæ*, which is the great healing power, so well developed as our own native population. Their previous mode of life, a life of excess, poverty, and exposure, is sufficient to account for this. Add to this, that the greater part of them are intemperate, which in a large number is the actual cause of their coming here, and we shall see abundant reason why more of them should die, and fewer recover, than among any other class."

"In previous years it has been our experience, as it has, I believe, been that of every other hospital in New England at least, to have the larger number of females; but, during the year past, the relative proportions of the sexes have been at once and decidedly reversed."

"Ten convicts, three of them from the State prison, and seven from the houses of correction, are now inmates of the institution. * * * One of them, during the past year, made a murderous assault upon an attendant, and very nearly effected his escape. He subsequently confessed that his object was murder, and through that, liberty. Another is an incendiary, much feared in this section of the State, and who ought to be kept in a place of the most perfect security. A third is a murderer of the most dangerous character, now under a sentence of twenty years' imprisonment for killing his wife. A fourth, a burglar by profession, attempted murder in the house of correction, before being sent here. * * * It is only the *dangerous* insane convicts who are sent from the places of confinement and correction to the hospitals. These are almost uniformly incurable cases. Since the opening of this hospital no convict sent from the State prison has recovered, and but two sent from the jails and houses of correction."

	Men.	Women.	Total.
Patients admitted since the opening of the asylum	444	445	889
Discharged, recovered	127	123	250
Died	62	83	145
Married	181	194	375
Unmarried	247	197	444
Widowed	16	54	70
From 20 to 30 years of age when admitted . .	118	129	247
From 30 to 40 " " . .	112	126	238
From 20 to 30 years of age when first attacked .	146	149	295
From 30 to 40 " " .	113	112	225

The following singular case is recorded in Dr. Choate's report for 1855. It was overlooked at the time that report was reviewed.

"One case occurred after the bite of a rabid cat. The patient, a lad of sixteen, in perfect, even redundant health, not known to have any hereditary predisposition to insanity, the victim of no evil habits, and of quiet, simple, frank disposition, though rather nervous and susceptible, was bitten by a furious cat, and almost immediately became insane. His case has been one of periodical mania. Subject to attacks of great violence, which continue two or three days, he is in the intervals calm, quiet, and nearly rational, except for a fixed delusion that he resembles, in the form of his face, hair, and some other respects, a beast. His case is now of several months' standing, and does not offer at present a prospect of complete recovery. The bite, which was a severe one upon the thumb, healed kindly."

A recent visit to Taunton afforded us the gratification of accompanying Dr. Choate through the several departments of this hospital. We had never previously seen it; and as much has been said of the imperfections of its original construction, our conception of its claims to commendation, we rejoice to acknowledge, was not commensurate with its actual merits. The apartments of the better classes of patients are handsomely furnished, and ornamented with pic-

tures; neatness and good order prevailed throughout the entire establishment; and, irrespective of the cultivation of its grounds, which time and labour are rapidly perfecting, and aside from some radical defects of construction, the institution may be classed among the best of those which have been founded by the several States. Of the original faults of construction, that of numerous "strong rooms" has been removed by their demolition and the conversion of the space which they occupied into ordinary dormitories. The imperfections in heating and ventilation have been overcome. One grand defect, architectural in its nature and consequently not of easy removal, yet remains. The hospital was erected about the time in which associated dormitories were most *à la mode;* and this fashion, as in the case of many others, was carried to an extreme. We have never believed that the welfare of the patients in an institution like this can be promoted by increasing the number in associated dormitories above from twenty-five to thirty-three per cent. of the whole. But, in his report for 1856, Dr. Choate states that there were 158 patients in the department for females, while the number of single sleeping rooms is only fifty-six. No vigilance of attendants can entirely overcome the difficulties and the annoyances, not to say the dangers, of such enlarged association.

3. The professional banquet to which Dr. Butler, of the *Hartford Retreat,* this year invites us, is mostly on the tables.

	Men.	Women.	Total.
Patients in the Retreat, April 1st, 1857	99	107	206
Admitted in course of the year	67	77	144
Whole number	166	184	350
Discharged, including deaths	64	78	142
Remaining, April 1st, 1858	102	106	208
Whole number of cases admitted since 1824	1545	1721	3266
Discharged, recovered, "			1582
Died "			337
Whole number of persons admitted "	1025	1125	2150
Of these there were admitted a second time	161	152	313
Of the 313, there were admitted a 3d time	30	46	76
Of the 76, " " 4th "	13	16	29
Of the 29, " " 5th "	5	10	15
Of the 15, " " 6th "	4	4	8
Of the 8, " " 7th "	1	1	2
Of the 2 there was admitted 9 times		1	1
Between 20 and 30 years old on admission, in 14 years	207	296	503
Between 30 and 40 " " "	217	261	478
Between 20 and 30 years old at 1st attack, 13 years	247	369	616
Between 30 and 40 " " "	158	178	336
Married, of those admitted the last 15 years	398	516	914
Unmarried, " " "	550	519	1069
Widowed, " " "	47	159	206

Causes of death this year.—Exhaustion, 4; consumption, 2; diarrhœa, 2; marasmus, general debility, catalepsy, disease of stomach, suicide, epilepsy, and disease of brain, 1 each.

In the course of the year, a second story was added to the building for excitable females which was erected three years previously.

"There are many objections," says Dr. Butler, "as well as an obvious impropriety in designating as 'lodges,' or 'strong rooms,' any department of a lunatic hospital,[1] especially where, as in our case, these rooms, which have at large expense been constructed for the noisy and excitable, are confessedly among the most pleasant and cheerful of the institution. Strange as it may seem, there still lingers in the public mind an impression that there is attached to this, as well as to all other lunatic hospitals, a class of rooms for violent patients, akin to the old-fashioned dungeon in their construction and arrangements. We learn

[1] What does the doctor think of the term "*lunatic* hospital?"

this from the extraordinary inquiries occasionally made of us. These apartments are a conclusive answer. The comfort and enjoyment afforded by them, the respect paid to the pictures, &c. &c., by which they are ornamented, with the general effect that the whole arrangements produce in the minds of all the patients, especially of the convalescent, are a satisfactory comment upon the wisdom and liberality of their construction."

4. Dr. Brown, in his report for 1857, gives the following numerical results of the operations of the *Bloomingdale Asylum*:—

	Men.	Women.	Total.
Patients in the asylum January 1st	64	80	144
Admitted in course of the year	65	63	128
Whole number " "	129	143	272
Discharged, including deaths	62	64	126
Remaining, December 31st	67	79	146
Of the patients discharged, there were cured	29	20	49
Died	7	8	15

Causes of death.—General paralysis, 5; pulmonary consumption, 4; puerperal mania, 2; apoplexy, 1; chronic disease of the brain, 1; gradual exhaustion and abstinence, 1; erysipelas of the head, 1. The patient last alluded to "had been a resident in the asylum thirty-six years, having until her short and fatal illness, enjoyed a remarkable exemption from physical disease."

5. Dr. Lansing, who had been for some months connected, as its Resident Physician, with the *King's County* (N. Y.) *Lunatic Asylum*, withdrew from that institution, and was succeeded, on the 7th of May, 1858, by Dr. Edward R. Chapin, the author of the report now before us.

	Men.	Women.	Total.
Patients in the asylum July 31st, 1857	97	141	238
Admitted in the course of the year	86	115	201
Whole number " "	183	256	439
Discharged, including deaths	75	96	171
Remaining July 31, 1858	108	160	268
Of the patients discharged there were cured	34	42	76
Died	14	13	27

Causes of death.—Phthisis, 9; meningitis, 2; apoplexy, 2; old age, 2; erysipelas, dysentery, cerebritis, hemiplegia, atrophy of brain, softening of brain, atheromatous disease, general paralysis, epithelial cancer, chronic diarrhœa, hematuria, and dropsy, 1 each.

Of the 268 patients remaining in the asylum at the close of the year, 187 were foreigners, and 81 natives of the United States. Of the foreigners, 139 were from Ireland, 26 from Germany, and 18 from England.

The number of patients at this institution has been gradually increasing for several years.

6. From the report of Dr. Read, of the Department for the Insane in the *Western Pennsylvania Hospital*, we take the following statistical items for the year 1857.

	Men.	Women.	Total.
Patients at the beginning of the year	14	9	23
Admitted in course of the year	53	34	87
Whole number " "	67	43	110
Discharged, including deaths	19	17	36
Remaining at the end of the year	48	26	74
Of the patients discharged, there were cured			23
Died			6

"Four of those who died were residents of the hospital for periods varying from six to sixteen days; and two, less than two months.

"The health of our household has been eminently good. We have enjoyed a singular exemption from sickness, but one patient having been prostrated by disease induced after admission.

"The number of patients has largely increased during the past year. The ward recently constructed was soon occupied, and all others have been filled to their utmost limits. * * * * In Western Pennsylvania there are over twelve hundred insane, and not two hundred enjoying hospital care; the remaining thousand drag out miserable lives at home, or in loathsome cells of jails and poor-houses, under the care of unpaid or reluctant nurses. The other hospitals in the State are filled; and to the enlargement of this building or the erection of another, can these unfortunate men and women only look for alleviation of the evils and miseries they are compelled to endure."

In our notice of a former report from this institution, it was mentioned that a farm upon the Monongahela River had been purchased, with the intention of erecting thereupon a distinct and complete establishment for the insane, as a substitute for the apartments now occupied in the General Hospital. Since that time, the Board of Managers have determined to obtain another site, less secluded and more easily accessible by railroad. The construction of the new buildings is consequently postponed. Improvements, meanwhile, are constantly being made in the premises now occupied, rendering them more comfortable to their inmates, and enlarging the resources for the moral treatment of the patients. A library has been established and has already been increased, by donations, to several hundreds of volumes. Musical entertainments have been given; and religious exercises upon the Sabbath have been instituted. "More than one-half the female patients have been induced to occupy their time in reading, sewing, knitting, fancy-work, or assisting the attendants in the household duties of the wards. * * * In their apartments, cheerfulness and industry have taken the place of the gloom and idleness that formerly prevailed." A less encouraging account is given of the men: and such must the account probably continue to be, until they shall have been removed to an establishment connected with a farm, upon which they can labour. "The occupation of the male patients," continues the report, "is limited almost entirely to reading and assisting the attendants in their labours. We find it impossible to prevent those who have nothing to do, or to interest them, from falling into careless habits and vicious practices; and we hope provision will be made for the occupation and amusement of this class of patients, and feel satisfied that money thus expended would bring a good return. All would be rendered more comfortable; health would be improved; their irritability expended in useful exercise; they would be more easily managed; an air of contentment would pervade their apartments, and our efforts for their care and restoration would be much more efficient and valuable."

7. The numerical records for the *Maryland Hospital for the Insane*, from the 1st of January to the 30th of November, 1857, are as follows:—

	Men.	Women.	Total.
Patients in the hospital Jan'y 1st	54	56	110
Admitted in the eleven months	26	17	43
Whole number	80	73	153
Discharged, including deaths	24	21	45
Remaining, Nov. 30	56	52	108
Of those discharged, there were cured . . .	5	7	12
Died	4	2	6

Besides the above, twelve cases of mania-à-potu were under care. Ten of them were discharged, recovered; and two remained in the hospital. Patients with this disorder have not been received previously to this year.

"Two maladies, diarrhœa and dysentery," remarks Dr. Fonerden, "have usually required much medical attendance in the summer and fall. But this year no cases of either have been violent or of long duration. There have been fewer inmates with wornout constitutions than we have had in some previous years. Partly for this reason the health of the institution has been better than ever before."

A very large proportion of the patients in this hospital are permanent residents, with chronic and incurable mental disorder. No less than forty-three of them were admitted prior to the year 1850.

8. The report of the *Government Hospital for the Insane*, for the fiscal year ending June 30, 1857, is not published separately, but is included in the documents accompanying the message of the President of the United States; being introduced as a part of the report of the Secretary of the Interior, under whose official surveillance the institution is placed.

	Men.	Women.	Total.
Patients in the hospital July 1, 1856	54	39	93
Admitted in course of the year	25	27	52
Whole number	79	66	145
Discharged, including deaths	19	16	35
Remaining, June 30, 1857	60	50	110
Of the patients discharged, there were cured	2	5	7
Died	8	8	16

Instead of giving the causes of death in the usual manner, Dr. Nichols arranges the fatal cases in a table, which, from its originality, deserves a transfer to our columns.

"Chronic, organic, and functional degeneration of the brain, irregular in character and extent	9 cases.
Same, with phthisis	2 "
Same, with paralysie générale	1 case.
Same, with menorrhagia	1 "
Same, with hemiplegia	1 "
Same, with epilepsy	1 "
Same, with pneumonia	1 case."

The few recoveries relative to the whole number of patients is easily explained. But about one-third of the hospital building has been completed. The apartments in this are barely sufficient comfortably to accommodate the old cases of dementia which, belonging to the army, the navy, and the indigent classes in the District of Columbia, have been removed to this institution from the various receptacles in which they were placed prior to its establishment. All the patients who died were demented. Only one of them had been insane less than two years; and the average duration of their mental disorder was upwards of nine years. Of the fifty-two patients admitted in the course of the year, the insanity of but twenty of them had existed a less term than twelve months; while that of the remaining thirty-two, varying from one year to fifty years in duration, made an average of more than eight years.

"The fatal case of pneumonia, herein reported, is the only acute disease that has ever occurred among the patients; and not only have the inmates enjoyed an exemption from grave physical disorders, with the exception named, but other instances of indisposition have been trifling and infrequent."

"No suicide nor other serious accident of violence has occurred since the hospital was opened for the reception of patients, in January, 1855, though the household has never been without its suicidal members, and the facilities for violence, both to one's self and to others, have always been extraordinary, owing to the work of building and other improvements, which has never been wholly suspended."

The progress made in the erection of those portions of the hospital edifice which have been commenced, is indicated by the following extract: "If the remainder of the building season (in 1857) should be mild, the three sections of the wings now under way will be roofed in before its close, and the walls of the centre building so far advanced that they can be finished and covered, in the spring, before the plasterers have completed their work in the wings." A mechanic's shop, and a substantial and convenient brick stable, arranged according to the most approved method, have been completed.

In these times, when, in some of the States, we have seen the interests of

benevolent and charitable institutions either utterly neglected, or rendered subordinate to the capricious policy of political partisans, it is especially gratifying to perceive that this hospital, the representative of our federal philanthropy, has, from its very inception, been sustained and fostered, with undeviating interest and an effective and enlightened zeal, by the Department of the Interior.

9. It becomes our duty to chronicle another case in which the baneful influence of political strife has extended to some of the benevolent institutions of our country. A flourishing institution for the insane, when in the "full tide of successful experiment," protecting and ministering to the shattered health of about three hundred patients, has been vacated—that is the word, strange and novel though it be in such a connection —vacated by the removal, *en masse*, of its invalid inmates, and that, too, at a time when the coffers of the State Treasury were overflowing with funds collected expressly for their support by the taxation of a people willing thus to contribute to the alleviation of the sufferings of their fellow-men.

"The Constitution of Indiana," say the Commissioners of the Hospital for the Insane in the State mentioned, "provides that the legislature shall provide for the support of her benevolent institutions, and that no money shall be drawn from the treasury but under appropriations made by law."

Owing to a political disagreement between the two branches of the legislature, the appropriation bill which passed one house, was lost in the other; and the General Assembly adjourned leaving the benevolent institutions without funds for the current expenses of the year.

In this dilemma, the Commissioners of the Hospital for the Insane, of the Asylum for the Blind, and of the Asylum for Deaf-mutes, twice petitioned the Governor of the State to call an extra session of the legislature, in the hope that the impending evil might thus be averted. Their petition was not granted. Being prohibited by statute law from borrowing money for the support of the institutions under their charge, but one resource was left to these commissioners. They must STOP. Accordingly, the patients of the establishment first-mentioned, and the pupils of the other two, were summarily discharged in April, 1857.

"The greater number of the patients discharged from the hospital," says the superintendent, Dr. Athon, "were placed in the poor-houses of the several counties; the remainder were confined in jails and out-houses—all disposed of without any reference to their treatment and cure, but merely for confinement and support."

It is no wonder, that "the return of so many insane persons, in so short a period, produced a profound sensation in every part of the State, filling the hearts of many with consternation and alarm." Nor is it surprising that, under such circumstances, "a few of the boards doing county business took up the subject and proposed to the officers of the hospital to return their patients and support them there at the expense of the counties" respectively.

The order for the evacuation of the hospital was issued upon the 3d of April. On the 27th of the same month the commissioners met and resolved that patients, either private or from the counties, might be received, provided they were sent in conformity with the law, and that they should pay all expenses incurred by the institution beyond that which it would cost to take care of the establishment without any patients. "This experiment was fairly and faithfully tried for five months, and in the most favourable part of the year, and, we may safely say, utterly failed to satisfy either the officers of the hospital or the public." The average number of patients during its operation was about thirty.

At length, in September, at "a council of State officers, it was resolved to furnish money from the State Treasury to open up and carry on all the State benevolent institutions;" and in pursuance of this resolution, on the 22d of the same month, the president, *pro tempore*, of the hospital issued a notice that from and after the fifth day of October next ensuing, it would be "opened for the reception of patients according to law."

We rejoice in the assurance which comes to us from Indiana, that the course

pursued by the legislature, or by a portion of its members, meets with no sympathy or commendation among the people of that State.

We quote the annual registration mostly in the language of the report—

	Men.	Women.	Total.
Patients in the hospital October 31, 1856	114	121	235
Admitted in the course of the year	82	78	160
Whole number	196	199	295
Discharged (of admissions during former years) officially, during the month of April			229
By death (of admissions during former years)	3	3	6
Discharged (of admissions during the present year) officially, during the month of April			74
By death (of admissions during this year)	2	0	2
Discharged at the close of this year	8	2	10
Whole number discharged during the year	167	157	321
Remaining in the hospital October 31, 1857	29	42	71
Of the patients discharged, there were cured	29	20	49
Patients admitted from the opening of the hospital in 1849	625	615	1,240
Discharged recovered	326	317	643
Died			108

Causes of death this year.—"Maniacal exhaustion, chronic arachnitis, epilepsy, old age and debility, general paralysis, chronic diarrhœa, congestive pneumonia, œsophagitis," 1 each.

In reference to causes of mental disorder, Dr. Athon makes the following remarks:—

"The insatiable desire to accumulate property, which is the characteristic of the age, is the most fruitful source of insanity. The passion for gain is contagious, and although a majority of people are attacked by and survive the malady, not a few succumb to its mental ravages; and although the table of causes of insanity accompanying this report does not seem to bear us out in our opinion, yet we think there is indubitable evidence that the causes assigned for the insanity of patients are at least one-half incorrect, and ought to be put down '*over-desire to accumulate property.*' The table referred to is made up according to causes assigned by the friends or physicians of the patients, and not by us."

Since this report was written the number of patients has increased to two hundred and seventy-seven. P. E.

ART. XVII.—*Brief Expositions of Rational Medicine, to which is prefixed The Paradise of Doctors, a Fable.* By JACOB BIGELOW, M. D., late President of the Massachusetts Medical Society, Physician of the Massachusetts General Hospital, &c. Boston: Phillips, Sampson & Co., 1858. 12mo. pp. 70.

THIS is a small book, but it contains more than some medical books which are larger. It is well known that its author has been prominent among those who have thought that there was much overdosing in the profession, and that there was great need of a reform in this particular. The first proclamation of his sentiments was made in 1835, in his essay on Self-limited Diseases. By a large proportion of the profession he was regarded at that time as being unwarrantably sceptical in relation to the powers of medicine in curing disease; but there were many who agreed with him, and who were pushing their inquiries in the same direction. Indeed, we may say that many years before this, there were medical men here and there, who were led, by the discrepancies between the statements of those who advocated opposite modes of practice, to question whether the favourable results boasted of in these statements were not to be attributed more to nature's salutary efforts than to medicine. So far were many

led in the right path by this questioning, that the sentiments of Dr. Bigelow's essay were fully responded to by no small portion of the profession, and at the present time they are the sentiments of almost all truly intelligent medical men. It is true that some physicians of high respectability make objections to them; but it is only from a misapprehension of Dr. Bigelow's meaning. They suppose him to go much further than he really does, ascribing an ultraism to him that is not his due.

The main part of the book is prefaced by a fable, entitled the Paradise of Doctors, which was read at the last annual dinner of the Massachusetts Medical Society. The fable begins thus: "It happened, once, that a general awakening took place among the physicians, druggists, and citizens of the quiet old State of Massachusetts, during which it was discovered that a great and culpable neglect had long been prevalent throughout the community in regard to the important duty of taking physic. A conviction fell upon all that it was now imperatively necessary that every man, woman, and child, should proceed at once and habitually, in sickness and in health, to take three times as much medicine as they had taken before." The results of this great propensity to physic are very humorously set forth. Among other things it is said, "Clergymen and moralists forgot that men were sinful; it was quite enough that they were bilious. Bile was regarded as the innate and original sin, which was to be extirpated with fire and physic even from the newly-born child. Nobody was aware that bile is necessary to life; no two persons were agreed as to what the term *bilious* meant; it was something insidious, mysterious, and awful. Some held that it consisted in having too much bile; others in having too little. According to some, the bile was held back in the blood; according to others, it was absorbed ready formed into the blood. Fierce schisms and sects were generated on the question who, and whether any, were exempt from its contaminating presence. The bon vivant, after his night's carouse, furnished abundant demonstrations of its existence on the following morning. A healthy labourer, who had had the temerity to boast of his freedom from bilious taint or suspicion, was convicted and brought to his senses by the ordeal of a dozen grains of tartar emetic." The trade of physic was so lively, that doctors and druggists poured into the Bay State from other quarters, and the trade was overdone. This, together with the manifest effect upon the general health of the community, began to produce some doubt about the paramount duty of physic-taking. But "nevertheless," the fable goes on, "weak-minded men and strong-minded women failed not to harangue audiences in the streets on the astonishing powers of medicine. Spirit-rappers were summoned to evoke from their rest the heroic shades of Rush and Bouillaud, Sangrado, Morrison, and Brandreth. These distinguished worthies exhorted their followers not to shrink or falter under the trials to which they were subjected, but rather to redouble their perseverance, until the truth of the faith which they held should be established by the testimony of their martyrdom in its cause."

But at last there came an end to all this in this wise: "A meeting accidentally took place between two old shipmasters, one of whom had lost overboard his barrel of beef, and the other his medicine-chest, in a gale of wind at the commencement of their passage. On examination and comparison of their respective crews, the contrast was so marked between the ruddy faces of the latter, and the lantern-jaws of the former, that a general mutiny sprang up in both crews against the further tolerance of the physic-taking part of their duty. The contagious insurrection spread from Fort Hill to Copp's Hill; and on the following night several medicine-chests were thrown overboard by men in the disguise of South-sea Islanders." The result was that the people were aroused, and a mass meeting was called on Boston common. Many speeches were made, and very discrepant opinions were advanced. After a while "an old lady, whose shrill voice drew immediate attention, protested against violent measures of all kinds, and moved, as a middle course, that resort should be had to homœopathy. It never did any harm, and was very comforting, especially when well recommended by the physician. It cured her child of the measles in six weeks, and herself of a broken leg in six months, during which time she had two hundred and ninety-five visits, and took more than two hundred globules. She had walked to the

meeting on her crutches to exhibit to the assembly the astonishing powers of the Hahnemannic system. Here she was interrupted by a bluff marketer, who somewhat rudely pronounced homœopathy to be a great humbug, since, but a short time before, his child had eaten part of a raw pumpkin, and was seized with convulsions; and the physician who was sent for, instead of taking measures to dislodge the offending cause, took out a little book, and remarking to the bystanders that 'like cures like,' proceeded to prescribe the hundred millionth part of another pumpkin. The next person who rose was a manufacturer, who had calculated that the homœopathic profit on the cost of the raw material was altogether unreasonable. He had himself expended seventy-five dollars in a quarter of a grain of belladonna, so divided as to keep off scarlet fever; but found, after all, that he had not bought enough, for his children had the disease a little worse than any of their neighbours."

At last an old gentleman started the inquiry whether there was no such thing as rational medicine, and whether nothing could be made acceptable to the public but extremes of absurdity. And he went on to make a very sensible speech, which took well with the meeting. Resolutions were thereupon adopted "to the effect that it was unbecoming a free and enlightened people to be drug-ridden or globule-ridden, and recommending recourse to temperance, exercise, regularity, and rational medicine, whenever it happened that medical treatment was necessary."

The meeting dispersed, and its results were immediately seen. Drugs fell in price and provisions advanced. The doctors and druggists that came from other quarters left. "Faces assumed a more vigorous and healthy aspect, and the country once more resounded with the music of the axe and the hammer, and the cheerful rattling of knives and forks. Steam engines, which had been erected for the pulverization of drugs, were attached to saw-mills and spinning-jennies. Last of all, a noble and useful art, which had long been depressed under the effects of its own exaggeration, was enabled once more to raise its respectable head, and to regain the confidence of society, under the name of RATIONAL MEDICINE."

After this amusing Preface, which the author, in his dedication to Sir John Forbes, says is introduced because "the public ear is sometimes attracted by exaggeration to give its attention afterwards to more chastened expositions of the truth," he proceeds to the subject matter before him. After some brief remarks on the tendency to ultraism, which appears in practical medicine as well as on other subjects that are agitated in the community, he says: "The methods which, at the present day, are most prevalent in civilized countries, in the treatment of disease, may be denominated the following:—

"1. The *Artificial* method, which, when carried to excess, is commonly termed heroic, and which consists in reliance on artificial remedies, usually of an active character, in the expectation that they will of themselves remove diseases.

"2. The *Expectant* method. This consists simply in non-interference, leaving the chance of recovery to the powers of nature, uninfluenced by interpositions of art.

"3. The *Homœopathic* method. This is a counterfeit of the last, and consists in leaving the case to nature, while the patient is amused with nominal and nugatory remedies.

"4. The *Exclusive* method, which applies one remedy to all diseases, or to a majority of diseases. This head includes hydropathy, also the use of various mineral waters, electrical establishments, etc. Drugs newly introduced, and especially secret medicines, frequently boast this universality of application.

"5. The *Rational* method. This recognizes nature as the great agent in the cure of diseases, and employs art as an auxiliary, to be resorted to when useful or necessary, and avoided when prejudicial."

Our author comments briefly on each of these methods. The *artificial* method relies little upon nature and almost wholly upon art. When pushed to a high degree it is "heroic" practice, as it is termed. We think that Dr. Bigelow rather overestimates the present prevalence of this method. He commits in some measure the same error that Sir John Forbes did when he said, *things have arrived at such a pitch that they cannot be worse; they must mend or end.*

He says, for example, that "the present period appears to be more marked than preceding ones by the opposite methods of treatment pursued by medical men in the management of disease." We do not believe this to be true. We are at the present time in a transition period, immediately succeeding one which was marked by warm strife between the advocates of "opposite methods of treatment." This strife is now vastly diminished, *modes* of practice have begun to pass away, the propriety of rigid discrimination in therapeutics is extensively recognized, and the sentiments which Dr. Bigelow and others have promulgated are thoroughly leavening the medical public.

But although this decided advance has been made, there is still much of undiscriminating overdosing in the profession. As Dr. Bigelow remarks, "a considerable amount of violent practice is still maintained by routine physicians, who, without going deeply into the true nature or exigencies of the case before them, assume the general ground that nothing is dangerous but neglect. Edge-tools are brought into use as if they could never be anything more than harmless playthings. It is thought allowable to harass the patient with daily and opposite prescriptions; to try, to abandon, to re-enforce, or to reverse; to blow hot and cold on successive days; but never to let the patient alone, nor to intrust his case to the quiet guidance of nature. Consulting physicians frequently and painfully witness the gratuitous suffering, the continued nausea, the prostration of strength, the prevention of appetite, the stupefaction of the senses, and the wearisome days and nights, which would never have occurred had there been no such thing as officious medication."

We are glad to see that our author speaks out plainly in regard to "interested specialists, who inflict severe discipline and levy immense contributions on credulous persons." Those regular physicians, who in great numbers, both in city and in country, make a hobby of either swabbing the throat or cauterizing the os uteri, do effectual service in opposing the progress of rational medicine, and in upholding the dominion of quackery. It would be well for the interests of medical science and of humanity if they should leave the profession, and consort with the quacks where they rightly belong.

In remarking upon the *expectant* mode, Dr. Bigelow says: "I sincerely believe that the unbiased opinion of most medical men of sound judgment and long experience is made up, that the amount of death and disaster in the world would be less, if all disease were left to itself, than it now is under the multiform, reckless, and contradictory modes of practice, good and bad, with which practitioners of adverse denominations carry on their differences at the expense of their patients." If he mean that it would be better on the whole to leave disease to nature than to subject it to medicine as it is *now* administered by the profession at large, we dissent. But we do not believe that this was true of medicine in the first quarter of this century when the "heroic" practice was everywhere in high favour. Then the profession *as a whole*, we have no doubt, did more harm than good; and we think it was good advice which was given then by a physician to a gentleman who was about to travel extensively, and who asked him what he had better do if taken sick in a place where he knew nothing of the character of the physicians, viz., that he had better keep still, diet, and leave himself to nature, rather than hazard the committal of his case to a stranger.

Dr. Bigelow's remarks on the *rational* method are exceedingly interesting, but they are not sufficiently extended. He deals too much in generals. If he had descended more to particulars, and given us a little in detail the results of his experience as to the limits of the power of medicine in the treatment of disease, he would have done a greater service to the profession than he has now done. As it is, the suggestions which he has made, followed out strictly by a multitude of practitioners, with full records of their individual experience, would largely sift the supposed therapeutical acquisitions of the profession, and at the same time add greatly to the real resources of our art. On this sifting of our therapeutics Dr. Bigelow thus justly remarks: "It is the part of rational medicine to require evidence for what it admits and believes. The cumbrous fabric now called therapeutic science is, in a great measure, built up on the imperfect testimony of credulous, hasty, prejudiced, or incompetent witnesses, such as have afforded authorities for books like Murray's Apparatus Medicaminum, and Hah-

nemann's Organon. The enormous polypharmacy of modern times is an excrescence on science, unsupported by any evidence of necessity or fitness, and of which the more complicated formulas are so arbitrary and useless, that, if by any chance they should be forgotten, not one in a hundred of them would ever be re-invented. And as to the chronicles of cure of diseases that are not yet known to be curable, they are written, not in the pages of philosophic observers, but in the tomes of compilers, the aspirations of journalists, and the columns of advertisers."

There is one very important point which is left in the dark in Dr. Bigelow's book, simply because he is so brief. We refer to the relation of what is ordinarily reckoned as palliative treatment, and so called by Dr. Bigelow, to the cure of disease. He states the objects of medical practice to be, "1. The cure of certain diseases. 2. The relief or palliation of all diseases. 3. The safe conduct of the sick." Now palliation or relief in the large majority of cases in which recovery takes place, has more to do with the recovery than any of those means which are considered as positively curative. Indeed in many the recovery is dependent upon palliation and relief, it being in such cases absolutely necessary to the successful issue of nature's efforts to relieve the suffering and quiet the disturbance of disease. In every such case the physician as really cures the disease as when he applies medicines that are positively and directly curative.

Our author believes that medical practice ought, so far as it can be, to be laid open to the public, instead of being wrapped in more mystery than belongs to it, and asserting claims which are entirely unwarrantable. On this point he thus remarks: "It is the part of rational medicine to enlighten the public and the profession in regard to the true powers of the healing art. The community require to be undeceived and re-educated, so far as to know what is true and trustworthy from what is gratuitous, unfounded, and fallacious. And the profession themselves will proceed with confidence, self-approval and success, in proportion as they shall have informed mankind on these important subjects. The exaggerated impressions now prevalent in the world, in regard to the powers of medicine, serve only to keep the profession and the public in a false position, to encourage imposture, to augment the number of candidates struggling for employment, to burden and disappoint the community already overtaxed, to lower the standard of professional character, and raise empirics to the level of honest and enlightened physicians." W. H.

ART. XVIII.—*Practical Dissections.* By RICHARD M. HODGES, M. D. Demonstrator of Anatomy in the Medical Department of Harvard University. 1 vol. 12mo., pp. 254. John Bartlett: Cambridge, 1858.

WE have presented to us in this little book, a new American dissector's guide. It comes in a style somewhat out of the ordinary way, being printed on tinted paper and bound in marbled covers, giving it rather a "holiday aspect." These peculiarities, at first sight attractive, are, we think, of positive disadvantage, since being intended for use in the dissecting-room, its covers should be made of the least absorbent and perishable materials; and further, as during the winter the greatest number of dissections are invariably made at night, the want of contrast between the ink and the tinted paper, makes the perusal rather painful than pleasant; besides which the book soils just as readily as if the pages were white.

The *practical directions* are set up in entirely too small type.

So much for the general appearance of the work, which we presume is chargeable to the taste of the publisher.

In regard to its intrinsic merits, we are sorry to say that we do not find as much to commend as we should like. Our author in his preface quotes, thus "the smallness of the size of a book is always its own recommendation, as on the

contrary the largeness of a book is its own disadvantage as well as the terror of learning." And he certainly has been fully impressed with the correctness of this very erroneous idea. We, on the contrary, maintain that no book is too large which properly investigates the subject upon which it treats, and every book is too small which gives crude and imperfect notions of what it proposes to teach. "Μεγα βιβλιον μεγα κακον" is a proverb which in the majority of instances is better forgotten than remembered. Indeed the prevalent desire at the present time seems to be, to see not *how much* but *how little* of anatomy may be learned by a student of medicine to insure his degree; these *little* books are the very things for such cases. We certainly agree with the author that minute anatomy and some other matters are out of place in a "dissector," but we want full and lucid descriptions of all that is left after this pruning out.

We are told that "illustrations have been omitted, for the reason that they add to the expense of a book, often without enhancing its real value, and from a belief that they are liable to great abuse, by distracting attention from the descriptive text to the numbered references, the simple verification of the latter taking the place of the full information only to be obtained from the former." We had supposed that the time had gone by, when any individual would attempt to excuse himself for the neglect of a most valuable method of teaching upon such untenable grounds as these. Pictorial illustrations are of such acknowledged value that the student would willingly pay for their use, rather than be deprived of them; and as to their "great abuse by distracting attention from the descriptive text," our author might on the same basis recommend the student to eschew dissections lest when he turns to his subject, the original of the pictures, his attention should be distracted from the full information of the said text. It is well known that few descriptions, even those emanating from the most practical minds, are adequate to convey a distinct impression of the object under consideration, and that pictures may therefore be made a means of correcting ideas and assisting the student to recognize the part in his subject. The use of a practical anatomy constructed without illustrations, will invariably double the labour of the demonstrator, who is then constantly called upon to recognize for his student, the nerve, vessel or muscle sought, when a simple glance at a wood-cut would "verify" the description; and this remark will be fully supported by the experience of any one who has had the charge of large classes. The decided advantage which illustrations would be to this book, will be shown by a reference to the description of the muscles of the pinna, on p. 3.

"The *major helicis* is to be found on the anterior border of the helix, just above the tragus.

"The *minor helicis* is placed upon that part of the rim of the ear which extends into the concha," etc. etc.

Now, if these descriptions give the slightest idea of the shape or direction of the muscles in question, we have failed to gain it. To be sure these muscles are not of any particular importance to the student, but if it was worth while to put them in large type, it was worth while to illustrate such imperfect text in a manner to render it clear.

The body of the book is opened by "A few general rules to be observed in dissecting," thus: The use of the belly of the knife in exposing muscles, the point in exposing arteries, the necessity of good forceps, etc., are briefly referred to, and then the "Maxims for the dissecting-room" are summed up thus:—

"1. Cover the part with damp cloths after dissecting. Drying is worse than decomposing.

"2. Put all the fragments on a piece of paper, which, with all fluids, are to be removed as they accumulate.

3. "Make everything tense, if possible, before dissection, and complete the dissection of one thing thoroughly, before another is commenced.

"4. Never leave a muscle until you come to bone at both ends—if there is bone.

"5. Let the eye go before the hand, and the mind go before the eye.

"6. Know what the books say, and cross-examine them in presence of the subject."

We cannot say that we consider these maxims as either elegant or correct.

In regard to the second maxim we should simply remark that in most well-

regulated dissecting-rooms, tin dishes and sponges are furnished, which are more appropriate than pieces of paper.

Concerning the impropriety of the latter part of the third, and the fourth maxims, as general rules, many of the subsequent pages amply testify. It is frequently impossible to "complete the dissection of one thing thoroughly before another is commenced;" and the direction, "never leave a muscle till you come to bone at both ends—if there is bone," is equally erroneous in practice. Thus, see p. 90: "The insertion of the biceps and brachialis anticus cannot be examined till a subsequent stage of the dissection." Neither can the long head of the biceps be properly examined until the shoulder-joint is opened for its study. On p. 125: "The insertion" of the flexor carpi radialis "cannot be seen till the hand is dissected." On p. 126, in speaking of the flexor sublimis digitorum: "inferiorly it divides into four tendons which pass beneath the annular ligament, to be inserted into the base of the second phalanges of the fingers, as will be seen in the dissection of the hand." In short, any one can call to mind many instances, both of muscles, arteries, and nerves, the dissection of which cannot be completed before they are left, since they extend through various regions, all of which cannot be opened at the same time.

There are some errors in the book which we are inclined to regard as typographical, although they are of such a glaring character that they should not have occurred. Thus on p. 7 we have: "Beneath this muscle," speaking of the levator labii superioris proprius, "will be found the branches of the *infra-orbital* nerve; being the terminal filaments of the superior maxillary branch of the THIRD pair of cranial nerves." This, however, is put correctly on p. 22. On p. 9: "Beneath the depressor anguli oris will be found issuing from the mental foramen, the termination of the *inferior dental* branch of the SUPERIOR maxillary trunk of the THIRD pair of cranial nerves." On p. 11 this is in a casual way stated correctly.

There is also to be noticed the improper spelling of certain words, which we hold to be of importance. We refer, *firstly*, to the name of a part of the innominate bone, viz., *pubes*. In using this term our author seems to vacillate somewhat between pubis and pubes, but on the whole, we think he rather favours pubis, as this is his most frequent spelling. Thus on p. 148 he speaks of the "spine of the pubis;" on p. 149 of the "crest of the pubis;" and on p. 230, says, "attached to the bones of the pubis," whilst on p. 211 he speaks of "the ramus of the pubes and ischium." Without doubt *pubes* is the proper word to employ, either in the singular or plural, and the bone is either "the pubes," or the "os pubis," or the "pubic bone."

Secondly. *Hilus* is improperly used for *hilum*. There is no such word as hilus, although it has been used pretty freely in the descriptions which our author gives of the spleen, liver, and kidneys. We must, however, give him credit for the use of the word *calvaria*, which is often very improperly written *calvarium*.

The practical directions, though short, are, in general, good; some, however, are, we think, open to criticism; thus on p. 3, we are told: "To obtain a perfect idea of the external auditory canal, a plaster or wax cast of it should be made; the canal being previously cleared of all accumulated cerumen, and oiled with a feather, the cast when hardened will be easily extracted." Experiment does not seem to verify this statement, which we were inclined to doubt in the first place, from the anatomical construction of the parts, which is very imperfectly referred to on p. 79, where the author, speaking of the osseous portion, says "its lower wall describes a curve, the convexity of which is directed upwards;" and, in fact, all anatomists agree that the meatus is most contracted in the middle, whilst Sharpey and Quain tell us that "the outer opening is larger in the vertical diameter, but the tympanic end of the tube is slightly oval in the opposite direction." We however had the experiment tried in some five or six cases; the plaster in every instance, even though set in some for twenty-four hours, broke without coming away, and the wax could only be removed while yet warm, so that, yielding to compression, it could not give "a perfect idea of the external auditory canal."

We are told on p. 6, when speaking of the lachrymal gland, that "its excretory ducts cannot be demonstrated." This can often be done, as pointed out by Cru-

veilhier, by rubbing a solution of carmine, or slightly diluted ink, in the superior palpebral sinus, when the orifices will fill with the colour.

In the directions on p. 13, for the removal of the calvaria, we are told "it may be done more neatly with a saw guided in the proper direction by a string tied round the head." This is almost equal to making an ink-mark over a vein as a guide in bleeding. It is useless as a guide, since, if the saw leaps from the forming groove, the string will certainly be cut; we fancy that the operator had better "let the eye go before the hand, and the mind go before the eye," when he will perform a much more successful section. We are also told that "the saw is to be carried through the outer table of the bone only, and the inner one fractured by a chisel and mallet; this saves the membranes of the brain from being wounded." Yes, this result would certainly follow *if* it were possible to saw through the outer table *only;* but we do not suppose, knowing the varying thickness of the several bones, that this feat is likely to be performed more than once in a very long series of attempts. We have rarely seen it successful, either in our own or other hands, and Cruveilhier, who has had *some* experience, says, "but it is almost impossible to avoid cutting the brain with the saw." The dura mater, and even the brain, will accordingly very often be found cut, especially in the regions of the temporal fossæ, after such a section.

In speaking of brains on p. 64, he says: "The autopsy-room is the only place where they can be obtained fresh enough for dissection, and the firmness of their tissue may be improved by several days' immersion in alcohol." It would seem, therefore, that our author has no experience with the solution of chloride of zinc as a preservative, with which nearly all subjects are now injected previously to the use of plaster or other material. In these cases, the brain is almost invariably so firm that in recent subjects the cranium need not be opened until every other part is dissected, and even after "material" has been kept for many months, the brain is not unfrequently in excellent order for examination, its colour only, in either instance, being destroyed.

The anatomical descriptions are singularly loose in many instances.

In referring to the borders of the falx cerebri on p. 14, he says: "the inferior, which is concave and free, is in contact with the corpus callosum of the brain;" now this *contact* occurs *only* at the back part, so that, in the front part of the median fissure, the hemispheres covered by pia mater are actually in contact; we have frequently seen a very considerable space between the greater part of the inferior margin of the falx and the corpus callosum, the visceral arachnoid, as usual, passing from one hemisphere to the other, not being far beneath the margin.

On p. 167: "If the peritoneum be peeled off from the intestine, the *muscular coat*, consisting of pale, transverse fibres, will be seen beneath it; in the large intestine these are chiefly collected in the longitudinal bands characteristic of that part of the tube." We, however, think it probable that, unless the external longitudinal layer of fibres of the small intestine is peeled off with the peritoneum, these will be seen overlying the circular fibres; but we do not think it probable that the *transverse* fibres will be seen gathered into the longitudinal bands of the large, as the text certainly reads.

On p. 202, we are gravely informed that "The orifice of the vagina is transversely elliptical; in the virgin it is sometimes partly closed by a circular fold of mucous membrane, called the *hymen;* after childbirth, this is destroyed," etc. etc. Comment upon this paragraph is perhaps unnecessary. A vaginal orifice, *transversely elliptical*, would be curious to look upon, and the history of a hymen destroyed *after* childbirth, would certainly border on the marvellous. That this destruction, when delayed, must occur *during* childbirth, there seems to be but little doubt; and that it is accomplished, in most instances, at a period considerably anterior to this event, seems to be the general experience of observers. Accordingly, this destruction will be found to have taken place, *usually*, at the time of the first successful sexual connection.

The anatomy of the perineum, p. 184, is particularly unsatisfactory; so is the description of the nasal fossæ. In fact, our new practical guide resolves itself into nothing but an imperfect anatomical remembrancer.

We must, however, commend the division of the book into dissections of the

proper length, which is well done, and would form a most useful basis for a larger and more complete work. Economy of "material" and systematic labour are certainly the results of this proceeding.

In concluding this notice, we take pleasure in saying that we have observed on several pages indications that Dr. Hodges is not by any means unacquainted with his subject, and has alluded to the more recent investigations in descriptive anatomy. But we have always held that, if correctness of language and spelling, and clearness of description, are ever necessary to a medical man, it is during his pupilage, when impressions are received that are almost ineffaceable, even though they may be learned to be wrong years afterwards; hence our critical examination of this preliminary work.

The A B C's of the profession should be clear and distinct if it is to be properly respected. W. H. G.

ART. XIX.—*Guy's Hospital Reports.* Edited by SAMUEL WILKS, M. D., and ALFRED POLAND. Third Series, Vol. IV. London, 1858. 8vo. pp. 371.

THE present volume of this always welcome publication contains twelve original communications, eight wood-cuts, and fourteen lithographic plates. The following is an analysis of its contents:—

I. *Case of Epithelial Cancer of the Œsophagus, in which Gastrotomy was performed.* By S. O. HABERSHON, M. D.

II. *Description of the Operation of Gastrotomy.* By J. C. FORSTER.

These two articles, as they relate to the same case, for the sake of simplicity and brevity, will be considered conjointly. They present the detailed history of the symptoms observed in a patient affected with epithelial cancer, which, from its position, interfered both with deglutition and respiration, and of an operation performed for the purpose of establishing an artificial opening through which food might be introduced directly into the stomach.

It is well to state here that when they submitted their patient to the operation, neither Dr. Habershon nor Mr. Forster was aware that it ever had been practised before. Some of our readers must have remarked in Dr. Habershon's recently published treatise on diseases of the alimentary canal,[1] that the propriety of forming a gastric fistula in cases of organic stricture of the œsophagus is therein argued in a manner that shows the author not to have been aware that an operation for the formation of one had ever been performed. From its extreme rarity, we will give the whole history of this case, as briefly as possible, and using, so far as we can, the words of the original communications.

A man, 47 years of age, of ordinary stature, entered the hospital in the beginning of October, 1857, under Dr. Habershon's care, labouring under considerable dyspnœa. Examination showed the pulse compressible, but regular; the heart's sounds normal; the respiratory murmur indistinct, with some sibilant râle. After a short time he began to complain of severe pain in the throat during coughing, and in a few weeks pain was also produced in swallowing, especially when solids were taken. In December deglutition had become very difficult, so that liquids only could be taken. In February, on examining the throat, Mr. Forster detected, for the first time, a rounded tumour, situated below the epiglottis, towards the right side, and evidently obstructing the commencement of the œsophagus. In March the voice had become more feeble, and deglutition had become so difficult that nutrient injections were had recourse to. The respiration had now become so difficult that, on the second of March, tracheotomy was performed, the incision being made as low down as possible. The operation, however, did not afford any relief; the trachea appeared flattened from behind, and the patient could not bear the tracheal tube inserted; when it was attempted, he appeared to be quite

[1] Pathological and Practical Observations on Diseases of the Alimentary Canal, Œsophagus, Stomach, Cæcum, and Intestines. London, 1858. P. 28.

incapable of breathing. On the 25th of March the patient appeared to be sinking, and the rectum ejected the enemata almost at once. Under these circumstances, the question presented itself, whether life was to be allowed gradually to die out, or whether an attempt was to be made, by any other means, for the introduction of food. Dr. Habershon thus discusses this question:—

"Three modes of relief suggested themselves: 1st, the forcible introduction of an œsophageal tube; 2d, opening the œsophagus in the neck; and 3d, opening the stomach. In reference to the first, there was evidence of a growth at the commencement of the œsophagus; and the trachea appeared partially compressed, as shown in the operation of tracheotomy. The disease in the throat was probably of the form of epithelial cancer, and the passage of a bougie must have been constantly repeated. The great irritation and coughing produced by attempting to swallow showed that the epiglottis was extensively ulcerated; *or* that there was a connection between the œsophagus and trachea, which would render the passage of a bougie very dangerous. In some cases of cancer of the œsophagus, a bougie has been passed into the pleura, and led to speedy death; and probably the passage of a bougie could not have been effected; this decided against the first proceeding. As to the second—opening the œsophagus—the most frequent seat of cancer in that tube being opposite to the root of the lung, about the third dorsal vertebra, and consequently beneath the position at which the canal could be opened, would have made the operation a very formidable, dangerous, and useless one. In reference to the third—opening the stomach—this alone appeared to be the operation which could possibly relieve the patient. Wounds of the stomach, as that of Alexis St. Martin, the cases recorded by Mr. South, those by Dr. Murchison, &c., showed that life could be continued after fistulous communication had been thus made. The operations on the lower animals proved that it could be performed with some probability of success; such an operation would give a chance of prolonged life where death was certain; and where the peritoneum was healthy, there was less danger than in abnormal conditions of that membrane. If life were prolonged only for a short time, and food introduced, there would be relief to the distressing thirst and the fearful sense of starvation: and lastly, it was evident that the patient was dying from inanition rather than from disease, nutrient enemata being refused. On the other hand, however, I felt that the disease was probably of a cancerous character, and would sooner or later terminate life; that the operation was a hazardous and uncertain one; and that life might possibly be continued for a few days by a small portion of the injection being retained."

After carefully weighing all these facts, and calling in the assistance of Mr. Forster, opening the stomach was decided upon. The operation is minutely described in the second communication. An incision was made through the abdominal parietes in the course of the left linea semilunaris, commencing between the eighth and ninth ribs, and carried downwards to the extent of three and a half inches. The peritoneum was divided the whole length of the wound, upon a director, and the stomach thus exposed; it was then hooked up, with a tenaculum, to the abdominal parietes, and sewed thereto by an uninterrupted suture, with a strong silk ligature. After the first two stitches had been put in, the organ was opened by dividing what was included between the two portions of the tenaculum; the opening thus made was about three-quarters of an inch in length, and was afterwards slightly enlarged. In making the stitches, the needle was passed one-third of an inch within the stomach, so as to get a good hold, and as much as possible of the abdominal walls were included. As the opening in the stomach was much less than the external wound, the remaining part of this was brought together with an uninterrupted suture, the divided peritoneum being left untouched. An elastic tube was at once introduced into the opening, and milk with egg was injected through it into the stomach. In this way, in the course of twenty-four hours, six eggs, beaten up in twelve ounces of milk, were given in small, divided doses, with four ounces of rum, the quantity introduced each time being two ounces. The patient, however, began to sink, and, notwithstanding the free use of stimulants, he died about forty-four hours after the operation. Dr. Habershon expresses his belief that if the operation had not been performed, death might have taken place as speedily, if not more so, and that it relieved the

thirst and sense of starvation, and rendered the patient more comfortable. "Under these circumstances," he says, at the close of his communication, "it is urged that, if a favourable case be presented, the same operation be performed, but without waiting till life is almost extinct."

At the *autopsy* of the patient, the epiglottis was found healthy; at the posterior surface of the cricoid cartilage there was a growth connected with the mucous membrane, about a quarter of an inch in elevation, and extending from side to side, soft and slightly injected; passing downwards, there was irregular ulceration, and towards the trachea destruction of all the coats of the œsophagus; an opening one-sixteenth of an inch in diameter existed between it and the trachea; below the ulceration the canal was so much contracted, by infiltration into the surface of the mucous membrane, that a probe could only be passed after death; *this constriction was situated at the level of the first bone of the sternum;* the rest of the œsophagus was healthy. One or two glands in the neck were infiltrated and diseased. In the upper part of the right lung there were numerous lobules of iron-gray consolidation, and in the lower lobe there were numerous gray tubercles. The lower lobe of the left lung was much congested, and one or two lobules were softened and breaking down, from acute changes, probably a very short time before death. There was no evidence, the report says, of any cancerous disease affecting any part except the œsophagus and one or two adjoining glands. It is to be regretted that no microscopical examination was made of those tissues said to have been affected with cancerous disease; or, if any was made, that no mention is made of it. In a case resembling in very many respects the one under notice, and which we witnessed ourselves, and have reported (see *Clinical Lectures on Surgery*, by M. Nélaton, p. 50), the stricture was owing to a large mass of tubercular matter that had been deposited between the trachea and œsophagus, and had undergone the usual changes witnessed in such deposits. In connection with this subject, it may be interesting to mention that, some years ago, in making some dissections for the purpose of discovering why strictures of the œsophagus should always be situated near the upper part of the sternum, we invariably found a number of lymphatic glands lying between the œsophagus and the trachea, just in that region.

An operation similar to the one above described has been performed twice before; in both instances by M. Sédillot, of Strasbourg. Mr. Forster refers to them very briefly at the end of his communication: he was not acquainted with them at the time of performing the operation himself. M. Sédillot calls the operation *gastrostomy* (*stomach-mouth*). The first time he practised it, which was in 1849, he made a crucial incision, about an inch and a half in diameter, over the rectus muscle, two inches and a half below and to the left of the xiphoid appendix. Dividing the rectus muscle and the peritoneum, and holding back the four flaps by the aid of the superficial and upper layers of the epiploon, he drew out a portion of the great curvature of the stomach, and making a puncture into it, he introduced a double canula, each tube having a projecting edge bent at a right angle, in order to keep the stomach against the walls of the abdomen, and also to retain the instrument at the external wound. Almost immediately, however, the stomach was drawn forcibly backwards; the canula followed, and was dragged into the abdomen; the patient died in twenty-one hours. The second time he performed the operation, which was in 1853, he modified his first proceeding; the incision was made extending from a spot two finger-breadths from the median line to less than an inch from the edge of the false ribs on the left side. After opening the peritoneum, the stomach was drawn up and fastened to the abdominal walls by the aid of sutures, passed through the skin and the peritoneal and muscular coats of the stomach, the opening of which was delayed until adherences should have formed between it and the abdominal walls. Two hours afterwards, during a violent attack of cough, the stomach was dragged into the abdomen. The threads of the sutures were removed, and a portion of the stomach, pulled out, was inclosed in the forceps of Assalini, with the hope of obtaining adherences, and afterwards an opening, from the mortification of the part inclosed by the forceps. By the fifth day solid adhesions were established, the gangrene was complete, and the mortified part was cut off by the scissors. Nutritive injections

were then thrown in through the fistula, but at the expiration of ten days the patient died of peritonitis.

In performing this operation of gastrostomy, the plan of waiting to open the stomach until adhesions have been formed between it and the walls of the abdomen, is certainly advisable. The mistake made by M. Sédillot was in not including the whole thickness of the wall of the stomach in his sutures. It seems to us, however, that it is an operation that should never be performed in cases such as that related by Mr. Habershon, where the stricture of the œsophagus is owing to organic degeneration, and when the general condition of the patient is so miserable. It could only be authorized when the stricture is caused by cicatrices, the effect of the action of hot liquids or of chemical agents, and the patient has no other concomitant affection.

These communications are accompanied by four plates, showing the position and extent of the external incision, the position of the stomach, the interior of the stomach at the opening, and the growth in the œsophagus.

III. *Pathological Observations.* By Samuel Wilks, M. D.

These are, as we learn from a few preceding remarks, the first of a series of pathological observations on subjects of interest, which the author has made by carefully reviewing the reports of nearly 2000 post-mortem examinations. The remainder of the series, from want of space, has been left to a future occasion for publication. The first chapter is upon Cancer and New Growths.

The last number of *Guy's Hospital Reports*—a notice of which was given in the April number of this journal, for the past year—contained a valuable communication from Dr. Wilks on the specimens of diseases of bone, contained in the Hospital Museum, with reference chiefly to the question of malignancy. We have been considerably disappointed at finding the present communication not to be one of the same kind. In his former communication Dr. Wilks confined himself, almost entirely, to stating facts, and then adding the deductions he would himself be disposed to draw from these facts. In this one, on the contrary, he indulges freely in fanciful speculations on great pathological questions, here and there adding thereto a fact that suits the particular hypothesis at the moment in favour.

Dr. Wilks believes that in every new growth the question to be asked is not so much whether it is malignant or not, as what is its degree of malignancy. In every case of adventitious growth or deposit in the body, two causes are in operation, one local, the other constitutional. The first cause alone in operation has a tendency, in his opinion, to the formation of a tissue allied to the healthy structures, whilst the latter has simply a power for the production of the simplest elements, as seen in malignant growths; when combined together we have, as one or the other cause predominates, all the intermediate varieties between extremely benignant and extremely malignant. To discover how much one cause is in operation and how much the other, Dr. Wilks believes to be the clue to the whole question of new growths. When, for instance, the local cause does not act from any reason, when plasma is thrown out, "the tendency is to the production of cells such as existed in the embryo preceding the formation of the tissue, and, therefore, if such embryonic cells spring up and continue to grow in the perfected body, we have a cancer; a cancerous tumour being simply a mass of embryonic cells." "A woman of middle age receives a blow on the breast, a slow growth springs up, composed of nucleated cells—this is cancer. Let the same local cause be in operation in a young woman of comparatively healthy constitution, when the procreative powers are in full force, and then the blastema or crude formative material supplied by the blood is disposed to assimilate itself to the natural structure, and the growth resembles, though imperfectly, the original gland; we have thus a mammary tumour or adenocele."

The views of Dr. Wilks on the subject of new growths are to a considerable extent novel, and, at all events, quite ingenious. If they should prove to be correct, however, it would be to us another example of the fact that *le vrai n'est pas toujours le vraisemblable.*

The second article is on the subject of Acute and Chronic Diseases. As the result of his observations, Dr. Wilks believes, that disease of the various parts

of the body is, as a rule, chronic, and that the acute affections are merely terminations of these, or are set up by them. This article is very short, only occupying four pages, and the subject is not illustrated by examples.

The third article is on the Relative Importance of Disease of the Aortic and Mitral Valves of the Heart. The different opinions respecting the relative severity and importance of these two lesions are explained by Dr. Wilks as owing to the facts that the existence of the aortic lesions is dated from the commencement of symptoms connected with them, and of mitral disease from an inflammatory attack which may have occurred many years before, and that the duration of the severe symptoms is much greater in disease of the aortic valves than in disease of the mitral.

The fourth article is upon Contre-coup. As the result of his observations, Dr. Wilks states, that fracture by contre-coup never occurs, and as to trephining the cranium on the side opposite to the one upon which the blow was received for the purpose of affording an issue to effused blood, he declares that he has never met with a case on the *post-mortem* table where an operation on the side opposite to that injured could possibly have given any relief.

The fifth article is on the subject of Chronic Rheumatic Arthritis, the disease so fully and ably treated of in the recent publication of Dr. Adams (*A Treatise on Rheumatic Gout, or Chronic Rheumatic Arthritis of all the Joints*, by Robert Adams, M. D., Surgeon to the Richmond Hospital, London, 1857). The museum of Guy's Hospital contains thirty specimens of the disease as it affects the hip-joint, nine of the disease of the shoulder, five of the elbow, but scarcely any of the other joints, together with four or five doubtful specimens of the disease of the vertebra and the pelvic articulations. As the connection of the disease with rheumatism is not established, Dr. Wilks would much prefer to call it simply chronic arthritis, "for the relation between it and acute disease of the joints is the same as between all other acute and chronic disease."

IV. *On the Operation of Opening the Urethra in the Perineum.* By THOMAS BRYANT.

With the laudable design of giving some new information for the settlement of this much vexed question, of opening the urethra in the perineum, Mr. Bryant has taken up the subject in order to illustrate it by the cases that have occurred at Guy's Hospital. These cases are eighteen in number, and the conclusions he draws from them are as follows:—

"1. In uncomplicated retention of urine from organic stricture the operation of opening the urethra in the perineum is not required, the more simple and safe one of puncturing the bladder through the rectum being preferable.

2. When complicated with extravasation of urine from any cause, it should be performed at once, and the stricture, when present, divided, if possible.

3. In laceration of the urethra from injury, when a catheter cannot be passed, the urethra should be opened.

4. And also when the above injury is associated with pelvic mischief.

5. Strictures are occasionally met with which are impermeable, and urethras which are obliterated.

6. That in cases of organic stricture, where the passage of a catheter is possible and not difficult; when it does not produce either any injurious or painful constitutional or local disturbance, and when, after dilatation of the stricture, an occasional passage only of the instrument is required to maintain an open channel, no other surgical means can be called for.

7. That cases of stricture do occur occasionally which are so exquisitely sensitive, and in which the passage of a catheter, however skilfully performed, is followed by such severe constitutional and local disturbance, as to produce more harm than good; and *others* which are relieved by means of a catheter, and are even fully dilated, but which have a tendency to contract again immediately upon the omission of the treatment; in such cases the operation of 'external division' (this is, with a staff in the urethra) is most valuable.

8. That the majority of cases of what are called impermeable strictures may be rendered permeable by constitutional treatment, but that some are undoubtedly impermeable; in such cases, the operation of 'perineal section' (this is, opening the perineum without having a staff in the urethra) is of value.

9. When the urethra is obliterated, the operation of 'perineal section' may occasionally be demanded, particularly when associated with perineal fistula.

10. That the worst and most intractable forms of stricture are the result of injury, and in these cases the operation either of 'external division' or 'perineal section' is of great value.

11. That in boys the operation is not so successful as in adults, although no better can be suggested."

V. *On Ophthalmostasis, with an account of an Improved Method in Extraction of the Cataract.* By John F. France.

In this communication, after giving an account of many of the modes used for fixing the eye, Mr. France advocates the use of artery forceps. He applies their extremity with rather firm pressure a little beneath the inferior margin of the cornea, and clasps securely a somewhat broad portion of conjunctiva and of the submucous fascia. For the *idea* of using the artery forceps, he acknowledges his indebtedness to Desmarres, who mentions their use, but prefers other instruments.

Two plates accompany this paper, showing the instruments referred to therein. It is worthy of remark that no mention is made of the ophthalmostat which acts by the pressure its plate exerts in the oculo-palpebral cul-de-sac at the external angle of the eye, thus fastening the conjunctiva against the bone and making it impossible for the eye to turn inward.

VI. *On the Existence of Copper in Organic Tissues.* By William Odling, M. B., F. C. S., and August Dupré, Ph. D.

This communication, in a medico-legal point of view, is one of very great interest. While some very distinguished toxicological and physicological chemists affirm that whenever copper occurs in animal or vegetable tissues or fluids, its presence is to be regarded as exceptional, or abnormal, or accidental, others equally eminent maintain that copper is a natural and constant constituent of living organisms. From the experiments made by the authors of this paper, we think it must be positively concluded that copper does very frequently occur in certain vegetable and animal tissues, particularly in those of the kidney and liver, and that in the last named organ especially it sometimes presents itself in very considerable quantity.

VII. *A Collection of several Cases of Contusions of the Abdomen, accompanied with Injury to the Stomach and Intestines.* By Alfred Poland.

From a variety of sources Mr. Poland has collected as many as sixty-four cases in which different portions of the alimentary canal were injured by contusion. They are all arranged in order, as the injury affected the stomach, the duodenum the jejunum, the ileum, the cæcum, and the colon, and the most important points in each case are briefly related.

In three of the cases, the ruptured bowel occurred in persons the subject of hernia, but independent of any hernial mischief. A case of this kind would demand the utmost attention and circumspection, not only as to the question of diagnosis, but as to the treatment to be adopted. In one of those related by Mr. Poland, a woman, afflicted with a femoral hernia, was struck by her husband on the abdomen, and she died in twelve hours. Before her death, however, a surgeon reduced the hernia. At the autopsy the small intestine three feet from the stomach, and exactly opposite the seat of the blow, was found nearly torn across. The husband was accused of having caused her death, and the defence set up was—1st. That the rupture occurred in the hernial sac, and that the rent in the bowel was caused by the efforts of the surgeon in reducing the hernia; and 2d. Supposing this not to have been the case, that the rupture was spontaneous and accidental from the giving way of a diseased bowel. The arguments against these theories were considered conclusive, and they have thus been summed up :—

1. The pain came on immediately after the blow, and before the reduction of the hernia.
2. The ruptured bowel was at a considerable distance from the sac.
3. No inflammation or fecal matter in the sac.
4. Distinct external mark opposite to where the ruptured intestine lay.

5. No appearance of ulceration of the mucous membrane.

The husband was therefore found guilty of the death of the woman.

Another class of cases are those in which there is contusion of the intestine while in the hernial sac. Four cases of this kind have been reported in a former number of *Guy's Hospital Reports*, by Mr. Aston Key, and to these Mr. Poland has added eleven more. Of the whole fifteen cases thus assembled, in thirteen the gut was ruptured by the injury, and all died; in two the bowel was bruised and sloughed, and recovery took place. These two cases of recovery show the importance of laying open a hernial sac, after a blow thereon, whether there be intestine down or not, provided there be severe local or constitutional symptoms. In no case of the kind should reduction ever be attempted.

In drawing attention to this class of injuries Mr. Poland states that he "was mainly influenced by the fact that much injudicious and careless (we might almost say reckless) treatment has been resorted to. In the numerous instances here collected, we have sad specimens of the action of purgatives in hurrying the unfortunate patient to a more speedy and inevitable death."

The treatment in all such cases should be perfect rest of the body, and total abstinence from food or drink for forty-eight hours; the occasional use of ice, in small quantities, and at prolonged intervals might perhaps be permitted. Opium should be given to quiet the system and to prevent peristaltic action. In addition Mr. Poland advises the free and unsparing use of leeches, so soon as peritonitis sets in, to be replaced by hot fomentations, to be repeated with judgment and without fear.

VIII. *Cases of Paraplegia.* Second Series. By WILLIAM GULL, M. D.

In this second series of cases of paraplegia—the first, which contained sixteen cases, was published in the Report for 1856—the histories of seventeen cases are given, with the *post-mortem* examination of each, and remarks by the author. These cases are, perhaps, most particularly interesting from the aid afforded by their pathology for the solution of some of the most hidden questions in physiology; the instruction given by these experiments made by nature is, in our mind, more trustworthy than that extracted from those instituted by the most skilful experimental physiologists.

From Case XVII., Dr. Gull says that it may be concluded with certainty that the spinal cord may have its functions impaired, and even lost, and that suddenly, as far as the power of motion is concerned, without any distinct amount of anatomical lesion. He urges with earnestness the investigation of the nervous substance by other means than the microscope, and declares his belief that Dr. Stankey's observation on the variable specific gravity of the brain lets in some light in this direction. He believes that "it is from an increased knowledge of 'atomical' as distinguished from 'anatomical' conditions that we may hope for future advances in nervous pathology." Notwithstanding such statements at the commencement of his communication, Dr. Gull, in all the other cases, invariably attributes to alterations found by the scalpel and the microscope the symptoms observed before death, and would not listen with patience, probably, to an objection which these same statements would justly allow us to make to such a proceeding, namely, that these same symptoms might have been owing, not to these anatomical lesions, but to certain atomical changes in other, and apparently healthy, portions of the nervous system. We have ourselves no desire to urge such an objection, for we do not coincide with him at all in this peculiar belief.

In Case XIX. the prominent symptom was a total want of power to regulate the muscular contraction. The affection of sensation was limited to numbness, and formication of the hands and feet. The autopsy showed that the posterior columns were atrophied throughout their whole length. Brown-Séquard found in his experiments that when the posterior columns were destroyed for a limited extent, hyperæsthesia is produced below the seat of injury; when, however, they are destroyed throughout their whole length, instead of hyperæsthesia there is loss of sensibility to some degree. The effect upon the muscular movements is in accordance with the theory of the use of the posterior columns proposed by Dr. Todd, that they "propagate the influence of that part of the encephalon which combines with the nerves of volition to regulate the locomotive powers,

and serve as commissures in harmonizing the actions of the several segments of the cord."

In Case XVIII. the symptoms were numbness and weakness of the legs for several months, sensation not being impaired. At the autopsy the gray substance of the cord was found atrophied, and the posterior columns in a state of chronic inflammatory degeneration.

In Case XIX. there was paraplegic weakness of the lower extremities, characterized by a want of control over the contraction of the muscles; the voluntary muscles, generally, were emaciated. After death, chronic inflammatory degeneration of the posterior columns of the cord, throughout their whole length, was discovered; the disease was strictly limited to the posterior columns.

In Case XX. there was paralysis of both seventh nerves; the upper extremities were weak, the lower nearly completely paraplegic. The onset of the symptoms in this instance was acute. At the autopsy the substance of the cord in the cervical and dorsal regions was found to be somewhat softer than natural, but no other unequivocal change was discovered by the unassisted eye or by the aid of a common lens. On hardening the pons Varolii, medulla oblongata, and cord, and preparing sections of these parts, it was seen that in the anterior part of the commissure, throughout the length of the cord, but principally in the lumbar and superior cervical regions, and throughout the structure of the medulla oblongata, but chiefly at its superior part, there were exudation-cells scattered interstitially amongst the tissue. These cells were recent; they had not undergone fatty degeneration.

Case XXII. is interesting as showing that the substance of the cord may receive an injury through violent muscular exertion, whilst the surrounding tissues escape. The paraplegia, which was complete, supervened two days after a violent exertion in lifting a heavy weight. There was no injury of the membranes, ligaments, or bones of the spine; but opposite to the fifth and sixth dorsal vertebræ the cord was softened through all the columns into a thick, greenish, muco-puriform fluid, with a tinge of brown, which, examined by the microscope, was seen to consist of disintegrated nerve-tissue, with a few irregular collections of granules. The greenish and brownish tints of the softened parts were owing, probably, to blood-colouring matter.

In Case XXIII., after direct violence to the posterior cervical region, the legs, the left arm, and the sphincters were paralyzed; loss of sensation, which followed the injury and lasted for a few hours, gave place to hyperæsthesia, the sensation in the most distant parts returning first. The membranes of the cord were found healthy; opposite the fourth and fifth cervical vertebræ the substance of the cord was contused. On section, there was found ecchymosis of the posterior horn of gray matter on the left side, and of the adjacent part of the lateral and posterior columns. There were also other limited spots of ecchymosis on the right side, one in the right posterior column, and one in the anterior cornua of the gray substance. The development of hyperæsthesia in this case is in accordance with the experiments of Brown-Séquard, who has shown that injury of the posterior cornua of the gray matter is followed by hyperæsthesia of the parts below.

In Case XXIV. the patient was paraplegic in both the upper and lower extremities, having become so several hours after a fall backwards from a moderate height; there was loss of sensation in the paralyzed parts. After death an effusion of blood was found outside of the theca vertebralis in the neck; the cord itself was uninjured, the extravasation came from injury to the lower part of the body of the fourth vertebra, which had been fractured and the intervertebral substance torn.

An interesting phenomenon was observed in this case, one to which attention was directed in an interesting paper in the last number of this journal, entitled "Remarks on some Affections of the Spinal Column," by Dr. Packard, namely, the increase of temperature of the surface of the body. The skin, it is mentioned, became "intensely hot," and, at the same time, "the ribs scarcely moved in inspiration." To explain the phenomenon, Dr. Packard is inclined to believe that by the injury to the cord "a check is removed from some heat-generating agent —possibly the sympathetic system of nerves—which, no longer controlled by the regulating influence of the cerebro-spinal axis, carries on its functions to an

inordinate degree." (*Loc. cit.*, p. 65.) Another supposition which we venture to make, though with some hesitation, is this, that the nerve-force generated by the nervous system in its disturbed condition may be converted rapidly into heat. We know that electricity, under certain circumstances, may be thus converted, and electricity and nerve-force, if not identical, are very similar in many respects.

Case XXVII. has been recorded in order to afford an instance of how lesions of the cord are occasionally attended with an affection of the joints, not to be readily distinguished from that which occurs in acute rheumatism. In this instance the patient had received no injury, and she recovered, so that no *post-mortem* examination was made. The case might, we think, be very properly considered as showing rather how in acute rheumatism the spinal cord is occasionally affected. The patient had, in addition to redness, pain, and swelling of the larger joints, a white, furred tongue, hot skin, profuse perspirations, having an acid smell, and a systolic murmur over the left ventricle.

What Dr. Gull says of the effects of strychnia, when administered in affections of the spinal cord, is worthy of attention. His opinion in regard to it coincides, we know, with that of the physician in this country who has had the greatest experience in the treatment of diseases of the nervous system.

"The therapeutical agency of strychnia in organic lesions of the cord has yet to be proved. Judging from its effects, we should say its direct operation on the tissue was the very reverse of nutritive or reparative. If function is, as there can be no doubt, the effect of a mode of disintegration, agents which directly increase function must produce a disintegrating action. If this be a sound inference—and experience leads to the same conclusion—strychnia has but a limited therapeutic application in paraplegic affections. It is well known that immediate and striking effects can be produced by the drug, but these are often followed by hopeless bankruptcy of the spinal power."

This communication is accompanied by four exquisitely executed plates, representing magnified transverse and longitudinal sections of the diseased portions of the spinal cord in the various cases.

IX. *A Case of Pharyngotomy for the Extraction of a Foreign Body, with some Remarks.* By EDWARD COCK.

The foreign body lodged in the pharynx in the case whose history is given in this communication was a gold plate, shaped to correspond with the hard palate behind the incisor and bicuspid teeth, and holding a false central incisor tooth. The extreme length of the plate was an inch and five-eighths, while a line drawn from the extremity of the false tooth to the other edge of the plate measured exactly one inch. It was arrested in the gullet directly behind the cricoid cartilage. A pair of strongly-curved forceps detected the plate, but it could not be grasped, nor could it be moved from its position. With the hope that its position might possibly be altered by the action of vomiting, a pint of milk was conveyed into the stomach by a flexible catheter, and then half a drachm of sulphate of zinc and a scruple of powdered ipecacuanha administered. These emetics did not, however, excite even a sensation of nausea, and no others were tried. Some futile attempts were afterwards made to pass a looped wire round the plate, and also to manipulate with a flexible tube from the extremity of which a pair of forceps could be projected. Four days having now elapsed, and the patient beginning to feel seriously the want of rest and nourishment, it was determined to cut down and open the gullets.

The incision was made on the left side, and the operation was performed in the way usually advised in treatises on operative surgery. The gullet was opened by cutting against the most prominent part of the foreign body, which proved to be the false tooth. After the operation, the external wound was dressed without any attempt to bring the edges together, and for three weeks the patient was fed through an œsophagus-tube; at the expiration of another week the external wound had closed, and his recovery was complete; at least, with the exception of some modification in the tones of his voice, the result, in all probability, of injury done to branches of the recurrent laryngeal nerve during the operation.

In addition to his own case, Mr. Gull relates briefly the histories of seven

others, in which œsophagotomy has been performed. Five of these were successful, while in two death followed the operation. In one of the fatal cases, where the patient died in two days from collapse, the foreign body, a portion of bone, was swallowed during the operation, and found after death in the rectum.

The operation performed in this case by Dr. Gull, though the one usually recommended when opening of the gullet is thought indispensably necessary, is not the one we should prefer ourselves. It is one that presents very considerable difficulties, on account of the depth to which the dissection must be carried, and the important vessels and nerves in the neighbourhood. It is, besides, very difficult to distinguish the muscular fibres of the œsophagus, when they have lost their normal appearance from the inflammation occasioned by the presence of the foreign body, and covered by the blood which flows abundantly. The following operation, which has been recommended by very distinguished authority, appears to us to offer greater facilities: Divide the integuments in the median line, and separate and hold aside the sterno-hyoidian muscles, so as to expose the isthmus of the thyroid gland; under this pass a blunt needle, carrying a double thread, so as to make two ligatures, between which the isthmus is divided; the left lobe of the thyroid gland is separated by a blunt instrument, which, passing close to the trachea, must necessarily encounter the œsophagus, which is opened in the usual way.

Before, however, proceeding to any operation in these cases, it would certainly be very advisable to exert a greater degree of ingenuity and patience in the employment of ordinary measures, than appears to have been exerted here. Although it was supposed that the action of vomiting might be beneficial, only one emetic dose, and that of ipecacuanha and sulphate of zinc, was administered. To extract the body no other instruments were used but a looped wire and curved forceps, and yet there was sufficient space to enable a flexible catheter to be passed down into the stomach. The well known history of the manner in which a fish-hook was extracted from the œsophagus, shows what a certain degree of ingenuity may accomplish in these cases. In this case it seems to us that a bag of caoutchouc attached to a tube could have been introduced empty beyond the gold plate, then distended so as to loosen any entanglements formed by it with the walls of the œsophagus, when it would be removed with the withdrawal of the instrument. In the *Memoirs* of the Academy of Surgery of France, vol. iii. (Paris, 1743), some most curious cases of foreign bodies arrested in the œsophagus are given, and in some of them sponge was used successfully, to distend the canal and thus loosen the body and make it fall into the stomach. It is but right to state here that Mr. Gull, who refers to this memoir, is incorrect in his declaration that in no single instance does any attempt appear to have been made or contemplated to cut into the pharynx or œsophagus, and relieve the patient. The operation is discussed, and two very remarkable observations are given to show how the œsophagus has been largely opened, and yet the patient has recovered. (*Loc. cit.*, p. 151 *et seq.*)

This operation of œsophagotomy, or pharyngotomy, must be regarded as one never to be performed, unless absolute necessity calls for it; the ancient plan recommended by Aëtius for the removal of foreign bodies arrested in the œsophagus, we had rather see adopted in general practice.[1] Before proceeding to active measures in these cases, the surgeon should make very sure that there is undoubtedly a foreign body lodged in the gullet, for the sensations of the patient himself are by no means to be trusted. We know of an instance where no less distinguished a surgeon than M. Nélaton made several ineffectual efforts to remove from the pharynx, with forceps, the great horn of the hyoid bone. The patient was sure that a piece of bone had become arrested there, and the finger pushed deeply into the throat came in contact with a small resisting body—which ought to have been there, and which fortunately remained.

X. *Contributions to the Practical Surgery of New Growths or Tumours.* Series II. *Fibro-plastic Growths.* By John Birkett.

This communication is made in pursuance of the design, announced in the

[1] See the Medical Profession in Ancient Times, by John Watson, M. D., p. 196.

last volume of *Guy's Hospital Reports*, of illustrating in a series of cases the natural history of every form of new growth which has come under observation within the walls of the hospital. In Series I. several cases of cancer were related to illustrate the progress and result of that disease. The two varieties of fibro-plastic growths are comprised in this paper—the soft, gelatinous, and rapidly growing; and the more firm, fibrous, and more slowly growing. The number of cases is, altogether, fourteen. The conclusions deduced from them, by Mr. Birkett, are the following:—

"1. That the elementary tissues comprising the fibro-plastic growths differ from those entering into the composition of the tumours called carcinoma.

2. That the natural history of the fibro-plastic growths is different from that of carcinoma.

3. That the fibro-plastic growths may recur at the primary site of the new growth, or in its immediate neighbourhood.

4. That, unlike carcinoma, there does not appear to be a disposition to the production of fibro-plastic growths in any of the viscera of the chest or abdomen.

5. That when secondary growths are developed in these organs, it will probably be carcinoma.

6. That the glands of the lymphatic system do not become secondarily involved in disease with the fibro-plastic growths, as they do in carcinoma.

7. That amputation of a portion of a member will not in every case prevent the reproduction of fibro-plastic growth in the stump, even although a joint intervene between the seat of the primary development and the stump.

8. That excision of a primary fibro-plastic growth may be undertaken with a better chance of the eradication of the disease than follows the removal of carcinoma.

9. That by the reproduction of a fibro-plastic growth, and as the result of changes taking place in the tumour itself, death may ensue without the viscera being affected by any organic disease.

10. That the progress of the disease is slower than carcinoma; that is, that the time occupied by the development of the recurrent growths may extend over a very long series of years.

11. That the fibro-plastic growths are developed at a somewhat earlier period of life than carcinoma.

12. That they appear to be closely in relation with fasciæ, and very often to spring up in those parts of the body where the fascial envelops or tendinous aponeuroses are developed in the most prominent manner.

13. That they always form circumscribed lobes or masses, and never infiltrate the tissues of the organs of the body like carcinoma so frequently does."

This communication is accompanied by two plates, the second of which, representing the dissection of an amputated leg, illustrates remarkably well how these growths are connected with the fasciæ and the tendons of muscles.

XI. *Contributions to Dental Pathology.* By S. James A. Salter, M. B., F. L. S., &c.

These contributions are three in number. The first is upon the Shedding of Teeth and Exfoliation of the Alveolar Processes, consequent upon the Eruptive Fevers.

Mr. Salter states that, during the past few years, he has had under his care a number of cases in which necrosis and exfoliation of the alveolar processes of the maxillæ, accompanied by shedding of the contained teeth, has been one of the secondary consequences resulting from attacks of the eruptive fevers—scarlet fever, measles, and smallpox. After scarlet fever there were some eight or ten cases, after measles three or four, and after smallpox only one. Though following different forms of eruptive disease, they were all so singularly uniform in their origin, course, and entire history, that, in Mr. Salter's opinion, they would seem to have a generic identity.

In all these cases the first evidence of exfoliation was always apparent within eight or nine weeks after recovery from the eruptive fever, usually within four or five. It was never preceded by pain or swelling, or accompanied by periosteal abscess; the suppuration always occurred at the part where the gum peels

from the alveolus, which appears to be the simple method of exposing the dead bone. Whichever jaw has been the subject of exfoliation, it usually occurs on both sides. It occurs in patients about five or six years of age, during the time that the most active tooth-development is going on in the jaws. In every instance the temporary molar or molars, and the corresponding bicuspid or bicuspids, with their containing alveoli and loculi, have been the parts to suffer. In no one case was the shedding of teeth confined to the temporary set; the successional (bicuspids) have always been involved, and shed also.

The occurrence of these cases is explained by Mr. Salter as depending upon the fact that the teeth are modified papillæ, are members of the tegumentary system, and that the *materies morbi* in the eruptive fever affects them by virtue of their being dermal or tegumentary organs. Blighted and destroyed, they light up in the alveolar periosteum an inflammation which, while it is destructive, is curative; while it destroys the bone, it accomplishes the casting off of effete and dead organs.

The second contribution is on the subject of Warty Teeth. The teeth sometimes, though very rarely, become *warty*, that is, have compound papillary projections attached to them. The explanation of such specimens is obvious: the superficies of a tooth is the part first formed, and it corresponds in every particular in shape with the original formative pulp, so that it follows that the pulp must in these cases have borne the same relation to a normal pulp as a tegumentary wart does to normal papillary structure.

It is worthy of remembrance by the surgeon, that teeth occasionally exhibit such change of form and aspect that they can no longer be recognized as teeth, and that they may be mistaken for necrosed bone. In one of the cases recorded by Mr. Salter, an eminent metropolitan surgeon, in order to extirpate what appeared to be a piece of porous bone denuded of periosteum, removed the angle of the man's jaw (a young man, twenty years of age), and thus put an end forever to efficient mastication, when the whole of the ungainly mass could have been readily extracted with a pair of forceps.

The third of these interesting contributions is on Polypus of the Tooth-Pulp. In this affection the pulp of the tooth sprouts into a mass having very much the aspect and about the same sensitiveness as the gum. The tooth itself is never the subject of odontalgia, and is always imperfectly calcified, "presenting that peculiar globular calcification in which the substance of the dentine becomes rapidly sodden with saliva, and carious without limit from the enamel to the pulp." It is believed by Mr. Salter that the changes in the pulp—hypertrophy, with structural modification—are owing to some influence exerted upon it from the presence of the saliva.

This communication is accompanied by two lithographic plates containing numerous figures, representing warty excrescences on the teeth, and polypus of the tooth-pulp, as they appear to the naked eye, and also under the microscope. Two wood-cuts in the text illustrate the article on shedding of the teeth and exfoliation of the alveolar processes.

XII. 1. *On the Alleged Sugar-forming Function of the Liver.* 2. *The Influence of Diet on the Liver.* By F. W. Pavy, M. D., *London.*—This communication is one of exceeding interest and importance, certainly in these respects not second to any in the whole volume. The experiments detailed by Dr. Pavy, and the deductions he draws from them will aid materially in the solution of the problems that now occupy most the attention of physiologists. From the care and skill with which the experiments were evidently made, and the great number of times they were repeated, we must rely upon the results uniformly obtained by them. The deductions drawn from them are fully warranted, indeed they are forced upon us. A little reflection too will show clearly that they are not only not opposed to the facts obtained by Bernard and others, in their experiments, but entirely in accordance with these facts, and only opposed to the *interpretation* hitherto given to them.

Experiment has proved, beyond a doubt, that a large quantity of sugar is met with in the liver, and in the blood of the hepatic veins, and of the right side of the heart of an animal that has been for some time previously restricted from

the ingestion of saccharine materials, *after the death of the animal.* The question Dr. Pavy desires to solve is whether sugar is natural to the right-ventricular blood of the *living animal* and to the *living liver*, for to find sugar on examining the blood or the liver after death, is no proof that it was also present during life. The results he has obtained in endeavouring to solve this question are very striking, and lead one irresistibly to the conclusion that during life sugar does not exist in the liver, but a substance which happens to be with extreme facility, by a process allied to fermentation, convertible into sugar, and which has the power to resist transformation into sugar, whilst located in the tissue of the living and healthy liver. With the destruction of life this power of resistance is at an end, and the organ then becomes speedily charged with the saccharine principle.

When blood was removed by Dr. Pavy during life from the right ventricle by means of a catheter passed into the right jugular vein, it was found to contain only the merest trace of sugar, the reaction with the Barresvil solution being sometimes so slight as to be liable to be overlooked altogether. Dr. Pavy made upwards of sixty observations to establish this fact. When in these cases the animal was killed, the blood collected after death from the right ventricle occasioned with the same reagent an abundant orange-yellow precipitate of suboxide of copper.

When an animal is pithed, and a minute or two is allowed to elapse before the chest is opened, the blood flowing from an incision into the right side of the heart will be found strongly saccharine. When the chest was instantly opened by Dr. Pavy after the pithing was effected, and a ligature placed around the base of the heart, the contents of the right ventricle were found to be free from sugar.

Having discovered that the sugar-forming substance of the liver was not transformed into sugar, whilst in contact with a ferment, when an alkali was present, Dr. Pavy immediately after death injected a strong solution of potash into the liver of a dog. The presence of sugar was in vain sought for, either in the liver or the contents of the circulatory system. Again injecting only half of a liver, in the same manner, this half contained no sugar, while the other gave the ordinary amount. When the organ was allowed to remain a few moments after death before the injection was practised, so as to give time for the *post-mortem* transformation of liver material into sugar to take place, the presence of sugar was as easily shown as if the potash had not been made use of.

Knowing how much changes partaking of the character of fermentation are promoted by an elevated temperature and checked by cold, the influence of a freezing mixture upon a piece of liver removed at once after death was essayed. When an animal was suddenly killed by the destruction of the *medulla oblongata*, and the abdomen instantly opened and a piece of liver cut off and plunged into a mixture of ice and salt—mixed half an hour before, to have it liquid and of a much lower temperature—the *post-mortem* transformation of the liver material was prevented, and an absence of sugar was observed. Bernard had noticed that if the spinal cord be divided above the origin of the brachial nerves, "a peculiar modification was produced in the liver, a sort of perversion of secretion, or rather a veritable arrest in the series of transformations that take place in the liver in order to change into sugar the albuminoid matters of the blood." (*Leçons de Physiologie Expérimentale, &c.*, Cours du Semestre d'hiver, 1854–1855, page 374.) In the experiment he relates, the animal was killed the day after the division of the cord; a decoction of a part of the liver was made at the very moment of the autopsy, and not the slightest trace of saccharine matter was detected; a portion of the same liver left to itself was taken twenty-four hours afterwards, and it contained an enormous quantity. After making an hypothesis to explain this, he makes a second one which, he adds, is more probable than the first. "This new hypothesis would consist in saying that the transformations that take place in the liver require, not only nervous influence, but also a sufficient temperature, and that the temperature of the animal (a rabbit) before you being no more than 24° (75° Fah.) at the time it was sacrificed it is not sufficient for the accomplishment of the phenomena in question. You know, in fact, that certain chemical actions cannot take place without a certain quantity of heat, and that in animals whose temperature varies, they cease when the cooling has reached a certain point, although the nervous excita-

bility should then appear more intense, as takes place precisely in the rabbits in which we have divided the spinal cord. It may be the same for the glycogenic function; the temperature to which the animal has fallen may be no longer sufficient for determining the series of transformations that finally give sugar; and what proves that heat favours this formation is this, that after having removed the liver thus modified, if you expose it to a high enough temperature, the sugar is soon seen to appear in the tissue. *It would probably be the same if, without extracting the hepatic organ, the animal were placed in a situation where its normal temperature, which is* 37 *or* 40 *degrees* (99° or 106° Fah.) *would be maintained without allowing the cooling to take place.* If, on the contrary, the liver should be exposed to a low temperature, sugar would form therein but very slowly." (Pp. 378–9.) We have extracted the whole of the passage, for it is interesting to notice how M. Bernard's hypothesis coincides with Dr. Pavy's manner of interpreting these phenomena. The experiment suggested in the part we have italicized, was performed by Dr. Pavy, and a rabbit, after having its spinal cord divided as above said, was placed over an engine boiler where the thermometer stood at 88°. At the end of three hours the animal was killed, and the indication of sugar in the liver was as strong as if the specimen had been derived from an animal which had been suddenly sacrificed without having been submitted to any previous operation. Dr. Pavy found, also, that when the temperature of the animal was reduced by other means than by the division of the spinal cord, the same condition of the liver was met with. By oiling the coats of animals so as to deprive them of their non-conducting investment, and exposing them to cold, their temperature rapidly falls; if killed when the temperature had descended to about 70° or 80°, the liver was found free from sugar immediately subsequently to death, and became strongly saccharine afterwards, precisely as in the case of section of the cord.

With frogs, Dr. Pavy found in his experiments that when they were killed at an elevated temperature the *post-mortem* transformation of liver material into sugar takes place—as in warm-blooded animal—so rapidly, that sugar is found under the ordinary process of examination. When, on the other hand, the animal possesses a low degree of temperature at the time of death, the *post-mortem* formation of sugar is so retarded, that an examination of the liver is easily effected before a change has taken place to any considerable extent. M. Bernard, it may be interesting to state, obtained precisely the same results, and communicated them to the French Academy in March last; he attributes, however, these results to the "*ralentissement de la circulation qui est lié à l'abaissement de la température!*"

From a consideration of the facts brought forward by Dr. Pavy, we think it must be concluded with him that though the liver and the right ventricular blood are strongly saccharine *after death*, yet that this is not their condition *during life*. It would appear that there is formed in the liver a substance which is convertible with extreme facility into sugar, by a process allied to fermentation; that this substance seems to have the power to resist this transformation while located in the tissue of the living and healthy liver, but with the destruction of life this power of resistance is at an end, and the organ then becomes speedily charged with the saccharine principle. Until other facts are made known, we feel inclined to accept the conclusions of Dr. Pavy, which not only appear to be true themselves, but also enable us to explain the difference in the results obtained by other experimenters. It may be added that, in accordance with them, diabetes must be considered as dependent upon some defect in the functional performance of the processes of the liver, owing to which the substance naturally produced by it is incapable of resisting transformation into sugar, as it does under normal circumstances.

The second part of this communication is on the Influence of Diet on the Liver; and before giving an abstract of its contents, as we have just done of the first part, we will, on account of the interest now attached to the subject, recall, in as short a space as we can, the present state of the science on this point, at least so far as the influence of diet upon the quantity of sugar-transformable material in the liver is concerned. M. Bernard, in a memoir read before the Academy of Sciences, March 23, 1857, maintains that "the liver of dogs nou-

rished exclusively with meats possesses the property, special and peculiar to that one organ, of creating a glycogenous matter analogous to vegetable starch, and having the power, as it, of becoming afterwards transformed into sugar, in passing by an intermediary state to that of dextrine. In herbivorous animals the glycogenous matter is likewise produced in the liver, but he does not doubt that some is also formed at the expense of amylaceous matters." The most important paper that has appeared in contradiction to these doctrines of M. Bernard is that of M. Sanson, entitled "On the Origin of Sugar in the Animal Economy," published in Brown-Séquard's *Journal de Physiologie*, for April, 1858. According to M. Sanson, it is established, by the results of his experiments, that the source of sugar in the animal economy is always exterior, and resides exclusively in the alimentation, whether this be of a vegetable or an animal nature. "In the normal conditions of their existence," says M. Sanson, "herbivorous animals find the amylaceous principle in the vegetables by which they are nourished, and where it is, as we know, elaborated in great abundance. They transpose it into dextrine, a part of which undergoes, in their own economy, the whole series of metamorphoses by which this principle must pass in order to serve for the support of life, and be destroyed and eliminated; the rest of this same dextrine is accumulated in their tissues, or, rather, is incessantly present, by the fact of the constant access of the blood that contains it in excess relatively to their physiological necessities. The carnivorous, in their turn, to the alimentation of which these are destined in the far-seeing economy of nature, find in this excess of dextrine the primitive element of the sugar necessary for the accomplishment of their functions, and consequently are not forced to elaborate it." (*Loc. cit.*, pp. 273, 274.)

This paper of M. Sanson was the occasion of a report to the Academy of Medicine, in which M. Poggiale, the reporter, says:—

"In the present state of the science nothing seems better demonstrated than this, that the glycogenous matter met with so abundantly in the liver of carnivorous animals is not furnished by their food. Whatever be the alimentation, whatever be the aliments on which we operate, a great quantity of sugar is constantly found in the liver, while its presence in other parts of the organism is accidental, and is only due to certain physiological conditions that can most generally be modified. The glycogenous matter is only found in the other organs of herbivorous animals when these animals are nourished with aliments rich in amylaceous substances."

The views of M. Sanson are not sustained by the experiments of Dr. Pavy, which would show that the so-called glycogene belongs particularly to the liver, and is secreted by it from saccharine, amylaceous and other principles contained in the blood circulating through its capillaries. The facts which he has brought forward, showing that sugar does not belong to the healthy liver during *life*, alter, however, the position of this subject so as to render unnecessary the discussion of whether the production of sugar results from a process of secretion or of a simple chemical transformation. Whatever in future may be definitely shown to be the particular purpose of glycogene, or hepatine, as Dr. Pavy called it with more propriety, as it is not *sugar producing*, in the economy of life, it certainly does not appear to be formed, as has been thought, for the object of transformation into sugar.

In experimenting upon the influence of diet upon the liver, Dr. Pavy found that the size of this organ is influenced to a most astonishing extent by the kind of food that is given; and that the alteration it thus undergoes is chiefly, if not entirely, due to the relative amount of hepatine present. The processes by which he obtained his results are carefully detailed, particularly the manner of collecting the hepatine, and they appear to us to be deserving of all confidence. The exact composition of hepatine he states that he is not acquainted with; and as it is a matter of some interest we will give it here, on the authority of M. Pelouse, as carbon 39.8, hydrogen 6.1, oxygen 54.1; corresponding to the formula $C_{12} H_{12} O_{12}$.

When dogs were kept exclusively on a diet of animal food, the relative weight of the liver to the animal was 1–30; and the relative amount of hepatine in the liver was 6.97 per cent. When they were kept upon a diet of vegetable food

the relative weight of the liver was 1–15; and the quantity of hepatine was 17.23 per cent. When they were fed upon animal food with an admixture of sugar, the relative weight of the liver to the body was 1–16½; and the amount of hepatine yielded was 14.5 per cent. Some experiments which Dr. Pavy records, that he has made upon rabbits, are strikingly corroborative of the deductions he has drawn from the observations on the dog.

If this hepatine is not naturally formed for the purpose of transformation into sugar, the question as to what naturally becomes of it, is most interesting. Dr. Pavy believes that in its solution attention will have to be specially directed to the bile and to the production of fat, a material which is undoubtedly largely formed, particularly under certain kinds of vegetable food, in the animal system.

XIII. *On Poisoning by Nicotina, with Remarks.* By Alfred S. Taylor, M. D., F. R. S.

In this communication Dr. Taylor gives the history of a case where a gentleman committed suicide by nicotina, with an account of the manner in which this substance was sought for and detected in the body, and some very interesting considerations upon its action as a poison.

The only other case of poisoning by nicotina on record, is that which occurred about eight years ago in Belgium, where a M. Fouguies was murdered by the Count and Countess Bocarmé, his brother-in-law and sister, while dining with them in the chateau of Bitremont. In that instance, M. Stas conducted the chemical investigation, and succeeded in detecting the poison in small quantity in the tongue and fauces, stomach, liver, lungs, and in a wooden plank of the floor of the room in which the deceased was sitting.

In his case, in searching for the poison, Dr. Taylor made use of the plan pursued by M. Stas, and also of that advised by Orfila. By both nicotina was detected, but that of M. Stas enabled a greater quantity to be extracted.

As to the action of nicotina in destroying life, Dr. Taylor believes that the degree of purity of the nicotina employed may, in some measure, account for the different conclusions arrived at by experimenters, some finding it to act exclusively on the muscular system, while others contend that the circulation alone is directly affected. M. Bernard, it will be recollected, found that when pure it spent its effects chiefly on the capillary circulation, while, when partially decomposed, the functions of the heart and lungs were directly affected. (*Leçons sur l'Effet des Substances Toxiques et Médicamenteuses*, p. 397.)

This paper of Dr. Taylor, as all that he publishes of the kind, is of the highest importance in a medico-legal point of view. W. F. A.

Art. XX.—*A Treatise on the Human Skeleton—(including the Joints).*—By George Murray Humphry, Esq., M. B. Cantab., F. R. C. S., Surgeon to Addenbrooke's Hospital, Lecturer on Surgery and Anatomy in Cambridge University Medical School. 8vo. p. 620: Cambridge, Macmillan & Co., 1858.

Mr. Humphry, in the preface to his work upon the Human Skeleton, informs us that it has always been to him a matter of regret, that the study of the bones should be regarded as dry and tedious. In reality it is, and ought to be, one of the most interesting of all studies, essential, as he remarks, alike to the practical surgeon, and to the philosophical anatomist. Looked upon, therefore, in this light, we cannot help agreeing with the author, that the framework of the human system deserves a more minute investigation, and a more careful study than is usually bestowed upon it.

We do not intend in our brief space to enter upon any very critical examination of Mr. Humphry's somewhat voluminous work; our object is rather to draw attention to the manner in which the author himself has considered his subject, leaving to those of our readers who may be more particularly interested in the matter, the task of passing in detail the various topics considered in the book before us.

The earlier pages of the work are devoted principally to general observations upon the skeleton, including the chemical composition, the shape, structure, and physical characteristics of bones, their development and mode of growth; the nature and uses of the periosteum, and the peculiarities of the senile skeleton. Into the latter topic Mr. Humphry enters at some length, and furnishes us with much valuable information relative to the changes which occur in bones during the advanced periods of life.

It has always been a prevalent opinion, that the proportion of the earthy constituents of the bones gradually increased with increasing age, and that the change of structure in bone thus brought about, acted as one of the most powerful predisposing causes of fracture in the aged. The various statements, however, upon this subject, have been conflicting in the extreme, and the only certain result arrived at by our author, is that "the bones of the aged differ a good deal in different individuals."

This want of uniformity in the characters of the bones of old people is probably to be explained by the constantly varying relations of absorption and deposit. In one class of cases, these processes being nearly balanced, but little alteration, save in the enlarged condition of the cancelli results; in other instances the bony deposit may exceed the process of absorption, so that the bone may become absolutely increased in weight, hardness, and solidity. The third and most numerous class of persons are those in whom the absorption goes on with greater rapidity than the deposit, producing that brittle condition of the bones already alluded to. Be the explanation of the fact what it may, it seems certain, however, that in many cases the earthy constituents are not relatively increased, a circumstance sufficiently proven by the experiments of Stark and Von Bibra, who found that the chemical constituents of old, and of adult bone are often relatively the same.

Were we asked to point out any portion of Mr. Humphry's work, which possessed peculiar interest, we would unhesitatingly lay our finger upon those pages occupied with the consideration of the joints. The examination of these parts has evidently been conducted by the author with the most rigid scrutiny, and the reader is presented with many anatomical facts which are not frequently alluded to. Such for example are the remarks upon the influence of atmospheric pressure on the joints, especially the ball and socket-joints; the fact that the bones are in no instance held together by their shape alone; that the retention of the articular surfaces of the joints in apposition depends entirely upon the ligaments and surrounding soft parts; and that the movements of the joints are under no circumstances arrested or limited by the shape of the bones, or by the contact of their edges. The checking of the movements in every articulation is produced either by the tension of the ligaments, or by the pressure of the soft parts; this can be clearly seen by examining the elbow-joint, in which, although the coronoid and olecranon processes enter, during flexion and extension their respective fossæ, yet they are never permitted quite to reach the bottom of these cavities. All jarring shock to the limb is thus prevented, and the movements of the joint are executed with safety and precision.

The much vexed topic of the structure of articular cartilage is closely examined by Mr. Humphry. He believes it to be non-vascular, and that only in morbid conditions have vessels ever been traced into it. The effect of disease upon this structure is also alluded to, and we are told "It is worthy of remark in a practical point of view, how quickly the muscles that act upon the joint fall away when disease attacks its cartilages. A year of simple synovial disease will not produce so much effect upon the adjacent muscles as a month or even a week of ulceration of the cartilages. Hence this wasting of the muscles becomes a symptom of great importance in assisting us to arrive at a diagnosis as to which of these two structures is involved, as well as in enabling us to decide whether disease, which commenced in the synovial membane, has extended to the cartilages"—p. 77.

In the chapter upon the proportions of the human figure, we find many curious analogies. One of these we subjoin, as to some of our readers it may be novel and interesting.

When speaking of the relative size of the head and trunk of the body, our

author informs us that the great modern artists have been in the habit of making the head about one-eighth part, and the face about one-tenth of the whole figure, giving to the lower extremities one-half the length of the body, and to the extended arms, the same distance as from the sole to the crown. He then adds: "In like manner the height of the columns in the various styles of architecture, and the dimensions of their capitals were regulated in certain definite proportions to the diameter of the several columns. The radius of the base was usually taken as the standard of measurement, and called the "module." Thus the Tuscan column measured 16 modules, the Ionic 18, and the Corinthian 20. Gradually, as the science of architecture made progress, the columns were rendered lighter and more graceful, and it is interesting to observe that the several parts were elongated, until the column, with its capital and base, acquired nearly the proportions of the human frame. Considering the capital as in the place of the head, the whole length of a Corinthian pillar is eight and a half heads." The "module" or standard proposed by Carus for estimating the comparative proportions of the human figure is one-third of the length of the spinal column, and with this ratio of measurement, the various portions of the body have been found to correspond in a remarkable manner.

Passing from the subject of general considerations, our author next takes up the anatomy of the various bones, commencing with the vertebræ and the spinal column. A point of interest connected with this latter is the position of the so-called "weak-points." The first of these is the junction of the dorsal with the lumbar portion of the column. The reasons for the existence of this weak point are stated by Mr. Humphry to be, that although it has to bear nearly as great a weight as the part of the column below it, its vertebræ are proportionately small; then its transverse and spinous processes are short; and again it is near the centre of the spinal column, and subjected to a great length of leverage. Lastly, he observes, that the component parts of the spinal column, both above and below, are comparatively fixtures, so that all movements are transmitted to and felt at the point in question, producing that disagreeable jar, or shock, or, as it is commonly termed, a strain in the back, so frequently experienced as the consequence of a misstep. Another weak point is found at the dorsal curve; here is the usual seat of angular curvature, the result of absorption or ulceration of the bodies of the middle dorsal vertebræ. The senile curvature, so frequently the accompaniment of old age, is produced by the gradual weakening of the various dorsal ligaments, and the resulting absorption of the foreparts of the bodies of the vertebra.

The description of the individual bones of the head forms a long and well written chapter of the work; the tediousness incidental to such minute description being well relieved by the interspersion of practical physiological and pathological facts. Especially is this the case where the author is treating of the various sinuses of the skull, and the meatuses of the nose.

At page 301, we have introduced to our notice a new ligament, the short internal lateral ligament of the temporo-maxillary articulation. This ligament, which, according to Mr. Humphry, has escaped the attention of anatomists, arises immediately underneath the long internal ligament, and passes to be inserted upon the ridge leading from the inner extremity of the condyle, and immediately behind the insertion of the external pterygoid muscle. At page 327, when treating of the imperfections of the sternum, the case of M. Groux, so familiar to many of our readers, is referred to. Clefts in the sternum are, we are told, more frequently met with in the lower than in the upper portion of the bone, a fact in accordance with the mode of closure of the thoracic cavity of the fœtus. A point of interest in the case above referred to, is the existence of the soft parts over the sternum; since it most frequently happens in cases of congenital fissure of this bone, that the skin and tissues which should cover it, are also deficient.

The mode in which the movements of inspiration and expiration are effected, are minutely discussed, and the relations of the thoracic walls to each other during these acts are illustrated by a series of diagrams. The remaining portions of the volume are occupied with the study of the bones of the extremities, and frequent and apt allusions are made to the injuries to which these parts and

those adjacent are subject, as fractures, dislocations, and injury or rupture of the surrounding muscles. The ligaments of the shoulder, and hip-joint, have received, as their importance well warrants, a full share of our author's attention, and we do not remember to have elsewhere seen a more truthful or more accurate description of the former than has been afforded us by Mr. Humphry.

In bringing our brief notice of this very admirable volume to a close, we cannot but express the satisfaction we have derived from the perusal of many of its pages. The book issued from a Cambridge press is essentially a volume of luxury, and is, we believe, the most voluminous treatise on the anatomy of the skeleton yet published. The only recent treatise with which it can be well compared is that of Mr. Holden; this latter, however, is confined simply to the description of the bones, and does not, we think, include the examination of the joints. The plates, however, in Mr. Holden's work, are superior to those of the volume we have just noticed. Indeed, it had been our intention to advert more strongly to the character of Mr. Humphry's illustrations, but as we are told in the preface, that the artist is the author's wife, gallantry forbids us so to do. We doubt not, that should the work in question ever reach a second edition, many alterations in the engravings will have suggested themselves to the author.

J. H. B.

ART. XXI.—*The Microscope in its application to Practical Medicine.* By LIONEL S. BEALE, M. B., F. R. S., Professor of Physiology and General and Morbid Anatomy in King's College. 2d edition, 8vo.: London, 1858.

THE physician who at the present day decries the use of the microscope, and tauntingly asks what it has accomplished for practical medicine, is possessed of a degree of boldness, which, resulting as it does from ignorance and prejudice, is not entitled to the respect which ordinarily is accorded to this element of character. And yet, notwithstanding the thousand evidences that a material portion of our recent progress in medicine is due to this instrument, we find those (many of them occupying high positions in the profession) who with an obstinacy worthy of a better cause refuse to be enlightened, and are content to grope their way in the venerated darkness of the past. No arguments reach them, no evidence convinces them, no inducements persuade them. But thus it has always been with every important discovery. Harvey was ridiculed and persecuted, and in the time of Galileo there were not wanting those who could see more stars with the naked eye than with a telescope. We smile now at such recollections, and those who decry the microscope smile with us, little dreaming that the time is at hand when they will be classed with the opponents of the telescope, the steam engine, the magnetic telegraph, and other great works which have triumphed in spite of opposition.

It would of course be foreign to our purpose were we in this place to refer in detail to the long list of valuable discoveries which medical science has received through microscopical investigation. They are to be found in the standard works on anatomy, physiology, pathology, and that science of microscopical creation—histology.

But although the microscope has done much for our profession, there is still a great deal for it to accomplish; and, therefore, we hail with pleasure the appearance of every work calculated to lighten the labour of investigation, and to instruct those desirous of devoting themselves to the task of elucidating subjects as yet imperfectly understood.

This volume of Dr. Beale's must undoubtedly prove useful to those engaged in microscopical observations. The author is so earnest, so thoroughly imbued with love for the subjects of his scientific labours, that with his experience he could not fail to produce a work useful to those following kindred pursuits. It is also decidedly superior in every respect to the first edition, in which all reference to several important subjects was omitted.

In using the microscope there are so many precautions to be taken, so many causes of misinterpretation to be avoided, that the greatest care should be

exercised by every observer before deciding definitely upon the value of the appearances presented. Dr. Beale, we think, does not lay sufficient stress upon these points, and so far we regard his work as being defective.

The range of subjects considered by Dr. Beale is very extensive, and, perhaps, more so than is requisite in a work devoted, as its title imports, to instruction in the use of the microscope in clinical investigation. Thus, the art of lithography, the respiratory apparatus of the lower animals, the kidney of the horse, the specific gravity of the brain, &c., though all useful and important subjects of inquiry, are, we think, somewhat out of place in a work of the character of that before us. It is difficult, however, to quarrel with an author for giving us more than he promises.

The volume is divided into three parts. Part I. treats of *the apparatus necessary for the examination of objects of interest in a clinical point of view, of the practical operations required for demonstrating objects, and of recording the appearances observed.* This division occupies about a third of the work, and contains a great deal of matter useful to the student. We are glad to see the stress laid by Dr. Beale upon the advantages to be derived from submitting objects under microscopical observation to the action of chemical reagents—micro-chemistry, as it is called. By neglecting the use of chemical tests, the microscopical investigator will constantly fall into errors, some of which will certainly not conduce to the formation of an exalted opinion of his perceptive faculties.

In Part II. *the demonstration of the microscopical characters of tissues in health and disease, morbid growths and deposits, the fluid products of disease, and animal and vegetable parasites,* are considered. In this section the various parts and organs of the body are brought under notice, and their microscopical characters described. In addition to the other subjects mentioned in the heading, the various secretions and excretions are considered.

The illustrations which abound in the volume are in general well executed. We must, however, protest against the deception practised in the assertion on the title page that there are 270 wood cuts. Certainly most if not all persons would infer from the statement that there were 270 *different* cuts, whereas many of them are duplicated, and even in some cases triplicated. For instance, figs. 1 and 2 are reproduced as 211 and 213, fig. 3 as 210, fig. 4 as 261, fig. 5 as 177, fig. 6 as 183, fig. 9 as 137 and 217, in which latter it is inverted in order to mystify it a little, fig. 68 as 92 and 101 and 102, fig. 75 as 239, &c. &c. This plan is so thoroughly carried out as to reduce the number of different cuts nearly one half. Space is thus occupied which should have been more profitably filled, or else the size of the book should have been reduced and its cost lessened. Many of the cuts, moreover, are of the most trifling character, and altogether useless.

We must also notice the style of composition, which is vicious in the extreme, and, from its undignified character, is entirely unbefitting a work pretending to a scientific status. We cite one or two examples of inelegance of composition, premising that similar instances are to be met with on almost every page. At page 161 the author informs us that "perhaps the best organ for examining the structure of basement membrane is the kidney." At page 283 starch *globules* are mentioned; and at page 352 he states that "fungus hæmatodes is applied to any soft, highly vascular, bleeding fungoid growth."

Dr. Beale's obligations to his friends are so frequently expressed for the most trifling favours, as to lead to the supposition that notoriety is at a premium amongst his acquaintances. Thus, he thanks "Mr. Spratly" for specimens; his "friend Dr. Scott Alison" sent another; his "friend Dr. Sankey" furnished some cystine; his "friend Mr. Cubitt" forwarded a specimen of chylous urine; his "friend Mr. Masters" also sent a specimen of urine; and we might refer to numerous other expressions of Dr. Beale's remembrance.

Notwithstanding the manner in which Dr. Beale's book is written, it contains, as we have already stated, a great deal of valuable information, and fills a place in microscopical literature which should not be allowed to remain vacant. We are, therefore, for want of a better treatise on the subject, forced, in a measure, to recommend the volume before us to those commencing microscopical investigations, trusting that ere long we may be enabled to ask attention to one more worthy of commendation. W. A. H.

Art. XXII.—*Nutrition in Health and Disease.* By James Henry Bennet, M. D., &c. London, 1858. 8vo. pp. 210.

This is a small book on a great subject, a subject upon which, at the present time, too many good books can scarcely be written, for with all our progress in medicine, physiology, and dietetics, with all the positive knowledge we have acquired on these subjects, we have really advanced but little towards a thorough understanding of the functions connected with nutrition; and, worst of all, those points which *have* been established are practically disregarded by those who know better, and are rarely enforced upon the ignorant, either by precept or example, by those whose duty it is to know and teach them.

Until, however, we have definitely ascertained the functions of the several organs concerned with the digestive process, the value of the several alimentary substances as nutritive agents, the circumstances which influence the course of the progressive and regressive metamorphosis of the tissues, together with several other points of minor importance, we cannot expect to establish many fixed rules for our guidance, still less to lay down the broad fundamental principles which govern this most important process of organic beings.

Much has been accomplished within the last decade, much is now being made clear to our minds, but still more remains to be performed. Experimental research, the chief means by which we are to succeed, must be more skilfully and carefully conducted than heretofore, and, above all, the number of earnest workers must be increased. The prize is worth all the efforts which centuries of time can produce, for, when we are able to propound the laws which govern the nutrition of the body, the other mysteries of organic life will not long remain unfolded.

This little book of Dr. Bennet's, though not a positive contribution to science, is yet, in many respects, a useful one, as exhibiting in a plain manner, and with tolerable correctness, the present condition of our knowledge of the nutritive processes, the derangements to which these processes are liable, with, in general, sound directions for the guidance of the appetite, and for the relief of the disorders consequent upon indiscretion or hereditary predisposition.

The first chapter treats of digestion and nutrition in health—of solid food and the actions of the several digestive secretions upon it—of the purposes which it subserves in the economy according as it is heat-producing or tissue-forming—and of the various kinds of liquid food, and of their elaboration and destination.

There is nothing in this chapter at all in advance of what is already known, and, upon the whole, it fails to exhibit a satisfactory view of our actual knowledge upon the subjects considered. The old division of food into respiratory or carbonaceous, and histogenetic or nitrogenous, is given without modification. Although in the main this division is convenient, and to a certain extent correct, it can no longer be regarded as rigidly existing. Experiments, both on the other and this side the Atlantic, have positively determined that life, with the normal amount of heat, can be sustained for a considerable period on food of the most ultra nitrogenous character. Dr. Bennet is, we think, disposed to over-estimate the importance of nitrogen as a tissue-forming substance. The statement is made that the framework of an animal is, to a great extent, composed of nitrogen, whereas, in those tissues of the most proteinaceous character, it does not exceed 16 per cent. of the dry substance; and in other tissues, as the bones for instance, it falls far below this ratio. Though essential to the composition of all the tissues it is certainly entitled to no pre-eminence over oxygen or carbon, or even over the purely inorganic elements of their substance.

In treating of the digestive fluids and their uses there are several misstatements which it is not proper to pass without notice. Thus, it is asserted that the saliva and pancreatic juice are identical in physical properties and chemical composition. In order that our readers may see for themselves how exact Dr. Bennet is in this statement, we subjoin the following tables of Bidder and

Schmidt,[1] showing the composition of these fluids, merely premising that the work from which they are taken was published at least six years before Dr. Bennet's, and is remarkable for the accuracy and extent of the investigations contained therein:—

SALIVA.		PANCREATIC JUICE.	
Water	995.16	Water	900.76
Organic matter	1.34	Organic matter	90.38
Sulpho-cyanide of potassium	0.06	Chloride of sodium and other inorganic salts	8.86
Epithelium	1.62		
Inorganic salts	1.82		
	1000.00		1000.00

Certainly no two fluids, at all approaching each other in composition, could be much more dissimilar, and, we may add, that their physical characters are just as unlike.

No reference is made to the action of the intestinal juice in digestion, and there are one or two other points to which we might allude as not being correctly stated.

With the exceptions noticed, this chapter forms a very suitable introduction to the main topics of the volume.

In the second and third chapters nutrition is considered in a more general manner, and the nutritive requirements of man with reference to temperature, climate, and occupation pointed out. Nutrition as it is effected in the lower animals, and in plants, is also touched upon.

These chapters appear to have been written with a good deal of care, and, to a great extent, are free from the inaccuracies of the first chapter. The true value of the accessory aliments, tea, coffee, alcoholic liquors, spices, tobacco, &c., is recognized by Dr. Bennet, though the evils of their excessive use are not the less indicated.

The fourth chapter treats of defective nutrition, and contains a good deal of practical information well arranged, and communicated in an easy and agreeable manner. The divisions and subdivisions of this chapter are well conceived, but there is occasionally an assumption of theory for fact and a looseness of expression, which, in spite of the many good points contained in it, are calculated to put the reader in a bad humour with the author.

Thus, for instance, when speaking of the formation of an excessive amount of fat from over-feeding, constitutional predisposition, or defective nutritive power, Dr. Bennet says, the fat is first deposited between the muscular fibres, but, if the deposition be not checked, it breaks up the muscular fibres and takes its place, thus, as he says, giving rise to fatty degeneration. Now, we conceive this to be an inexact statement of the process by which the muscular fibre is *converted* into fat, which *conversion* alone constitutes true fatty degeneration. The difference between *fatty deposit* and *fatty degeneration* is so simple that we wonder Dr. Bennet should not have more plainly perceived and expressed it. We do not believe that the latter results from over-feeding, whilst there is little doubt that the ingestion of an excessive amount of fatty food, or of food capable of being converted into fat, will assuredly lead to fatty deposit. The true cause of the retrogression of myolin into fat, which action alone constitutes fatty degeneration, is deficient exercise. The muscles of a paralyzed limb will, after a short time, be found in this condition, and the difference in colour and consistency between the flesh of the breast and legs of the common fowl is mainly, if not entirely, owing to the fact, that the former is in a state of fatty degeneration produced by disuse.

Urate of ammonia is regarded by Dr. Bennet as being present in the urine in abnormal amount if deposited by a moderate reduction of temperature, and the uric acid is deemed to be in excess if in larger quantity than eight grains. As we know that these substances, together with almost every other constituent of

[1] Die Verdauungssæfte und der Stoffwechpel, pp. 10 and 245.

the urine, may, within the strict limits of health, undergo considerable variation in quantity through the operation of other factors than that of defective nutrition, we cannot consent to receive Dr. Bennet's opinions for facts.

The fifth chapter contains practical deductions from the facts and opinions previously stated, and the sixth and last relates to confirmed dyspepsia. The advice given to those labouring under this disease is almost invariably of a sound and practical character, and such, that if consistently followed out, can scarcely fail to relieve the most inveterate case.

In our notice of Dr. Bennet's little work we have mainly called attention to what we consider to be its faults; knowing full well that the good points and conservative doctrines, which are met with on every page, will require no notice from us to render them apparent to the reader. W. A. H.

ART. XXIII.—*Syllabus of the Course of Lectures on Medical Logic, delivered in Marischal College and University, Aberdeen.* By FRANCIS OGSTON, M. D., Professor of Logic and Medical Jurisprudence. Edinburgh, 1858. 12mo. pp. 45.

RECENTLY there was founded, by endowment, in the Marischal College of Aberdeen, Scotland, a new chair exclusively devoted to the subject of *medical logic,* to which chair Dr. Ogston has been called, who commenced his first course of lectures in the autumn of 1858.

Of the importance of medical logic to the physician there cannot be a question in the mind of any one who has paid any particular attention to the subject. In the application of the principles of a correct logic to the investigation of the functions of the animal organism, and the phenomena to which these give rise during a state of health or of disease; of the several causes by which these functions are liable to be disturbed, and the manner in which each acts in the production of those conditions to which the term disease has been applied; and of the means adapted to counteract the action of disturbing agencies upon the organs of the living organism, or, when disease has been already induced in the latter, the modes by which it can be best arrested or removed, so that the normal functions may be reinstated, we pursue the only course by which the facts, derived either from a course of experiments or culled at the bedside of our patients in the ordinary routine of professional practice, can be secured from error and rendered available for the instruction of the individual or for the enlargement of the general fund of medical knowledge open to the entire profession.

Mere experience—the simple observance of the phenomena of health and of disease, and the changes in the morbid conditions of the organs after the employment of certain remedies—though the favourite method with the so-called "practical physicians," is valueless without the employment of a correct logic in judging of the character and relations and bearings of the phenomena observed, the tracing these up as far as we are able with our limited knowledge of the nature and laws of vitality, and the mode in which this is modified by external or internal agencies; our experience furnishes us with no absolute, practical information, no knowledge capable of any wide and useful application, in our endeavours to know, distinguish, prevent, or cure diseases, but merely a chaotic mass of disconnected facts, which often appear to be opposed to, and irreconcilable with, each other. The limits of pure observation in medicine are soon reached, and it is only through a proper course of legitimate theorizing that principles in either physiology, etiology, pathology, or therapeutics can be arrived at, that the healing office can be raised from the humble position of a mere art to the standing of a true scientific profession.

The distrust of theory, properly so called, in medicine, which is so frequently expressed, is worse than absurd; it is positively injurious to the onward progress of our profession in the only legitimate road. The union of observation and of speculation is as indispensable to the acquisition of clear views of the nature of disease as it is to the establishment of a successful practice.

In the preparation of his course of lectures on medical logic, Dr. Ogston, find-

ing the only treatise of any note available for his purpose as a text-book to be that of Œsterlin, resolved, for the sake of his pupils, to observe the general arrangement of the author, as being in most respects unobjectionable. Beyond this, however, he found it impossible to confine himself within the limits to which Œsterlin restricts himself in his treatise. In the details of his subject he has availed himself of not only the works of the earlier writers on pure logic, together with such of our best medical authors as have touched incidentally on the science, but also, to a large extent, of the labours of contemporary logicians, amongst whom he would assign the highest place to John Stuart Mill, whose doctrines the lecturer has followed Œsterlin in endeavouring to apply to the particular questions and objects of medical science.

So far as we can judge of the character of the thirty-two lectures of which the course on medical logic of Professor Ogston is composed, by the syllabus before us, it would appear to be a comprehensive, well-arranged, and most instructive one. We trust that the time may not be far distant when there will be founded in the medical schools of the United States a similar chair to that filled by Professor Ogston, from which shall emanate annually an equally full and excellent course of instruction.

D. F. C.

ART. XXIV.—*Demonstrations of Diseases in the Chest, and their Physical Diagnosis.* By HORACE DOBBELL, M. D., Licentiate of the Royal College of Physicians, etc. etc., London: John Churchill, 1858, pp. 165.

WORKS on physical diagnosis have so multiplied of late, and are so often but copies of each other, that we confess to have taken up Dr. Dobbell's work without expecting to find it differing from any other manual on the subject. But a more careful examination convinced us of our error. The plan of the work is to teach the student to connect physical signs with their physical causes, and to enable him to do this the volume is illustrated by coloured drawings giving a faithful representation of the most common lesions. Their corresponding physical signs are placed alongside, and so arranged that the eye can traverse easily from one to the other. The first chapters treat of acoustics, and an attempt is made in them to explain the principles upon which the physical diagnosis of lesions within the chest is based. The production of sound, or rather its reinforcement by unison, resonance, by consonance, by echo, are illustrated by reference to the physical signs, and explained in connection with the doctrines of those authors who found theories of physical signs on them. Something more than understanding the acoustic conditions requisite to the existence of different sounds is, however, necessary. Sounds must be heard to be rightly appreciated or remembered. No description in books, or in the lecture-room will ever take the place of bedside instruction. The author is fully impressed with this. But he has produced a useful book in attempting by a series of plates to associate the conditions causing them with their existence.

The manner in which this is accomplished, is, on the whole, deserving of praise. We meet with nothing very original; the views of prominent authors and their classifications are adopted and explained; the book embraces only the most prominent of the diseases of the lung; but what is given is well arranged and well expressed. Yet while according this praise to the author, we find here and there statements and terms to which we cannot subscribe, and some of which seem to us more calculated to confuse than to enlighten. Such expressions as these "fine crepitation is distinguished from the smallest crepitation (noted in Pls. IV. and V. figs. 13, 16, 18) by its crackling character, whereas in both of these cases the crepitation is bubbling. It must be most scrupulously distinguished from the *small crepitation* of capillary bronchitis," are not conducive of conveying a clear idea to a student, who would not be very likely to see a marked distinction between a *fine* crepitation, and a *small* crepitation.

The following is worse than a fault in the use of terms, it is an error in fact: "In Plate I., fig. 1, the most tangible physical sign is *prolonged expiratory murmur;* but, as in Pl. VII. fig. 24, it accompanies cavities, it cannot be said to

specify consolidation; in Pl. IX. fig. 29, it accompanies inflamed bronchial lining, and hence it does not necessarily indicate cavity. But all three cases will be found to agree in one respect, viz., *that the exit of tidal air* from vesicular lung tissue is impeded." As if the exit of tidal air from vesicular lung structure were the cause of the distinct blowing expiration of cavernous respiration! The changed *rhythm* of the normal vesicular murmur, and the change of *quality* of sound, which determines bronchial or cavernous respiration, are all jumbled together in the most inextricable confusion. We notice also at several places in the book, a mistake which is very common and which has crept into almost all works on diseases of the chest. It is repeatedly stated that the vesicular murmur is weaker on the left than on the right side. This is not correct. The vesicular inspiratory murmur is in the large majority of cases far more distinct on the left side. But there is more expiration at the upper portion of the right lung; hence the error.

There are remarks scattered throughout the volume, to which we turn with more pleasure. The absence of reliable signs of the earliest stage of tubercular deposit is forcibly impressed upon the young practitioner. The importance of teaching the patient how to breathe, and of not attaching to much weight to the prolongation of the expiratory murmur, unless the breathing be regular, is a point of practical value which is well enforced. The author also gives the results of some experiments which may serve to illustrate the difference between *tympanitic* and *amphoric* resonance on percussion. "Tympanitic resonance requires that the cavity percussed shall be full of air, but shall not communicate freely with external air. Amphoric resonance requires that the cavity percussed shall communicate freely with external air." This he demonstrates by sealing up the small hole in the vulcanized rubber balls used as toys, it will then yield a tympanitic resonance. Unseal the hole, and the resonance will still be imperfectly tympanitic. Cut the hole large enough to admit of free communication with the external air and to prevent the hissing produced by the escaping air, and percussion will elicit amphoric resonance. Several other experiments with reference to the varieties of percussion sound are spoken of, and, as they serve to explain and to make the student familiar with the causes of sounds, add to the usefulness of this little volume. J. DA C.

ART. XXV.—*The Hygiène of the Turkish Army.* By J. N. RADCLIFFE, M. R. C. S. Eng., late of the Staff of H. H. Omar Pasha. Reprinted, with additions, from the Sanitary Review. 8vo. pp. 60. London, 1858.

THE details presented by Dr. Radcliffe in respect to the particular hygienic condition of the Turkish army, and the influence exercised by it upon the health and efficiency of the men, are certainly most interesting, and furnish valuable hints to those upon whom the duty of arranging and carrying into effect the details of military hygiène devolves. It contains, however, few general principles that are not perfectly familiar to all conversant with the sanitary police of armies and encampments, and scarcely a single fact that had not already been verified by the experience of military medical officers.

What strikes us as most remarkable in reference to the hygiène of the Turkish army is, the extreme neglect of the soldiers to cleanliness of person and clothing; the filthiness of their habits, the polluted condition of the outskirts of their camp, and their entire indifference to preserve from impurity the sources from whence they derive the water for drinking and culinary purposes. The Turkish soldier may certainly perform with scrupulous regularity the daily ablutions prescribed by his religion; but these ablutions, as ordinarily practised, are not such as to insure cleanliness.

All this is truly surprising when related of men whose religion, as well by its precepts as in its ceremonial observances, inculcates so forcibly and constantly the duty of personal and domestic purity, and the defilement that ensues upon contact with every species of uncleanness. D. F. C.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Physiology of Digestion and Nutrition.*—BUSCH furnishes (*Virchow's Archiv*, vol. xiv., 1858) valuable information on the physiology of digestion and nutrition in general, derived from observations and experiments made on a female patient, aged thirty-one, in whom, through external violence, the intestinal tract was divided into two separate portions—the superior consisting of the stomach, duodenum, and a small part of the jejunum—the inferior of the larger part of the jejunum and the whole of the ileum and colon. The author had therefore the rare opportunity of observing the phenomena of a fistula of a jejunum in an otherwise healthy human subject. The accident had taken place six weeks before the admission of the patient into the University Hospital at Bonn; the wound in the abdominal walls was healed, with the exception of the fistulous opening. The woman had a voracious appetite, and devoured large quantities of food; the greatest part of this, mixed with the gastric, pancreatic, and intestinal secretions and bile, passed away from the fistulous opening in the jejunum; no particle of it entered the contracted and too distant aperture of the inferior portion of the intestinal tube. Extreme emaciation, loss of strength, tendency to sleep, and hoarseness, were amongst the principal constitutional phenomena.

The plan pursued by Busch, in order to restore the strength of the patient, consisted in the introduction of strong broths with eggs beaten up in them, occasionally also of pieces of boiled eggs and meat, into the inferior portion of the digestive tube. The strength of the patient increased rapidly under this treatment, and she gained also considerably in weight. This fact is of great interest, as it corroborates the view, that nutrition can take place—for some time, at all events—without the admixture of the gastric, pancreatic, duodenal, and hepatic secretions to the food. One might feel inclined to draw the further inference, that a complete fistula situated in the upper part of the small intestines renders the nutrition of the body through the stomach impossible; but this inference is made improbable by the circumstance that the ingestion of food through the mouth alone was sufficient to keep up and increase the strength of the patient after the recovery had reached a certain stage under the plan above described.

Although our space is limited, we cannot refrain from giving some of the principal observations made by the author, referring for the details to the essay itself. 1. There are two distinct sensations of hunger; the one is caused by the nervous system, in general perceiving the want of fresh supply for the exhausted tissue; the other is produced by the nerves of the digestive organs alone. The former of these may continue, even when the digestive organs are filled with food. 2. The peristaltic motion of the intestines is not continuous, but shows periods of rest and of increased action; there is, however, no regularity in the

alternation of rest and action. The force of the peristaltic motion could not be ascertained with precision, but it was sufficient to overcome a column of water two feet high. In the lower portion of the digestive tube, distinct antiperistaltic motion was frequently manifested. 3. The quantity of the intestinal juice (*succus entericus*) is very small and constantly alkaline. It will be remembered that this observation is in opposition to that of Frerichs, but in corroboration of Bidder and Schmidt's results. The percentage of solids varied between 3.87 and 7.4 per cent., which is considerably greater than that stated by the authors just mentioned. 4. There can be no doubt that the intestinal juice exercises a digestive influence on proteinaceous substances. This process, however, is accompanied by that of putrefaction. 5. The intestinal juice transforms starch into grape-sugar. 6. It does not transform cane-sugar into grape-sugar. 7. The cane-sugar, absorbed as such, does not reappear in the urine. 8. Without the influence of bile or pancreatic juice, there is either no absorption of fat or at all events it takes place only to a very limited amount. 9. The reaction of the mixture of digestive secretions passing from the upper portion of the tube during the state of fasting was almost always neutral, only in rare instances slightly acid or alkaline. 10. This mixture of fluids did never exhibit the reaction of saliva. We may therefore conclude that the saliva is absorbed before the mixture reaches the jejunum. 11. The average percentage contained in these fluids was 2.48. 12. The first portions of the food introduced into the stomach appeared in the jejunum from between fifteen to thirty minutes after the commencement of eating. 13. Solutions of cane-sugar disappear to the greater part already in the beginning of the digestive canal; what reaches the jejunum is transformed into grape-sugar. 14. Raw albumen of eggs is likewise partly absorbed already in the stomach and adjacent part of the digestive tube; the portion which enters the jejunum is unchanged. 15. Gum passes unchanged into the small intestines. 16. Gelatine becomes dissolved, and does not coagulate again; the greater part of it is absorbed. 17. Part of the casein contained in the milk reaches the jejunum in the uncoagulated state. 18. The mixture of the digestive fluids contained in the duodenum emulsifies fatty substances completely, if its reaction is alkaline, but less completely if it is acid. 19. This mixture of fluids exercises likewise a digestive influence on proteinaceous substances. 20. The lowest quantity of digestive fluids entering the jejunum within twenty-four hours amounts to more than one-seventeenth of the weight of the body.—*Brit. and For. Med. Chirurg. Rev.*, Jan. 1859.

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2. *On the Influence of Heat and Electricity on the Spinal Cord.* By DR. F. KUNDE.—1. Two normal frogs being placed in water, the one at the temperature of 1° C. and the other at 31° C., the respiratory movements and the pulsations of the heart abate in frequency in the one kept cool, while they increase in the other.

2. The heart's pulsations of a frog kept at 1° C. average 20–24 per minute; at 31° C. they average 60–64.

3. A frog may remain for twenty-four hours at a temperature of 1° C. without dying; the muscular movements become at such a temperature slower and less intensive.

4. A frog that has spent some time at a temperature of 34° C. loses both voluntary and reflex movements. Blood and lymph hearts continue, however, to pulsate. Should, however, this temperature be kept up for a considerable time, the animal dies without the occurrence of cadaveric rigidity.

5. If a frog be for some time exposed to the influence of an interrupted current of electricity, so that tetanus has primarily occurred, after a time, when examined after cessation of the current, it is found to have lost both voluntary and reflex motion, the blood-heart still beating forcibly. In a short time, such a frog recovers perfectly.

6. An elevated temperature and electricity remove the tetanus induced by strychnine.

7. A frog tetanized by a definite dose of strychnine loses its tetanus at a definite temperature, and is completely restored in a short time by a proper regulation of the temperature.

8. A frog which has received a certain quantity of strychnine becomes tetanic at 31° C., but not at 16° C. Even so a frog may become tetanic at 16° C., though it was not so at 1° C.

9. The administration of strychnine to frogs may be so regulated that they may remain unaffected at any definite temperature, and yet become immediately tetanic on a proper modification thereof.

10. A frog rendered tetanic at any temperature may be maintained in this state for fourteen days (and probably longer), by being kept at a temperature of 1° C. or laid on ice.

11. A frog which has lain several days on ice, tetanic from strychnine, loses this condition in a very short time by the application of warmth (as by holding in the hand).

12. A frog which has almost ceased to be tetanic at any definite temperature (as at 15° C.) has the tetanus immediately reproduced by being laid on ice or placed in a temperature of 1° C.

13. If a frog, tetanized by strychnine, be exposed to a current of interrupted electricity sufficient to produce tetanus in a normal frog, it speedily loses the strychnine tetanus both by the ascending and descending current.

14. A frog tetanized by strychnine may, for eight hours, be exposed to the most powerful action of two Daniel's batteries; nevertheless the tetanus recurs a short time after cessation of the current, while mechanical and chemical irritants are unable to produce reflex motions, though electricity can, immediately after removal of the wires, produce convulsive movements. (All these experiments must be made on newly caught frogs, and they must be reckoned not by the dozen but by the hundred.)

15. If two cats, from the same litter, get the same dose of strychnine, and both become tetanic, and the one then placed in a temperature of from 16°–19° C., and the other at 40°–45° C., that cat condemned to the low temperature speedily dies, the other is rapidly restored to its *status normalis*.—*Ed. Med., Journ.*, Jan. 1859, from *Verhand d. Phys. Med. Gesellschft. in Wurzburg.*

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3. *Sarcina Ventriculi.*—Dr. Hermann Itzigsohn directs attention to the fact that this, in common with all phycochromatous so called unicellular algæ, is neither a true species nor genus, but only a stage in the development of some polycellular filamentous alga, probably an oscillatoria; that the sarcinæ are, in fact, the free ripe gonidia or spores of some such alga. Their independent growth is no barrier to this view, as they have that in common with many other spores, as those of various lichens, which often form large pulverulent masses on stones, wood, etc., and also with those of many other algæ. He adds, also, that these gonidia are so minute as to be easily taken up by the absorbents, and carried by the bloodvessels to the bladder, bronchi, etc.; in a dry state, they may be even inhaled with the air, and subsequently grow.—*Edinburgh Med.-Journ.*, Jan. 1859, from *Allg. Wien. Ztwg.*

MATERIA MEDICA AND PHARMACY.

4. *Action of Carduus Mariæ, C. Benedictus, and Onopordum Acanthium.*—After a long and learned historical introduction, Dr. Lobach proceeds to state, that to Rademacher is due the credit of having rescued the Card. Mar. from oblivion, and given it a place in the materia medica as a remedy for obstinate hemorrhages. R. found it, as a decoction of the seeds, ℥j to ℥viij, dose ℥ss omni horâ, of great use in those hemorrhages depending on diseased liver or spleen (hæmatemesis, melæna, nasal and uterine hemorrhages); it also regulates the bowels, and is diuretic. L. has employed this remedy in fourteen cases of uterine hemorrhage; in three of chronic cough, accompanied by irregular and deficient menstruation and by occasional hæmoptysis; in two cases of abortion in the third and sixth month, in which the bloody discharge, after the lapse of

some weeks, had not ceased; and in one abortion in the second month, to check the gushing hemorrhage and promote a more regular flow. L. considers that there is no other remedy in the materia medica of equal value in the treatment of chronic uterine hemorrhage and melæna, inasmuch as it not only checks the immediate flow, but, by continued use, removes the cause on which it depends, regulating and restoring the natural portal circulation, and, where amenorrhœa depends on such irregularity, it tends to restore the normal flow. The other two thistles, C. Benedict. and Onopordum Acanth., have similar action. A tincture of the seeds is recommended as the best preparation.—*Ed. Med. Jl.*, Jan. 1859, from *Verhand. de Phys. de Gesell. in Wurzburg.*

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5. *Physiological Properties of Xyloids.* By J. BAKER EDWARDS, Ph. D. &c. —These "Substitution Compounds," containing nitrous acid, derived chiefly from the starch series, possess considerable interest from their physical properties. They are formed from these neutral, non-azotized vegetable compounds by the action of nitric acid, which removes more or less of hydrogen, and supplies $N\,O_4$ (nitrous acid) to the same extent, thus making up the original type. The first of these, formed from starch, C 24, H 20, O 20, takes up one equivalent of $N\,O_4$ in place of hydrogen and the xyloidine thus produced is the type of the series, and consists of C 24, H 19, ($N\,O_4$), O 20. By the same action upon lignine or cotton, we obtain two compounds, both "gun cottons," viz.—C 24, H 17, (3 $N\,O_4$) O 20, and C 24. H 15, (5 $N\,O_4$) O 20, the first being soluble in alcohol and ether, forming collodion; the latter, very slightly soluble in the same mixture, and forming the most violently explosive gun cotton. From sugar we obtain a resinoid substance, containing C 24, H 25, (3 $N\,O_4$), O 82 of a bitter taste, and highly explosive character. From glycerine, we obtain an oily fluid, insoluble in water, soluble in alcohol and ether, having a sp. gr. of 1.5. It contains C 6, H 6, (2 $N\,O_4$) O 6. Again, from Benzene (a pure hydro-carbon), C 12, H 6, we obtain by a similar process, nitrobenzene, containing C 12, H 5, ($N\,O_4$) a remarkable aromatic oil, closely resembling oil of bitter almonds, and used extensively in perfumery, as a substitute for this scent. With these several substances I have largely experimented upon animals, with a view of ascertaining the physiological effect of the group. The result is, that all appear to possess more or less the peculiar action upon the nervous system, characteristic of strychnine; and I believe, that further experiments upon man, would prove that they possess peculiar and valuable medicinal properties. Physically, they are allied, being all highly explosive bodies, deflagrating at a low temperature, or by concussion; and physiologically they all produce a powerful effect upon the heart's action, trismus, and a series of tetanic convulsion, terminating in death.

I have a few remarks to make upon each of these substances, which I thus distinguish—

1. Xyloidine	C 24, H 19 ($N\,O_4$) O 20.
2. Pyroxiline	*a*C 24, H 17 (3 $N\,O_4$) O 20. *b*C 24, H 15 (5 $N\,O_4$) O 20.
3. Saccharoine	C 24, H 25 (3 $N\,O_4$) O 28.
4. Glonoine or Nitroglycerine . . .	C 6, H 6 (2 $N\,O_4$) O 6.
5. Benzoine or Nitro-benzene . . .	C 12, H 5 ($N\,O_4$.)

With respect to the first two compounds, although in more than one instance slight convulsions followed their administration, yet the general symptoms were not very marked, probably owing to their insolubility in watery fluids they are not taken up to any extent by the stomach when exhibited in a dry state. The ethereal and alcoholic solutions are also unfavourable for the development of their properties, inasmuch as these fluids appear to act specifically as antidotes even to the more active of these substances, and therefore when administered with them, veil their action to a great extent.

A quantity considerably under a grain of saccharoine was given to a mouse, and the effect in this instance may serve as a general type of the results of other experiments upon the whole of this class of bodies.

In five minutes the mouse appeared wild and uneasy, ran quickly about under the large bell jar in which it was confined, and seemed anxious to escape; the

fur became raised, in ten minutes it began to scratch its jaw, as if uneasy about the mouth, the teeth chattered involuntarily, and once or twice the jaw was held firmly for a moment either when shut or open. The animal started violently at sudden noises, and in twenty minutes it began to fall forward when advancing, and soon after showed symptoms of partial paralysis, as its head was stiffly held in one position with a side twist, and when attempting to advance it made a circular motion and fell on the twisted side; in half an hour it began to have recurring spasms, which threw it on its side; the feet and forepaws being held firmly contracted. On recovering, it appeared easy, and breathed freely, but was stupefied and very reluctant to move. After a few moments, another spasm seized it as before, and held it for about one minute in a paroxysm. These were repeated until the animal died from exhaustion.

The most powerful of these is No. 4, nitro-glycerine, and this substance has two distinct physiological actions, which are modified by the dose and the duration of its action. At first the animal is excited and lively, the pupils of the eye are dilated, the heart's action accelerated and irregular, the breathing rapid and labored, the animal staggers in walking, and appears intoxicated. This effect goes off in about half an hour, when it eats and appears to recover. If the dose has been small, no further symptoms are observed; but when (in the case of a full-grown rabbit), the dose has been about twenty drops, or when four drops were injected into the jugular vein, the first symptoms having passed off; in the course of two hours, a chain of nervous symptoms sets in, resembling in a remarkable degree that produced by strychnine, every distinct stage being greatly prolonged, and after chattering and fixing of the jaw, and tetanic convulsions affecting violently the whole frame, the animal at last becomes exhausted and stupefied; the pupils contract, the breathing becomes laboured, and gradually the heart ceases to beat. The convulsions last from one to four hours, and sometimes the animal dies during a paroxysm.

The powerful action of minute doses of glonoine upon the human system I have experienced and repeatedly witnessed, and I am disposed to conclude that the physiological action of minute doses is different in kind to that of larger doses, and that in the latter case we by no means obtain the primary effect in an exalted degree, but a new kind of action altogether. The last-named substance nitro-benzene, produced death in a mouse, in a dose of 4 drops, the symptoms being less marked than in the other cases, and death resulted from a single convulsion; this substance, however, did not produce death in a rabbit, in a dose of 30 drops; it is therefore less energetic than glonoine.

It will be observed, that these symptoms, produced by glonoine especially, closely resemble the effects of strychnine, but the action is more steady, and accompanied with less distressing fear and trembling, and in addition exhibiting a narcotic action. We must therefore infer, that strychnine is not the only substance that is capable of producing a peculiar chain of tetanic symptoms, terminating in death. I also beg to suggest, that the saccharoine, and glonoine more especially, appear to be possessed of properties which promise to be of even more value than strychnine, while at the same time they are less violent in their action, and I would strongly recommend a trial of their effects, in cases of epilepsy and paralysis, &c.—*Liverpool Medico-Chirurgical Journal*, Jan. 1859.

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6. *Method of making the Mercurial Cigarettes.*—Dr. Nevins gives the following directions for preparing these cigarettes: A little practice, he says, is requisite for skill in making them; but the following directions will obviate the difficulties generally experienced on the first attempt—

Take of nitrate of mercury	15 grains,
Acid nitric: fort.	15 "
Aquæ destillatæ	6 drms: vel. q. s.

Mix the acid and water, and dissolve the nitrate of mercury by the aid of a gentle heat, which may be obtained on the top of an ordinary oven. Then soak in the solution a piece of thick white blotting paper, six inches by eight, and dry it. Before it is quite dry, cut it into eight slips, and roll each of these round a thin pencil, so as to make a small paper tube. To prevent this from unrolling, the

free edge should be gummed. The cigarette is now complete, and when dry, will burn like touch-paper when smoked. The interior may be stuffed with tobacco if desired, or the paper itself may be rolled into a cigar, along with tobacco in the first instance; but it ought always to be rolled before being quite dry, as it is liable to become brittle when perfectly dry, and to crack in the operation of folding. It sometimes happens, that the nitrate of mercury does not become perfectly dissolved, even by the aid of the gentle heat, in which case the mixture should be stirred up previous to the paper being immersed in it, and a slight degree of agitation of the liquid will diffuse the undissolved nitrate uniformly over the paper.

It is evident that other agents than nitrate of mercury might be employed in this way. Nitrate of potash, in the form of common touch-paper, would yield nitrous acid, which might be of service in a languid ulceration about the throat. Creasote and various pyrogenic compounds may be applied directly to the bronchial membrane, by smoking brown paper of a tarry composition; and benzoic acid might be introduced in a similar manner, by soaking the white paper in a very weak solution of saltpetre, to render it sufficiently combustible, and then adding a solution of benzoin or of benzoic acid, before rolling the slips into cigars; but the principle being once understood, the particular drugs to be adopted, and the cases in which they are to be applied, will suggest themselves to the mind of the medical man as the necessary occasions arise.—*Liverpool Med. Chirurg. Journ.*, Jan. 1859.

7. *Pagliari's Hæmostatic.*—The following formula has been given for the preparation of this article: Eight ounces of tincture of benzoin, one pound of alum, and ten pounds of water, are boiled together for six hours in a glazed earthen vessel, the vaporized water being constantly replaced by hot water, so as not to interrupt the ebullition, and the resinous mass kept stirred round. The fluid is then filtered, and kept in stoppered bottles. It is limpid, slightly styptic in taste, aromatic in odour, and the colour of champagne. M. Hepp, of Strasburg, has substituted white resin for the benzoin. Every drop of this fluid poured into a glass containing human blood produces an instantaneous magma; and by increasing the proportion of the styptic to the quantity of the blood, a dense, homogeneous, blackish mass results.

8. *Voltaic Narcotism.*—Dr. B. W. RICHARDSON, Professor of Physiology at the Grosvenor School of Medicine, has, by using electricity combined with a narcotic, succeeded in inducing local anæsthesia. He applies a narcotic solution, consisting of equal parts of chloroform and tincture of aconite, to the part in which he desires to produce anæsthesia; then covers this part with a plate connected with the positive pole of the voltaic battery, and applies the negative pole to an adjoining part. Dr. R. in this mode produced anæsthesia in a nævus on the back of an infant, when Dr. Halford transfixed and tied a ligature around the nævus without the infant giving any indication of suffering pain by the operation.

9. *Croton Oil as an Epispastic.*—Dr. VON BASTELAER recommends the following pomade as a very useful substitute for cantharides in cases where the influence of the latter on the urinary organs is feared: Take of fresh lard 22 parts, white wax 2 parts, croton oil 6 parts, by weight. Melt the wax and lard by a gentle heat, and rub up in a heated mortar until the mass becomes cool, and then mix in the oil intimately.

10. *Hæmostatic Properties of the Perchloride of Iron.*—The solution of this persalt is now almost universally employed to arrest arterial or venous hemorrhage, resulting either from accident or as a consequence of surgical operations. It has also been found useful in intestinal hemorrhage; in one case in particular, M. Demarquay, of Paris, administered, morning and evening, enemata of seven ounces of fluid, with twenty drops of the concentrated solution of perchloride of iron, and a tablespoonful of the perchloride syrup (five or six drops to the tablespoonful), where the hemorrhage from the bowels was considerable, and had re-

sisted the ordinary remedies. The result was extremely satisfactory. The same surgeon relates a second case of extensive abscess of the shoulder, where an injection of iodine caused severe hemorrhage. This was arrested by throwing into the sac a lotion composed of seven ounces of water and ten drops of the perchloride.

In gonorrhœa and leucorrhœa, injections of the perchloride have been tried with success in weak and lymphatic subjects, the proportion of the perchloride being twenty drops to three ounces and a half of water.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

11. *Hydrochlorate of Ammonia in Neuralgia.*—Mr. H. C. Brenchley, Surgeon to the Brighton Dispensary, relates (*Lancet*, Oct. 16, 1858) the following case to illustrate the efficacy of hydrochlorate of ammonia in neuralgia.

A young man, aged 23, unmarried, healthy, and without any other apparent complaint, had long suffered from very severe attacks of neuralgia of the face, coming on at intervals of about one month, and lasting from two or three days to a week. It sometimes came on on one side of the face, and at other times on the other side. During one of these attacks I saw him, and ordered him quinine and arsenic, which put an end to the attack for the time. After the lapse of a month or six weeks, he had a second attack, which was cured in the same way. A third attack, however, came on after a shorter interval. This time the quinine and arsenic failed to relieve him. On the third day, when I saw him, he was in great agony, propped up in bed, and unable to do anything from the severity of the pain. The left side of his face was swollen, flushed, and hot, the temperature considerably higher than on the right side; the heat, also, of the inside of the mouth was so great that I expected I should find matter forming from decayed teeth; but, on examination, I failed to do so.

I now tried the much-vaunted remedy—the valerianate of ammonia, but without any effect. The usual remedies having failed, I gave him the hydrochlorate of ammonia, in doses of half a drachm every hour, in camphor mixture. I saw him three hours after he had commenced this treatment, and found he had been much relieved after taking the second dose; and, having taken the third dose, he was almost free from pain, and begged to be allowed to continue the remedy. The heat and flushing of the face had subsided, and the temperature of the mouth considerably reduced, feeling quite cool after the burning heat of its former state. He went on for three or four days with the remedy, in doses of fifteen grains, three times a day, although there was no return of pain. Three months have now elapsed, and he has had no relapse.

The *modus operandi* of this medicine is not very clear; but whatever other specific virtues it possesses for the cure of neuralgia, in this particular case it evidently acted as an indirect sedative by lessening the arterial action; for the first and most striking effect of the medicine was the rapid lowering of the temperature of the mouth and face. From further observation, I have found that this remedy is most useful in those cases of neuralgia which are attended with heat and swelling.

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12. *Administration of Medicines in a State of Vapour.*—The treatment of the diseases of certain parts of the body sometimes presents difficulties from the difficulty of applying topical remedies to them. Such are the rima glottidis the mucous membrane lining the bronchial tubes; the Eustachian tube; the membrane lining the frontal sinuses, and the antrum maxillare. Dr. Nevins in a paper recently read before the Liverpool Medical Society, states that he has beneficially applied medicinal agents to these parts, in the state of vapour; and he relates the following cures illustrative of this principle:—

Rima Glottidis.—A healthy young woman came under my care with *complete*

loss of voice, which had continued for nearly twelve months. She attributed it to frequent colds, taken in answering the front door, which opened towards the east. She had, at first, been treated by a medical man of celebrity and skill, and all acute symptoms had long disappeared, and the loss of voice was all that remained—but she could barely speak above a whisper. Iodine and other remedies had been employed without avail, and the patient for many months had used no means at all for the restoration of her voice.

It appeared to me, that the aphonia was probably due to chronic thickening of the chordæ vocales, and that if local stimulants and some mild mercurial could be applied, benefit might be anticipated. I therefore availed myself of a method for attaining this object, with which I had become casually acquainted whilst translating Trousseau's work on the "Art of Prescribing."[1] He there describes some "mercurial cigarettes," which appeared exactly calculated to accomplish the object in view. I therefore directed the patient to smoke one of these cigarettes twice a day, and instructed her, instead of blowing the smoke at once from her mouth, to breathe gently inwards, so that it might pass through her windpipe once in breathing it in, and a second time in breathing it out again. At first, the smoke occasioned a slight disposition to cough, but the larynx soon became accustomed to the stimulant, and in a day or two she smoked them without difficulty. In a short time improvement was evident, and in about a month her voice was restored nearly to its natural power, and it continued healthy to the time when I lost sight of her, which was several months after her recovery.

I have had one or two similar cases in which the like benefit has been derived; and I conceive, that chronic ulceration of the larynx or vocal organs would be advantageously treated in a similar manner, though I am not able to speak with equal confidence on this point, not having had the opportunity of submitting the plan to the test of experience.

FRONTAL SINUSES.—A middle-aged gentleman had suffered for many months from pain in the frontal sinuses, and a very offensive discharge in the nose, which destroyed his comfort, by the odour constantly rendering his food unpalatable, and exciting notice, as he fancied, when he was in company. He had used lotions and injections into the nose without avail, previous to consulting me. I directed him to use the same cigarettes, but instead of inhaling the smoke, to force it into his nose when closed by the fingers, and in the course of three or four weeks the discharge and pain had ceased, and he discontinued their employment.

NASAL POLYPUS.—A gentleman who had suffered from this disease for some time, and had had one polypus removed in London, and another by myself, was recommended to make trial of the cigarettes when he felt the early symptoms of the returning disease. He used them occasionally in their simple condition, blowing the smoke through his nostrils instead of direct from his mouth, and the symptoms disappeared, and the nose returned to its open state. He is an habitual smoker, and has latterly had tobacco made into cigarettes, by being rolled up in paper prepared with the mercurial solution. He has not required any further operation for some time, as he resorts to this remedy when his nose becomes uneasy, and it has hitherto been successful in restoring him to comfort.

EUSTACHIAN TUBE.—A patient suffered from deafness, which was not due to obstruction of the outer ear by wax, nor was there any evidence of inflammatory action to account for it. He had not recently laboured under cold, and the condition of the throat was not sufficiently disordered to explain the loss of hearing. He stated, however, that on one occasion he was sneezing, and for a moment, he regained his hearing distinctly, but lost it again almost immediately. This circumstance appeared to give the clue to the nature of his disease, which probably arose from an obstructed Eustachian tube. The difficulty and uncertainty of passing the Eustachian bougie are so great, that I preferred having recourse to the remedial effects of the stimulating vapour, and accordingly directed the patient to smoke the cigarettes; but in this instance the instruction was given to *swallow* the smoke, and endeavor at the same time to make the ears

[1] This translation with notes has been published by Bailliere, Regent Street, London, under the title of "The Prescriber's Complete Hand Book. 24mo. 1852."

crack. In his early attempts he did not always succeed, but he soon acquired sufficient skill to force the smoke into both ears until he felt them distended, and his deafness was cured after using the cigarettes about a fortnight. A case is now under treatment in the same way, in which the patient has been deaf for above seven years, and the improvement is slow. He has been using the cigarettes for about six weeks, and even now it is too early to pronounce the case a successful one; but his hearing is improving, whilst all previous treatment had been entirely without benefit.

General Lining Membrane of the Bronchial Tubes.—The method of applying astringents and opiates to the surface of the bronchial membrane, by inhaling very finely powdered alum and opium, was lately brought before the Society by Mr. Bickerton, who exhibited his ingenious contrivance for effecting this object, and mentioned cases illustrative of its benefit. The method was the same in principle as that illustrated in the foregoing remarks; and although the title of this paper, "On the Internal Employment of Medicines in a State of *Vapour*," will not apply perfectly to their employment in the state merely of very fine powder, yet the beneficial results in his cases were so directly traceable to the topical application of the remedies, by means of the respiration, that I shall be pardoned for adducing his results in support of the method now under our notice.

In Mr. Bickerton's cases, his object was to apply a local astringent for the purpose of checking profuse secretion from the bronchial membrane, and the result corresponded with his expectations, and the cough and expectoration were essentially diminished. With a design in some respects similar, I have employed the mercurial cigarettes for the purpose of acting upon the whole bronchial membrane, in a case of long-standing spasmodic asthma. In this case, the occurrence of damp weather was almost always productive of an attack, and it seemed probable, that if the irritability of the bronchial surface could be diminished, the attacks might be reduced in frequency; the patient was, therefore, desired to smoke the cigarettes, which she learned to make for herself, and they proved so far successful, that when she felt an attack of difficult breathing coming on, she had recourse to them, and on several occasions the threatening symptoms went off, and no attack of spasmodic asthma was experienced. I do not, however, venture to lay too much stress upon this case, as she has had some severe attacks, notwithstanding the smoking, and because other means of a general tonic character, such as bathing and exercise were employed at the same time. Notwithstanding all reasonable deductions, on these grounds I am still persuaded, that the remedy has been of service in lessening the irritability of the bronchial membrane.—*Liverpool Med.-Chirurg. Journ.*, Jan. 1859.

[The method of making the mercurial cigarettes will be found described in the department of Materia Medica and Pharmacy, in the present No., p. 539.]

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13. *Inhalation of Carbonic Acid in Granular Pharyngitis.* By Dr. Willemin.—The therapeutical application of the inhalation of carbonic acid has been made only in late years and principally in Germany. Bischoff and Ennemoser have made some experiments with carbonic acid, and have come to the conclusion, that when the gas is not breathed pure, nor for too long a time, it produces no unpleasant effects; but, on the contrary, that the respiration of the patients becomes more easy, and the expectoration more abundant. Germany possesses, in the present day, a great number of establishments where the inhalation of carbonic acid, more or less mixed with air or steam, is methodically practised. The gas is collected by means of an apparatus to which a caoutchouc tube is adapted, permitting the gas to be directed into the interior of the mouth, on the neck, or on any other part. At Pyrmont, a bell-glass has been placed over the spring from which the gas is disengaged, and three tubes carry away the carbonic acid. At Franzensbad a large basin has been constructed, and the patients descend into it by several steps, at the bottom of which the gas is disengaged. The superior stratum of air contains fifteen per cent. of carbonic acid; and in proportion as the patients are accustomed to this atmosphere, they approach nearer to the orifice from which the gas escapes.

The principal effect of the use of this gas, according to the German physi-

cians, is to increase the activity of the circulation, and to depress the functions of the nervous system. The indications for this mode of treatment are thus drawn up by Lersch; it is especially serviceable in cases of dyspnœa dependent on the accumulation of mucous secretion in the pulmonary vesicles, or on emphysema of the lung. Helfft considers that this gas exercises a stimulant action upon the respiratory tract, and he recommends its use in chronic inflammation of the larynx and of the pharynx, in bronchial catarrh, and humid asthma. M. Goin recommends its use in nervous affections of the respiratory passages, as in asthma, and also in intermittent fever. The contra-indications of this inhalation are said to exist whenever any affection of the respiratory passages is accompanied by excitement of the circulating system. The German physicians unanimously forbid this treatment in phthisical cases. According to Gräfe, it is absolutely injurious when cavities are formed, these being often surrounded by a zone of inflammation. Under the influence of the carbonic acid inhalations the expectoration is diminished, and the fetor of the secretions is corrected, but the inflammation of the pulmonary parenchyma is augmented. Its use must also be avoided in hæmoptysis.

Dr. Willemin draws the following conclusions upon the inhalation of carbonic acid—namely, that when mixed with air, this gas produces a more or less active excitement of the respiratory passages, the action being analogous to that of the same gas upon the skin, the eye, &c., and this effect ought to contra-indicate its use whenever there exists a disposition to acute inflammation.

That the excitement is followed by a sedative effect, which appears to depend on a special action of this gas on the nervous system, the respiration becomes easier, the cough is allayed, the circulation is retarded, and the redness of chronic inflammation diminishes.

That continued for a longer time, or made with a larger proportion of gas, these inhalations cause vertigo, relaxation of the limbs, and anæsthesia.

That, in addition to this general action, the gas possesses an anæsthetic power over wounds and parts denuded of epidermis.

That these inhalations are especially efficacious in chronic inflammations with atony of the mucous membrane and exaggerated secretion, and in nervous affections of the respiratory passages; but that they ought to be avoided in phthisical cases.—*Brit. and For. Med.-Chirurg. Rev.*, Jan. 1859, from *L'Union Médicale*, July 15, 1858.

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14. *Instillations of a Tepid and rather Concentrated Solution of Chlorate of Soda into the Trachea of Children affected with Croup.*—M. BARTHEZ has been trying local applications to the false membranes produced in croup. For about two months, an epidemic of croup had prevailed near the Hôpital Ste. Eugénie, and had assumed some peculiar characters, being attended with the production of a false membrane in the bronchi, trachea, and larynx, seldom in the pharynx, and hardly ever in the nasal fossæ. The general symptoms of blood-poisoning were generally absent, and after tracheotomy, the children died with the symptoms of slow asphyxia; they all died, and in the greater part of them, the trachea and the bronchi were filled with false membranes.

M. Barthez had previously made some experiments on the comparative effects of chlorate of soda and chlorate of potash, when these salts are applied to the false membranes. Two portions of false membrane, of nearly equal dimensions, were placed in contact respectively with a concentrated solution of chlorate of potash, and a concentrated solution of chlorate of soda. The false membranes were gradually altered in character; they softened and lost their opacity; their tissue became less compact, more transparent, and afterwards diffluent, and their membranous form disappeared without losing all its cohesiveness. The only difference observable in the two solutions was that the changes began later in the chlorate of potash than in the chlorate of soda, and required a much longer time for their completion. The same membrane immersed in water only, preserved its natural appearance for many days.

Guided by these experiments, M. Barthez instilled through the canula a tepid solution of chlorate of soda in some cases of tracheotomy, in the hope that he might thus effect the softening of the false membranes, and consequently the

more easy destruction of their adhesions, and their more rapid and complete expulsion. The results of these experiments were very encouraging, for all the patients previously treated had died, but when the new system had been introduced, three out of seven patients recovered. Judging from this result, M. Barthez is convinced that the instillation is innocuous, and that it is even useful. In order to ascertain the different effects produced by solution of chlorate of soda and by pure water, a patient was treated by instillations of water only, but although the instillation was very frequently repeated and cough was excited, yet no other liquid but water was thrown out. At last, solution of chlorate of potash was substituted for the water, and it was instilled every quarter of an hour; and at the end of about an hour a remnant of false membrane was evacuated, and then other portions in succession until the next day. The suffocation diminished in proportion, and the patient recovered completely.

M. Barthez relates five cases in which the chlorate of soda was employed in instillations in the trachea; and although he admits that the cases are not sufficient to establish incontestably the efficacy of this treatment, yet he thinks that the beneficial action of the soda-salt is very probable. Tracheotomy ought not to be performed unless there is reason to suppose that a false membrane exists in the trachea; even when the false membrane exists, it is useless to employ instillations when there are symptoms of diphtheric poisoning, or when the expectoration of false membranes is abundant; but when tracheotomy has been performed, and it is determined to use instillations, then the chlorate of soda appears to assist the child in discharging the false membranes which oppose the entrance of air into the lungs.—*Brit. and For. Med. Chirurg. Rev.*, Jan. 1859, from *Bull. Gén. de Thérap.*, May 30, 1858.

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15. *Chlorine in the Treatment of Diphtheria.*—The *British Medical Journal* of March 8, 1859, contains two communications on this subject, one by Mr. C. F. Hodson, and the other by Mr. S. A. Alford.

Mr. Hodson recommends the inhalation of the vapour of boiling water mixed with a portion of solution of chlorinated lime, and he relates the case of a child successfully treated by this method.

Mr. Alford recommends a gargle of chlorinated soda, of the strength of from half a drachm to a drachm in an ounce of water, to be used every ten or fifteen minutes, so as to wash away, and *keep washed* away, the morbid formation, "and, by the constant application of the chlorine contained in the gargle, destroy the poisonous character of the disease." When the plan is persevered in, he says, "the white fungi and jelly-like mass was washed away; and the throat is kept free by constant gargling."

He believes "the chlorine itself not only counteracts the poisonous nature of the morbid secretion, and destroys its deleterious character, but also has a beneficial effect on the system at large; and again, by destroying the virus, prevents its spreading to other members of the family. The disease has never spread where this plan has been adopted. All these points have satisfied me in the use of this remedy, and I am pleased to find others have found it equally successful, as noticed in the Journal by Dr. Bryden. The character of the gargle is important, but I feel the frequently washing of the throat to be equally essential; and for this reason, I prefer the gargle being used every ten or fifteen minutes to the dilute hydrochloric acid only applied occasionally, as used in some of the metropolitan hospitals. In addition to the frequent washings of the throat, which I consider *the* treatment, I apply *spongio-piline*, frequently steeped in hot water, tied round the neck; and at once keep up the strength by stimulants; viz., wine and brandy, with eggs, arrowroot, or water. I give bark and chlorate of potass with henbane, and an anodyne at night (twenty minims of liquor opii sedativus); for sleeplessness has been a marked feature in all my cases; aperients of a warm but decided nature, and little or no mercury, make up the treatment I adopt. When the above plans have been carried out, I have found them invariable successful.

"In several severe cases of ulcerated sore throat, the same gargle, constantly applied, has succeeded."

16. *Treatment of Chronic Alcoholic Intoxication.*—Dr. MARCET read a paper on this subject before the Western Medical and Surgical Society (Jan. 21, 1859). The author began by stating that nervous symptoms resulted from the long-continued use of alcoholic beverages, and that oxide of zinc was a remedy for such symptoms. The physiological and therapeutical properties of this drug were then passed in review, the author stating that he had himself observed the oxide to induce drowsiness, and even sleep, which might explain its sedative and anti-spasmodic properties. In cases of chorea, mild hysteria, paralysis, and lead palsy, its use gave but unsatisfactory results; and in the majority of cases of epilepsy it cannot be considered an effectual remedy. The author then analyzed twenty-seven cases of such a chronic state of intoxication, giving a synoptic table, on which were noticed the following points: 1. Name, etc. 2. Quality and quantity of drink taken. 3. Period of existence of intemperance. 4. Sleep. 5. Hallucinations. 6. Trembling. 7. Other nervous symptoms. 8. Pre-existing disease. 9. Result of the treatment. Ten of the above cases were taken in detail, the results showing the efficacy of the oxide of zinc as a remedial agent. With respect to the treatment, it was not merely necessary for the patient to cease drinking, as the symptoms referable to the nervous system often occurred long after the habits of intemperance had been abandoned, but that an active treatment was also absolutely necessary. The oxide of zinc was given in doses of two grains, twice a day, in the form of powders, an hour after each corresponding meal. The dose was generally increased in the ratio of two grains every three days, until the patient took six or eight grains twice a day. Thus sleep was soon induced, the trembling of the body and limbs rapidly disappeared, and the patient no longer suffered from headache or giddiness, the hallucinations vanished, and in the course of from three to six weeks the patient had recovered from a long and severe illness. The weakness, the common symptom accompanying the disease, was very difficult to overcome, and he states that it often persisted a long while after the individual was quite well in every other respect. Another fact noticed was the complication of chronic alcoholic poisoning with bronchitis and rheumatism, in which cases the effects of the oxide were less marked. In these instances the functional disturbance of the nervous system often gave way without any improvement in the co-existing disease. Accordingly, in these cases, the author added to the treatment as soon as the effects of the oxide were exhausted. The result of the twenty-seven cases were as follows: 6 continued attending; 11 had been discharged, cured; 4 left the hospital (Westminster), quite recovered; 4 much improved; and 2 ceased attending on the 1st and 2d inst.—*Med. Times and Gaz.*, Feb. 12, 1859.

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17. *Great Pigment Deposit in the Skin without Disease of the Supra-renal Capsules.*—In our previous No., p. 251, we gave a brief notice of this case, but as it is one of much interest we shall now give further particulars in regard to it, from an account recently published by Dr. E. A. PARKES. (*Med. Times and Gaz.*, Dec. 11, 1858.)

William Barker, aged 66, a cabman, of extremely intemperate habits, was admitted into University College Hospital on October 7, 1858, with ascites, dependent on contracted liver. He was a fine strong-built man, and in spite of exposure to weather and of his habits, had had remarkably good health. The only illness he could recall to mind was an attack of jaundice seven years before, for which he was treated in University College Hospital for five weeks. He left the hospital apparently well. Some time afterwards (four or five months) he noticed that some parts of his skin (which before had been of healthy colour) were gradually becoming darker, especially the skin of the face and neck; he then noticed dark patches on the body, and on the arms and thighs, and these increased until a very considerable part of the whole body had assumed a very dark hue. Those portions of the skin which did not become dark became, he thought, even whiter than before. This discoloration advanced very gradually and continually for several months; he was not quite clear how long; sometimes saying that it was about six months, and at other times that it was a year or even eighteen months. But it seems clear that after a certain time the darkening process stopped, and since that time, now certainly five years, if not more,

the skin remained unaltered, and presented the same characters as when he was admitted into hospital in 1858. During this change of color he appears to have had good health; he continued to follow his occupation, and to drink as before from half to three-quarters of a pint of gin daily. There seem to have been no general weakness, impairment of nutrition, or anæmia, until about four months before death.

In the summer of 1858 he began to feel ill and weak, and to lose his appetite, and in August he observed that the abdomen was swollen.

When admitted in October he presented a very singular appearance, from the extreme darkening of great part of the skin. The epithets "bronzed skin," or "mulatto skin," might perhaps be applied to it, and certainly would not exaggerate its intensity. The dark tint was uniform, or with slight variations of tint over the face, neck, shoulders, and arms; but over the trunk, and especially over the abdomen, it was diversified with irregular white patches, varying in size from one to four inches in diameter. It was uncertain whether these patches were whiter than natural. The scrotum gave the best example both of the dark colour and of the white patches. Over the upper part of the thighs the skin was also dark, with some small white patches; towards the knees the dark colour lessened, and below the knees the skin looked of a natural tint. The skin had its ordinary elasticity and sensibility. There was a little pigment on the conjunctiva, and a dark patch on the mucous membrane of the lips.

The conjunctivæ were also slightly yellow, and the urine contained a small quantity of bile pigment; but the discoloration of the skin was not like that of Melas Icterus, to say nothing of the white patches being altogether opposed to the hypothesis, that the dark tint could be attributed to bile pigment.

In other respects the patient presented the usual symptoms of contracted liver with ascites, and with very scanty and deeply pigmented (red and pink) non-albuminous urine. The lungs were healthy; the arteries at the wrist rigid; the heart was pushed up by the ascites, and there were extremely faint obstructive and regurgitant murmurs over the aortic valves. The nervous system was unaffected.

Paracentesis was employed, and eighteen pints of fluid drawn off; but the fluid collected again very rapidly; and in spite of various remedies the patient sank and died on the 10th of November. During life the blood was examined microscopically by Dr. Harley, who found no excess of white corpuscles, and no free pigment; the red corpuscles were "large, flabby, and dingy-looking;" blood crystals could not be obtained.

This patient had been regarded with much interest during life, as it was supposed to be a marked case of Morbus Addisonii. After death the microscopic characters of the skin were found by Dr. Harley to correspond with those which have been previously noticed in cases of the so-called bronzed skin; there was, namely, great pigment deposit in the rete-mucosum. There was also pigment deposit beneath the epithelium of the peritoneum, forming several black patches. The supra-renal capsules were perfectly healthy, both in size, shape, makroscopic and microscopic characters. I requested Dr. Harley to examine them, and annex his report.

"*Right supra-renal Capsule.*—Normal in color; of healthy consistence; of usual size and shape; measures $2\frac{1}{4}$ inches in its longest diameter; $1\frac{1}{4}$ in height; $\frac{1}{4}$ at the thickest part. On section the medullary substance is beautifully well marked, of the healthy slate colour, and firm consistence; no large cavity in it; no grumous matter; rows of small sinuses, distinct, full of healthy-looking blood; cortical portion well defined, running all round the medullary in a well-marked yellow ring; looks perfectly healthy."

"*Left Capsule.*—Normal in colour, size, shape, and consistence; measures $2\frac{3}{8}$ of an inch in longest diameter; $1\frac{1}{4}$ high. On section the medullary as well as the cortical substance appears perfectly healthy. Examined with the microscope, the columnar cell-masses of the cortical substance are beautifully seen; the medullary cellular matter was equally distinct; not the remotest trace of disease could be detected in either capsule."

I should mention that the capsules and a portion of the skin were modelled in wax by Mr. Tuson; and these models, as well as the capsules themselves, and

a piece of skin preserved in spirit, are deposited in the museum of University College, and can be seen by any one. Dr. Harley also has made drawings of the microscopic appearances of the skin and capsules.

I need not detail the condition of the other organs at length; the liver weighed thirty-four ounces, and presented a fine example of the contracted hobnail, or granular liver; the spleen weighed 14½ ounces, its capsule was uniformly thickened, to the amount of about a quarter of an inch; on section it was firm, not evidently hæmorrhagic, and without apparent enlargement of the Malpighian bodies; it was not examined microscopically. The kidneys 4½ ounces each; they seemed healthy to the eye: on microscopic examination they were found to contain perhaps a slight excess of fibrous tissue, but the tubes and epithelium were quite healthy.

The facts which may be taken as certain in this case, are the existence of extensive pigment deposit in the rete-mucosum of the skin, without the slightest trace of disease of the supra-renal capsules. Whether the disease is to be received as an example of the Morbus Addisonii, and if so, whether it is sufficient to destroy the doctrine of the supposed necessary coincidence between pigment darkening of the skin and disease of the supra-renal capsules, are points in which the readers of this journal will judge for themselves. For my own part, I can see no distinction between the skin affection in this case and in those cases recorded as examples of the Morbus Addisonii, in which the skin has been microscopically examined. The anatomical condition of the skin was the same; the depth of colour, though great, was merely dependent on a high degree of the anatomical condition (viz. pigment deposit) and the fact that some patches of the skin were devoid of colour, is pointedly described by Dr. Addison as occurring in some of his patients. I therefore can come to no other conclusion than that this case shatters the doctrine of the necessary connection between this peculiar state of the skin and disease of the supra-renal capsules. It is true, however, that there was no anæmia, nor any of those grave but obscure constitutional symptoms of weakness and general failure, which are described so carefully and emphatically by that eminent physician; and, therefore, this case proves or disproves nothing as to the connection between disease of the suprarenal capsules and grave anæmia with or without pigment changes in the skin.

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18. *Complete Disorganization of both Supra-renal Capsules without Discoloration of the Skin.*—While the case related above by Dr. Parkes distinctly proves that there may be very great bronzing of the skin without any disease of the supra-renal capsules, the case now to be noticed, an account of which we find in the *Med. Times and Gaz.* (Jan. 8, 1859), given by Mr. NORRIS F. DAVEY, demonstrates that there may be complete destruction of the capsules while the stain remains of marble whiteness.

R. A., aged 18½ years, a servant-girl, reputed to be healthy, was confined Dec. 22, 1857, of a child at the full period, and died rather unexpectedly two days afterwards.

Sectio Cadaveris, Dec. 30.—Body fat, of uniform marble whiteness; no putrefaction. Legs œdematous. Three-quarters of an inch of subcutaneous fat. *Thorax.*—Four ounces of serum in pericardium. Heart enlarged, pale; fatty degeneration of muscular substance. Both ventricles dilated and hypertrophied, the left very much so; both full of fibrinous clots. Valves healthy. Auricles: right dilated; left natural. Pleural cavities containing each about a pint of serum. Lungs pale, collapsed, gray; posterior lobes infiltrated with serum; otherwise healthy. Abdomen containing three or four pints of serum. Omentum, etc., very fat. Stomach quite healthy. Liver enlarged, pale; its convexity indented by the enlarged heart, fatty, containing much serum. Small intestines and colon healthy. Pancreas and spleen natural. Kidneys: left much enlarged, fatty; right less so. *Supra-renal Capsules.*—Left, large, dark gray in colour externally; no trace of natural structure on section; its contents consisted wholly of dark reddish-brown, soft matter, mixed with yellowish, cheesy masses. Right, very small, pink, semi-transparent, and gelatinous in appearance, both without and within. They were preserved for microscopic examination. Uterus

pale, firm, natural in appearance on section; point of attachment of placenta seen at fundus. Ovaries and bladder healthy. Head not examined.

Upon examining the supra-renal capsules under the microscope with a good $\frac{1}{4}$, and also with a Ross's $\frac{1}{8}$, I could find no trace of the normal cell structure, the left consisting of dark amorphous matter mixed with abundant oil-globules; the right almost entirely of fatty matter.

Dr. Parkes's case proves distinctly that there may be very great bronzing of the skin without any disease of the capsules; the present case proves that there may be complete destruction of the capsules, and yet the skin remain of marble whiteness; it must, therefore, be conceded that the remarkable coincidence of darkened skin and diseased capsules, discovered by Dr. Addison, though very frequent, is not necessary.

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19. *Intense Neuralgic Pain in the Head; Sudden Death; Cysts in the Cerebellum.*—Dr. James Turnbull, Phys. to the Liverpool Infirmary, relates (*Liverpool Medico-Chirurgical Journ.*, Jan., 1859) two instructive examples of this.

Case I.—Sam. Carpenter, a sailor, aged 32, was admitted into the Royal Infirmary on the 12th of April, 1857, having been ill with pain, in the head and neck, of most intense character for six weeks. He had the greatest difficulty in turning or moving the head, and the pain was chiefly in the right side. The sight of the right eye was somewhat impaired, but there was no difference in the size of the pupils. He had weakness, but no loss of power or feeling in the limbs. The tongue was very much furred when he came in, but it became nearly clean at one time, being variable in its condition. He had occasional vomiting, and the bowels were obstinately confined. Various remedies were tried—aperients, quinine, and opium, without any benefit. It was thought there might be some deep seated disease of the vertebræ or bones at the base of the skull, and issues were made with potassa fusa, and iodide of potassium given. These means all failed in affording any relief, and he was then brought gently under the influence of small doses of blue pill. When his mouth became affected, he appeared to experience great relief, but in a day or two after, he died suddenly whilst eating his dinner. He was admitted on the 12th of April, and died on the 14th of May, having been ill therefore about ten weeks.

Post-mortem.—There was no disease found in the vertebræ of the neck, or in the bones at the base of the skull, but a cyst, the size of a pigeon's egg, was discovered in the right hemisphere of the cerebellum, embedded in it, and almost entirely covered by the cerebral substance. The walls were very thick and rather vascular, and it contained a thick, yellowish, clear fluid, with also a clot of blood, which occupied about a third of the cyst.

Case II.—Mrs. M., a lady about 34 years of age, sent for me on the 8th of May, to see her in consultation with Mr. Kay. The previous summer she had suffered from a severe and acute affection of the brain, for which she had been bled in the arm. She was pregnant at the time, but soon recovered from the attack. Five months previous to my seeing her, she began to suffer severe pain in the head, for which almost every kind of treatment was tried—counter-irritation and depletion, as well as tonics, but all without the slightest permanent benefit. I found that she had very severe pain in the right side of the head, behind the ear. Occasionally, and especially when she moved her head, most acute paroxysms came on, causing her to scream out. At other times she was almost free from pain, but was always rather unwilling to move her head, lest it should bring it on. There was great irritability of the stomach, and she frequently vomited dark bilious matter. The bowels were rather costive. The tongue was a good deal furred, especially at the back. There was no heat of skin, or febrile disturbance, and the pulse was generally 90 or under. She had deafness of the right ear, but the sight was unimpaired, and there was no loss of sensation or muscular power.

She was brought gently under the influence of small doses of blue pill, and for a week after she was comparatively free from pain, so that hopes were entertained of her recovery.

The pain and vomiting then returned as badly as before, and belladonna and

Indian hemp were tried with some advantage. Morphia, however, was found to afford the most relief.

There was no particular change till the 20th of July, when she had a very severe paroxysm of pain, and died rather suddenly.

Post-mortem.—On examination of the brain, a cyst was found in the right hemisphere of the cerebellum, about the size of a small hen's egg, and contained a clear yellowish serous fluid. The cyst was lined with a very fine membrane, like serous membrane. The brain was otherwise healthy, and there was no disease of bone.

Remarks.—There can be little doubt, that in the first of these cases the cyst was the result of apoplectic effusion. Though not brought out by the history of the case, this seems sufficiently evident from the fact of there being a portion of clot in the cyst. In the second case there had been a previous acute attack in the brain, the real nature of which, though not exactly known, was probably of a similar description. These cases show us some very important practical facts, and I have not met with any recorded cases exactly like them. They prove that cysts in the cerebellum are, at least, by no means innocuous, but that they may produce the most intense description of paroxysmal pain, and that they are probably a more common cause of sudden death than is generally known.

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20. *Cases of Ulceration and Perforation of the Vermiform Appendix.* By Dr. A. MERTENS, of Berlin.—Before referring to the cases, Dr. Mertens makes some remarks upon the vermiform appendix; he notices its constant presence, not only in man but in apes, in the animals which walk erect, and its peculiar structure indicating that it must have an especial function. The elder Monro supposed that the vermiform appendix served to keep the neighbouring intestine moist. In modern times it is merely looked upon as a rudiment of what we see in perfection in the lower animals. Neither of these views is well founded. Dr. Mertens throws out a suggestion that the vermiform appendix may possibly be analogous to the pancreas, playing the same part, or a like part, with reference to the cæcum, as the pancreas does to the duodenal digestion. From his own investigations, he is led to believe that the cæcal end of the appendix is surrounded by muscular fibres, which may be looked upon as a sphincter muscle. The functions of the cæcum are not well understood; some authors have supposed that a secondary digestion takes place in it, and have consequently denominated it the second stomach, but this is all hypothesis. It would be well if some one with time and opportunity at his disposal would institute a series of experiments, and investigate the structure, the comparative anatomy, and physiological significance of the vermiform appendix. That it has great pathological importance is proved by the cases in which ulceration has caused death, and that it is not a mere portion of intestine we may assume from the fact that dangerous consequences arise from the presence of feces or foreign bodies. It is remarkable, that young persons are generally the subjects of ulceration and perforation of the vermiform appendix, a fact which reminds us of the observation of an English author, namely, that the vermiform appendix appears to have no other function than that of arresting cherry-stones swallowed by thieving children.

Dr. Mertens relates two cases which occurred under his own observation. The first was a child of four years of age, which had been previously healthy; in the afternoon, after a hearty meal, the child was attacked by severe pain in the abdomen, which was considered to be cold by the mother, and treated accordingly. The pains, however, became more intense, with scarcely a pause, and nausea and inclination to vomit also now being added. Towards morning of the following day, the child was first seen by the reporter, who found it with cold extremities, a small, quick, thread-like pulse, and a distended abdomen, sensitive on pressure; the tongue was not loaded, there had been no evacuation from the bowels for twenty-four hours. There could be no doubt of the existence of intestinal inflammation. Calomel in large doses was directed, and soothing enemata administered. These remedies proved unavailing, the nausea and sickness persisting. Ice swallowed in small pieces afforded some relief. On the following day, the state of things was such as to indicate approaching death; the hands and feet were covered with cold sweat, the pulse thready, the abdomen hot and

tympanitic, and vomiting of a coffee-brown-coloured fluid was now present. There had been no evacuation from the bowels; all means directed to attain this object had proved ineffectual. The child died towards four o'clock next morning. The body was examined thirty hours after death. The contents of the thorax were free from disease. On opening the abdomen, a quantity of fœtid gas escaped, also some yellowish fœtid fluid. The stomach was contracted; the liver, spleen, pancreas, and small intestines were perfectly sound. Some inches of the cæcum were intensely red; the vermiform appendix was narrowed at its junction with the cæcum, and at its free end thickened, swollen, red, and perforated by two ulcers. Neither in the appendix itself nor in the peritoneal sac could any foreign body be discovered. The large intestines were free from disease, and the peritoneum did not present any alteration, except that, in the neighbourhood of the cæcum it was strongly injected, but not inflamed.

The second case was of interest from its being diagnosed during life, and also from its being caused by a cherry-stone, which, being forced into the appendix, was the cause of perforating ulceration. The subject was a child of five years of age, who was in blooming health, stout, merry, and full of life and spirit; he was seized with severe abnormal pains in the evening, which increased from hour to hour. On the following morning he began to vomit, the first matters ejected being the remains of food, afterwards a bilious dark-brownish fluid. In the night there was an alvine dejection, but henceforward the constipation was complete, continuing until death, no means employed being adequate to overcome it. The belly was greatly distended, and painful on pressure, but this sensibility was greatest in the neighbourhood of the cæcum. The countenance was not so sunken, nor the expression so anxious, as is generally the case in abdominal inflammation. This circumstance, the manner in which the disease commenced, its duration (nearly three days), with the situation in which the pain was first experienced, and lastly, the absence of fecal vomiting, led to the belief that the vermiform appendix was the seat of the affection. Tympanitis increasing, and frequent attacks of vomiting of a dark fluid, but destitute of fecal odour, supervening, death ensued. On examination of the body, all the organs were found healthy. The stomach was empty and contracted; the intestinal canal full of air, but otherwise normal; the vermiform appendix was of a blackish-blue colour, and at one side was a gangrenous perforation; a cherry-stone was found not far from this, enveloped in a purulent mass.

In addition to these two cases which came under his own notice, Dr. Mertens gives two similar cases reported by Mr. Amyott, and published in the *Medical Times*. He notices the fact of the four cases occurring in young persons of the male sex—perhaps an accident. He regrets that he has not an opportunity of considering the subject as regards age, sex, &c. &c., but refers to Rokitansky's works, and Copland's *Dictionary of Practical Medicine* (article "Cæcum"), for further information.—*Dublin Hosp. Gaz.*, Oct. 15, 1858, from *Journal für Kinderkrankheiten.*

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

21. *Disarticulation of the Scapula from the Shoulder-joint.*—A case of this was communicated to the Royal Med.-Chirurg. Soc. (Dec. 14, 1858), by G. M. Jones, Esq. The patient, a girl aged fourteen years and a half, had enjoyed good health until December, 1857, when she first felt pain at the top of the left shoulder, ascribed to the exertion of carrying a heavy child; severe inflammation about the shoulder followed, being most intense over the upper part of the humerus; an abscess formed, and burst spontaneously. The constitution suffered severely. When Mr. Jones first saw the patient, there were four large fistulous openings over the left shoulder, two communicating with the clavicle, one with the head of the humerus, one with the glenoid cavity, and one with the dorsum of the scapula, bare bone being easily felt in each. Several small fistulæ, which

did not apparently lead to necrosed bone, existed in the scapular region, and yielded an offensive discharge. The textures covering the shoulder were generally thickened and puffy, and tender to the touch. The patient's health and strength were failing rapidly, and Mr. Jones deemed operative interference imperatively called for. Accordingly, on the 19th of May, 1858, the patient having been placed under chloroform, the operation was performed. An incision was first made along the whole extent of the spine of the scapula, and carried an inch beyond towards the mesial line of the back; another incision was then made to meet this along the upper border of the bone down to its angle. The integuments were raised by careful dissection, and by this process the whole bone was fairly exposed, its periosteal investment being everywhere found so thickened, pulpy, and softened, as to yield easily to pressure of the finger. The acromial end of the clavicle being found to be softened and altered by disease, an inch of the bone was removed. The posterior scapular artery was the only vessel which needed a ligature; several small ones were closed to by torsion. Sutures and strips of plaster were used to bring the edges of the wound together, the deeper cavities, including the glenoid fossa, being plugged with lint. The operation occupied three-quarters of an hour. On examination, the scapula was found so extensively diseased that its characters were almost destroyed. The glenoid surface and neck were entirely removed, and no vestige of the spine remained, its position being occupied by new, irregularly-deposited osseous matter, at the base of which lay a deep chasm, that extended three-fourths across the body of the bone. The inferior angle of the bone was the seat of extensive caries. The body of the bone presented two deep perforations, and all these different cavities contained sequestra of dead bone, while other portions of the scapula were in different stages of exfoliation. There was also a large amount of new osseous matter deposited in different situations about the bone. The head of the humerus was found healthy, and covered with its natural cartilage. With the exception of a very critical condition during the first two or three days, which was ascribed to the action of the chloroform, the patient made a good recovery. The wound healed entirely by granulation, the head of the humerus being exposed for some time. She was unsparingly supplied with nutritious diet and stimulants. She left her bed and walked in the garden in three weeks and as many days, and at the end of a month she could sew without pain or difficulty. At present she can raise her arm twelve inches from her side, and can support it horizontally from the body, with very slight exertion; she can raise the hand to the opposite shoulder or to the mouth with ease, but not to the top of the head; she can put her arms behind her, can lift a large and heavy hospital register book, and can scrub the floor or make her bed. There is a decided falling of the shoulder, but by no means amounting to deformity. There is no wasting of muscular substance on the chest or back, and when dressed it would not be perceived that any serious operation had been performed. The deltoid is fully developed. The head of the humerus is easily felt moving freely in its new bed, and not the slightest pain is felt on any amount of motion. Up to this present time, the range of this motion has steadily increased, and, with the exception of the movements for which the scapular origin of the deltoid is indispensable, it may be confidently expected to increase still further, and nearly to equal that of the other arm.

The author concluded his communication by observing that he considered there was much less danger from hemorrhage in the removal of the entire bone than, as in Mr. Liston's case, in the excision of only a portion; but he should prefer the former operation in any future case, and he trusted that the relation of this successful instance would go far to remove any prejudices now existing against this operation.—*Med. Times and Gaz.*, Jan. 1, 1859.

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22. *Statistical Analysis of* 186 *Lithotomy Operations.*—We find in the *Med. Times and Gaz.* (Jan. 8, 1859) an interesting statistical analysis of 186 lithotomy operations performed in different London hospitals within a period of three years and a half, ending July, 1857.

Of these 186 cases, 146 resulted in recovery, and 40 ended in death. Of the whole number, 137 were under the age of 20, and of these 123 recovered and only 14 died; while of the 49 in which the patients were adults, we find but 23

recovered, and no fewer than 26 died. These figures show in a very strong light the influence of the age of the patient upon the prospects of a lithotomy operation. We shall now proceed to exhibit the extent of this influence in greater detail, and then to examine as to the several causes of the fatality of this operation, and their relative importance.

Influence of the Patient's Age on the prospect of Recovery.—In the subjoined table the cases have been classified according to the patient's age:—

Age.	No.	Recovered.	Died.	Per cent. of deaths.			
1	1	1	...	...		Under 10 years of age.	8 deaths in 109 cases, or 1 in every 13.6.
2	8	7	1	12.5	1 in 8		
3	24	20	4	16.6			
4	15	15	...	...			
5 to 8	43	40	3	7	1 in 14		
8 to 10	18	18	...	...			
10 to 15	21	15	6	28.5	1 in 3.5	Between 10 and 25 years of age.	7 deaths in 36, or 19.4 per cent. or 1 in every 5.
15 to 20	7	7	...	...			
20 to 25	8	7	1	12.5	1 in 8		
25 to 30	3	2	1	33.3	1 in 3	Between 25 and 45.	4 deaths in 8, or 50 per cent. or 1 in every 2.
30 to 35	3	1	2	66.6	2 in 3		
35 to 40	2	1	1	50.0	1 in 2		
40 to 45	...	...	...	...			
45 to 50	3	1	2	66.6	2 in 3	Between 45 and 60.	9 deaths in 17, or 52.8 per cent. or more than half.
50 to 55	5	3	2	40.	1 in 2.5		
55 to 60	9	4	5	55.5	1 in 1.8		
60 to 65	7	1	6	85.6	6 in 7	Between 60 and 75.	12 deaths in 16, or 75 per cent. or 3 in every 4.
65 to 70	7	3	4	57.0	4 in 7		
70 to 75	1	...	1	100.			
75 to 80	1	...	1	100.			
Total	186	146	40				

Thus it would appear that between the ages of 8 and 10 is the period in which the lowest death-rate after lithotomy prevails. Of 18 operations performed on patients between these ages all resulted in recovery. If we group together all between 5 and 10, we shall have 61 cases, out of which only 1 in 20 ended fatally. Children under the age of 5 appear to bear the operation not so well as those a little older, since of 48 operations 1 case in 9 ended in death. If we pass by the fact that no death appears to have occurred out of the 7 cases between the ages of 15 and 20 as probably (on account of the smallness of the number) a coincidence, we may allege that amongst adults the rate of mortality rises with the age of the patient. Of those under 10, only 1 death in 13.6 cases occurred; in those between 10 and 25 the rate has risen to 1 in 5; in those between 25 and 45 it has reached 1 in 2; between 45 and 60 it is actually more than half; while subsequent to the age of 60 it attains the frightful proportion of 3 in every 4. There can be no doubt that this appalling mortality in patients of advanced age is in part produced by the fact that of late years the best subjects have been treated by lithotrity, and that, in the hands of many surgeons, only those patients not considered to be in sufficiently good health to bear the latter have been submitted to lithotomy. Still we regret to know that this circumstance has but a very limited application, since lithotrity has been practised to a very small extent.

The explanation of the comparative freedom from risk in young patients is to be found in the fact that disease of the kidneys is a very common concomitant of vesical calculus in grown-up persons, and a very rare one in children. The tables about to be adduced, exhibiting the cause of death at the several ages, will show that, of all the various evil influences, renal disease is by far the most important. In the list of 14, however, in which death was referable to it, we

find only 3 cases in which the patients were under 20, and only 1 in which the child was under 10. On the contrary, of the four who died of sthenic peritonitis, all were under 20, and 2 under 10. Peritonitis, hemorrhage, and shock of the operation, rank as the chief causes of death in childhood.

Much importance has been attached by some writers to the weight of the stone as a means by which to estimate the risk incurred. The heavier the stone the greater the danger. To Mr. Crosse, of Norwich, we owe a valuable collection of data on this point. Quoting Mr. Crosse's tables, Mr. Coulson, in his work on diseases of the bladder, actually goes so far as to assert: "The chance which a patient has for recovery can, therefore, be calculated beforehand, and, independent of every other consideration, from the ascertained dimensions and weight of the stone." The statistical fallacy here involved is transparent. Of course, children have, as a general rule, much smaller calculi than adults; and, as we have seen above, they are, by very far, the best subjects for lithotomy. The cause of the freedom from risk in children is not, however, to be found in the fact that their calculi are lighter, which is little more than a coincidence; but in their usual freedom from renal disease. Of course, no one will deny that the large size of a stone is a circumstance of some prejudice to a patient's prospects. A surgeon would, however, commit an absurd error if he should imagine that a boy of ten, from whom he had removed a mulberry calculus weighing several ounces, had only an eighth part of the chance of recovery which was possessed by a man of sixty from a phosphatic one of not as many drachms in weight had been removed. In all probability the reverse estimate would be nearer the truth. The fallacy to which we have referred, viz., that difference in weight of the stone is, as a rule, coincident with difference in age of patient, appears to us to make any statistical calculations of weights quite valueless for purposes of prognosis. All that can be said is that of patients of equal age and similar degree of health, those who have large calculi have a somewhat worse chance of recovery than the others; and even this rule must be allowed to receive constant modifications from consideration of the composition of the stone in each individual case. Leaving what we have to adduce, as to the influence of the previous health of the patient upon his chances of recovery after lithotomy, to another part of the report, we will now pass to

Causes of Death after Lithotomy.—In the following list we have endeavoured to classify the cases according to the accident or lesion which appeared to have exerted the most efficient influence in bringing about the fatal event. In many cases, of course, more than one had been at work at the same time. Thus, some cases in which the main lesion appeared to be disease of the kidneys had also inflammation of the bladder and of the pelvic cellular tissue; while in others, which died ultimately of some distinct visceral disease, hemorrhage occurring during or after the operation, might be reasonably supposed to have diminished the patient's chance and predisposed him to the attack. The classification is, therefore, only an approximation to the truth, and must be so considered. Proceeding on this plan, we find that, of the forty cases ending fatally, death was referred to

Renal disease	in	14	instances.
Hemorrhage	"	4	"
Pyæmia	"	4	"
Peritonitis	"	4	"
Shock of operation	"	2	"
Extravasation of urine	"	2	"
Abscesses about the bladder	"	2	"
Wound of the fundus of the bladder by the knife	"	2	"
Exhaustion	"	1	"
Convulsions	"	1	"
Cystitis	"	1	"
Bronchitis	"	1	"

In the remaining two cases we do not possess sufficient detail to allow of our assigning to any class without risk of error.

Renal Disease as a Cause of Death.—In the following table we have arranged

the fourteen cases in which disease of the kidneys was the lesion to which death was attributable:—

No.[1]	Age.	State of health.	Date of death.	REMARKS.
126	10	Delicate	11th day	Stone weighed 2 drs. Both kidneys disorganized.
41	14	Bad	11th day	Much bleeding; protracted operation; peritonitis.
31	14	Moderate	4th week	Abscesses in the right kidney.
12	21	Good	10th day	Stone large; acute inflammation of kidneys and ureters.
103	37	Bad	3d day	The subject of diabetes insipidus.
53	54	Very bad	6th day	
171	56	"	2d day	Stone large; he sank from the shock.
73	58	"	15th day	Had suffered from stone for ten years.
140	59	Hopelessly ill	2d day	Stone large; extensive disease of liver, kidneys, and bladder.
147	63	Bad	6th day	Calculi in the kidney; acute inflammation.
109	64	Good	19th day	Two large phosphatic stones; abscess behind the bladder, as well as diseased kidneys.
86	68	Very bad	3d day	Stricture of the urethra.
94	68	"	4th day	He was paralytic; acute inflammation of bladder; cystic disease of both kidneys.
116	68	Feeble	12th day	Calculi in the kidney; small stone.

We have not included in the above list those cases in which acute disease of the kidneys appeared to be secondary to a general pyæmic state. In most of those included there was reason to believe that renal disease existed prior to the operation. It will be seen that in two instances the patient sank on the second day, no doubt directly from the shock of the operation, while in five others death occurred within a week. In two cases, the operation was done at the patient's urgent request, in the hope of mitigating his sufferings, but under circumstances which almost precluded the hope of recovery.

Hemorrhage as a Cause of Death. (Five Cases.)—In the following five cases so much blood was lost, either at the time or afterwards, that it seemed fair to attribute the patients' ill progress to the debility induced.

No.	Age.	State of health.	Date of death.	REMARKS.
20	5	Good	8th day	Five or six attacks of secondary hemorrhage.
41	14	Bad	11th day	Free bleeding at the time, none afterwards.
157	26	Poor	6th day	Severe hemorrhage on same day (venous).
4	45	Fair	40 hours	The bleeding was venous, and at the time. There was a growth of cancer in the bladder.
131	79	Good	7 days	Secondary hemorrhage on the third day.

In only the first and third of these was the fatal event directly and unmistakably induced by the loss of blood. In the others, the hemorrhage acted rather as a predisponent. It is interesting here to remark that the two cases in which

[1] The numbers given in this column refer to the registry of the cases in the writer's note-book, and are retained only for the convenience of reference.—J. H.

of all in our list most blood was lost, both recovered. In one of these there was little doubt that the internal pudic had been cut, and the hemorrhage was most alarming. Case number 41 is one in which the cause of death was very complicated. It has already been mentioned among the deaths from renal disease.

Pyœmia as a Cause of Death.—(Four Cases.)

No.	Age.	State of health.	Date of death.	REMARKS.
159	13	Delicate	19th day	
15	31	Very bad	8th day	Diseased kidneys.
121	60	Very stout	6th day	
158	60	Good	2d week	

Pyæmia has hitherto not been much considered as a cause of lithotomy fatality. There can be little doubt that it has prevailed of late years much more extensively than it formerly did. The four cases in the above group were examples of the disease in its typical form. The first we have already published in full detail; in it purulent depôts were found in several organs. In the second, a man in miserably ill health, worn out with suffering, had lithotomy performed at his own request, and died on the eighth day, having had rigors and other symptoms of pyæmia. The kidneys were found disorganized, there were small deposits in the apices of the lungs (tubercular?), and in one knee-joint was a collection of pus amounting to four ounces. The third is a very remarkable case, on account of the early period at which the disease set in. The patient, a man of 60, appeared in good health, but was very stout. The stone was a large one, but was got away without unusual difficulty. A severe rigor occurred within twenty-four hours of the operation, and pain was complained of in one shoulder-joint. Profuse sweatings and an intermittent pulse soon followed, and he rapidly sank. At the autopsy, both knees and both shoulder-joints contained pus; but the lungs, liver, and other internal viscera, were healthy. In the fourth case, purulent depôts had formed in the cellular tissue of both arms.

It is very possible that in certain other of the cases a dyscrasia, closely allied to the pyæmic, was connected with the cause of death. In those, for instance, in which acute disorganizing inflammation of the kidneys was found, such a suspicion might fairly be entertained. We have preferred, however, only to include under this heading those cases in which the nature of the disease was well marked. As being the direct cause of death in one-tenth of the fatal cases, it is evident that pyæmia must henceforth assume an important position in the rôle of dangers to be encountered in lithotomy practice.

It will be for additional observation to confirm or alter the impression; but we cannot help thinking that the occurrence of external suppurations, either in joints or cellular tissue, is more common when pyæmia follows operations on the bladder or urethra than when it is consequent on those in other parts. Also that deposits in the internal viscera are proportionately less frequent after the former. This impression is formed on the observation of several instances of fatal pyæmia after the external division of stricture, and after lithotomy, etc.

Pelvic Suppuration as a Cause of Death.—(Four Cases.)

No.	Age.	State of health.	Date of death.	REMARKS.
72	50	Fair	1 month	Extensive suppuration between bladder and rectum.
117	61	Good	10 days	The stone weighed nearly four ounces.
109	64	"	19th day	Inflammation of bladder and abscess behind it.
94	68	Very bad	5th day	Paralytic; stone small, but lodged in a pouch.

Of the above four, two cases (94 and 109) are given also under the head of renal disease, as in both there was extensive disease of the kidneys, as well as abscesses about the bladder.

Shock of the Operation. (Two Cases.)—In seven cases, the patients died within forty-eight hours of the operation. In five out of these there were other ostensible lesions to which death was referred; so that in two only did it appear attributable to mere shock. In one of the latter, the patient was a delicate boy, aged 3; the operator had withdrawn the staff before it had entered the bladder, and much difficulty had been encountered in completing the operation. There was no doubt but that, in addition to the prolonged operation, much more than the ordinary amount of injury was inflicted on the parts. The child sank and died on the third day. In the second case, a heavy, corpulent man, aged 62, sank into low delirium soon after the operation, and so continued until the third day, when death occurred.

Other Causes of Death. (Eight Cases.)—The eight cases which yet remain to be accounted for may be classed as follows: In two men, one aged 56 and the other 70, both in fair health, death appeared to result from extravasation of urine into the cellular tissue of the scrotum and perineum. In both, the scrotum sloughed most extensively. In one of these, death took place on the seventh, and in the other, on the fourteenth day. It is not a little remarkable, the extravasation of urine, to which formerly a very large proportion of deaths after lithotomy used to be ascribed, should have come to be regarded as one of the least frequent sources of danger. In one other case (No. 90), a child, aged —, previously in good health, died in convulsions thirty-six hours after the operation. In another (No. 77), a child of two years old, also in good health, sank within forty hours from acute bronchitis. This case was one of so much interest that we recorded it at the time in full detail. In one (No. 53), we ought, perhaps, scarcely to class the case as a lithotomy fatality, since the child had so far recovered as to have been discharged from the hospital. It was that of a very delicate child, aged 3, admitted with a perineal abscess and fistula, communicating with the urethra. After the operation, all did well, but at the time of discharge the wound was not quite healed. The child was re-admitted in a much exhausted state, and finally sank six weeks after the operation. In two cases, death was directly referable to wound of the fundus of the bladder by the operator's knife. Both the patients in whom this accident happened were adults. In each, extravasation of urine into the peritoneum of course followed. The last remaining case is one in which, in an elderly man with enlarged prostate, not only was the stone extracted, but the projecting middle-lobe of the prostate was cut away. Death from inflammation of the bladder followed three weeks afterwards.

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23. *Ecraseur in the Operation for Anal Fistula.*—Mr. F. B. Quinlan, Surgeon to St. Vincent's Hospital, recommends the ecraseur for the operation of anal fistula, especially in severe forms of this disease occurring in persons of a broken-down or debilitated constitution. He prefers the instrument as improved by Charriere, in which instrument one end of the chain is fixed and the other is tightened by a nut working on a fine screw, each turn of which brings in one-tenth of a link. (See fig. in No. of this Journal for January, 1857, p. 56.) "The *ecrasement*, as accomplished by this latter instrument, whether it be quickly or slowly performed, is a process of crushing, smooth and perfectly free from jerking or laceration. If the surgeon be operating on a part in which there is no great dread of hemorrhage (if it be judged necessary to use the ecraseur in such a case), he can cut through it with M. Charriere's modification as quickly as with the original instrument. If, on the other hand, he be engaged in removing internal piles, vascular tumours, portions of the tongue, &c., he can bring in at each movement as small a fraction of a link of the chain as circumstances may dictate, and thus be insured, no matter how vascular the part may be, against all risk of bleeding. It has been objected to Charriere's modification, that in its use a portion of the part to be removed is apt to be dragged into it, instead of being smoothly cut through. If, however, the chain fit accurately to the slit in

the neck of the instrument—as in every well-made ecraseur it should—I do not see how this inconvenience can occur."

The only inconvenience experienced in operating on anal fistula with the ecraseur, Mr. Q. says, is in the introduction of the chain, but this may be easily accomplished as follows: "A long probe, bent to a proper curve, is passed through the perineal opening of the fistula, into the rectum, and out through the anus. To the eye of this probe is attached a piece of silk cord, the other end of which is firmly tied to the end of the chain (that end which is not provided with the hook or catch, of course, being preferable from its smaller size). The cord being drawn through the anus, is made tense by the left hand; the operator passes his right forefinger into the rectum, until he meets with the cord passing through the internal opening of the fistula. Hitching this part of the cord on his nail, he pushes it into the rectum as far as his finger will go; and in this way, by bringing a traction on the chain, as if from the interior of the pelvis, draws it easily and expeditiously into the rectum, and then out through the anus. This method (which was first used and recommended by Dr. O'Ferrall) has the advantage of getting the chain through with the smallest amount of laceration of the parts; in fact, I have never seen a fistula through which the chain could not be easily passed in this way on the first trial. It is certainly preferable to the plan of introducing it through a canula, or to that of previously dilating the fistula with an India-rubber tube."—*Dub. Hosp. Gaz.*, Jan. 15, 1859.

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24. *New Method of curing Hydrocele.*—This method, suggested by Dr. SIMPSON, at a meeting of the Medico-Chirurgical Society of Edinburgh, is founded on the fact that iron and other metallic wires, when placed in contact with living tissues, did not, as a general law, excite inflammation to a higher stage than that of adhesion, or the effusion of coagulable lymph. Dr. Rothmund, of Munich, performed the radical cure of hernia by exciting adhesive inflammation in the returned hernial sac, passing, for this purpose, and leaving for eight days, a metallic needle traversing the peritoneum; and had not, it was averred, lost a single patient out of 1000 operated on. If metals in serous sacs create a higher stage of inflammation than the adhesive, such a fortunate result as this would not have been attained. Dr. S. had thought for some time that metallic wires passed through the sac of a hydrocele would act in two ways: first, they would drain off the fluid; and, secondly, they would subsequently, by their presence, form the surest means of exciting the subsequent amount of adhesive inflammation that was required for the cure of the disease. Dr. Young had, in one of his patients, afforded him an opportunity of putting this idea to the test. Dr. S. showed the Society the slender wire or metallic seton which had been used in this case. It was passed through the sac by first traversing the sac from below upwards with a long-handled surgical needle, such as is used in transfixing and tying hæmorrhoids, threading the eye of the needle, after it was projected through the scrotum above, with three or four slender iron threads, pulling the needle then backwards through the sac and out, and thus leaving the metallic seton in its place. The liquid drained off in an hour or two; adhesive inflammation set in, and progressed for two days, when it began to subside. The wires were removed on the third day; and the cure had remained apparently quite complete, with the vaginal sac firm and consolidated. Dr. Young had promised to publish the whole case at length. This method of treating hydrocele was, Dr. S. held, much simpler in its performance than tapping and injecting; not by any means so painful to the patients; less likely to produce a suppurative or dangerous amount of inflammation; and, perhaps, experience would show also, betimes, that it was surer and more certain in its results.—*Edinb. Med. Journ.*, Dec. 1858.

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25. *Cases of Re-fracture of Bone, with Observations.*—Mr. F. C. SKEY, in an interesting paper read before the Royal Medical and Chirurgical Society (Jan. 11, 1859), brought before the society the subject of re-fracture of bone as a curative process in the practice of surgery. He refers the necessity for re-fracture to the occasional occurrence of cases of remarkable difficulty in the treatment, arising from complications of obliquity of fracture of the shaft of a long

bone, coupled with large muscular power, and unusual irritability of the nervous system. To such inherent difficulties are added a peculiar irritability of the integuments and moral wilfulness of disposition. Such, he says, are the causes of defective union of long bones, however assiduous may be the surgeon in attendance. They are not, however, selected by the author with a view to re-fracture, but are rather referred to as beacons to point to their rejection, such cases only being appropriate to the proposed treatment which are founded on causes incidental to the first fracture, but avoidable on the second. The operation of re-fracture is deemed warrantable in cases of bones of the lower extremity so united as to abridge the length of the affected limb to the extent of considerably impairing the locomotive powers of the person; and, in the case of the upper extremity, of restricting the movements of the forearm in rotation, or the fingers in flexion or extension; or, in either case, of local pain, caused by the entanglement of muscular fibre or nerve in the uniting medium of the fractured bones. The author states that re-fracture of bones, contrary to the authority of M. Dupuytren, may be effected without difficulty or danger, and at a term considerably beyond that prescribed as the ultimate limit at which re-fracture of bones is justifiable. The period is fixed by M. Dupuytren at sixty days. The author says: "I believe bones which have united to any length by lateral apposition of the entire diameter of the shaft, and in which the spaces formed by the contact of the two cylinders are filled with fibro-osseous matter possessing firmness sufficient to support the superincumbent weight of the body, are for a lengthened subsequent period susceptible of disintegration by a judiciously-applied force, to the advantage of their replacement to their natural relations prior to fracture, and without injury to the soft structures around them." The warrant for the appeal to the apparently violent proceeding of re-fracture is founded on the grounds of—1st, its practicability at a term of many weeks after the limb has been restored to exercise; 2d, its safety as an operative proceeding; and 3d, its indispensableness to the perfect utility of the limb involved. As regards the practicability: if the uniting material of bone be examined at the term of four or five months, it will be found to be composed of fibrous tissue, in which the deposit of bone is comparatively slight; while the perfect process of union is greatly protracted in cases of imperfect adaptation of surfaces, and we cannot calculate on any exact rate of progress towards recovery in cases presenting various and dissimilar conditions. In the upright position, the resistance is made by the whole length of the uniting medium. By bending the limb at the line of union, we have the resistance only of the transverse diameter. The practicability of re-fracture of a bone in any given case can only be brought to the test of experiment. It is obtainable on different conditions in different cases, depending on age, on sex, on the extent of surface involved, on the duration of the healing process, and, finally, on the tact, as well as the force of hand, employed in the operation; "but that on ordinary conditions," says the author, "it may be effected for many weeks or even months after the restoration of the limb to careful exercise, there appears to me no good reason to doubt." 2. With respect to the question of safety, the reasonable probability is considered to be that the bone will break at the site of the original fracture, and at no other part; and that the fracture may be effected without injury to the soft parts around. The author asserts the impossibility of breaking the dried radius by a powerful man; and, *à fortiori*, there can be no fear of injury done to any other cylindrical bone containing a larger quantity of osseous matter. A case is quoted in which the attempt was made to re-fracture a femur united for nine months, and no injury was sustained by the soft structures of the limb. 3. The question of interference can only be determined by the requirements of any given example. It is a question rather for the patient than for the surgeon. If fracture of the upper arm or forearm be followed by impaired utility of a character not likely to be benefited by time—supposing the union of the two bones of the forearm to be lateral, and not terminal—no evil can result from the attempt to re-fracture at any period, although the probability of success will necessarily diminish as time advances. In the case of the femur united by lateral contact, the necessity of an operation will be gauged by the imperfection of the gait in walking. The author has re-fractured the thigh-bone of a young healthy man at the period of seventy-seven

days from the date of the original fracture, and he believes the bone to have been susceptible of separation at a yet later period. The act of disuniting a bone is effected by slow laceration, rather than by a snap or fracture; it is not an immediate, but a gradual process, requiring persistent rather than sudden force—the act of tearing rather than breaking. With respect to the necessary elongation of the limb when the bone is disunited, the careful application of pulley force is recommended. The author concludes his paper by the relation of the particulars of six cases: two of re-fracture of the thigh, at the respective periods of seventy-seven and seventy-five days from the date of the original fracture; two of re-fracture of both bones of the leg, at the respective dates of the seventy-seventh and forty-fifth days, the latter operation being required in the case of a child six years of age; and one of re-fracture of both bones of the forearm, at the expiration of 120 days from the date of the original fracture. The sixth case is that of a fractured thigh, in which the attempt to re-fracture the bone at the end of nine months was unsuccessful.—*Med. Times and Gazette*, Jan. 22, 1859.

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26. *Possibility of always detecting a Fracture a few weeks after its occurrence.*—Mr. CHARLES HAWKINS mentioned, during a discussion at the Royal Medical and Chirurgical Society (Jan. 11, 1859), a case of fracture of the thigh which he had not seen until seven weeks after the accident, and when then examined it was difficult to detect any sign of fracture having existed; it was again put up in starch bandages, but the patient again met with an accident, and the bone was again fractured at the original seat of injury. This proved beyond question that fracture had existed, of which he himself had had no doubt, though some surgeons who saw the case had found difficulty in believing it. The parts had been so well adjusted that scarcely any callus had been thrown out, and hence the great difficulty of determining whether a fracture had really existed.

Mr. SPENCER WELLS said that there was great difficulty in detecting the point at which there had been fracture, in cases where the starch or the plaster of Paris bandages were well applied. He had seen a case of fracture of the ulna in a young lady last year, which was put up temporarily by Dr. Stewart, of Greenwich Hospital. Mr. Wells put it up a few days afterwards in the plaster of Paris bandage, and after six weeks it was quite impossible to detect the site of fracture. This case, like that narrated by Mr. Hawkins, confirmed the doctrine that if a fracture were well set there might be so little callus formed that it was ultimately impossible to determine by examination that any fracture had existed. —*Med. Times and Gaz.*, Jan 22, 1859.

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27. *Antiquity of Metal Sutures.*—Dr. J. H. AVELING calls attention (*Med. Times and Gaz.*, Jan. 22, 1859) to the fact that Fabricius Aquapendente, in 1647, gave all the reasons which are now being put forward for preferring the inorganic to the organic ligature. After describing the ligature of Fallopius, which was of thread, and like the one which we until lately have been using, and that of Guido, which was made of metal, and hooked the two lips of a wound together; he says that he provides himself with many flexible needles of iron or of brass, made soft, except at the point, over burning coals. These he passes through the lips of the wound, and then turns back the extremities, the right to the left and the left to the right, fitting them over the wound either straightly and plainly, or by making a knot and allowing them to remain until the wound is almost agglutinated.[1]

The following are the reasons Fabricius gave more than two hundred years ago for preferring his metal fibula to that of Fallopius, which was of thread:—

"Quod si licet aliquando paradoxum vobis afferre, dixero potius meam fibulam potiorem esse, propter rationes ex comparatione desumptas à juvantibus, et nocentibus, si quidem fibula Fallopii ex filo facta, mordet ubique carnem, quia filum asperum est, et inæquale, cum sit tortum, acus verò livigata est, et per-

[1] I think this plan of using needles might be returned to with advantage in some cases of wounds about the face.

polita. Rursus filum mordendo labia vulneris transversè ea perrodit, quod experientia passim patefacit, et confirmat; at acus flexibilis, cum rotunda sit, et levigata, nihil istiusmodi facit: exemplo sint annulli aurei, aut ferri, qui auribus perforatis diutissime gestantur, utcumque penduli sint. Rursus si filum valentius stringatur, interdum rumpiter, quo non patitur acus mollis, ferrea, aut œnea. Amplius filum est materia, quæ facilè tenditur, et laxatur, ferrum verò flexibile neutiquam laxatur. Amplius laxitas ex filo dupliciter succedit, tum ex laxa fili natura, tum ex perrosis labiis, unde etsi à filo labia vulneris ad mutuum contactum adducuntur, non tamen adducta conservantur, quia propter fili naturam dupliciter laxantem disjunguntur, et hiant; sed neutram laxitatem ex acu flexibili rotunda, et perpolita expectare oportet. Ultimò filum non difficulter putrescit à sanie, et ichoribus, at acus ferrea, aut œnea, immunis est ab hujusmodi labe. Quod si tandem addatis, æs, et ferrum habere vim refrigerandi, et adstringendi, vulneris glutinationi consentaneum erit: et hoc est argumentum veritatem pardoxi omnino comprobans, et confirmans."[1]

These advantages may be summed up thus: 1. Iron does not eat into the flesh. 2. It does not ulcerate out. 3. It does not stretch and break. 4. It is not rotted by the discharge.

What more remains to be said than is here stated? Is it not curious that we are only just now beginning to appreciate the fact which Fabricius gave to the world so many years since?

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28. *Perchloride of Iron in In-growing Nail.*—After fomentation, Dr. ALCANTARA interposes beneath the nail a small piece of lint, upon which some ointment of perchloride of iron has been spread. All the surface of the excrescence deprived of its epidermis is covered over with this, and the dressing is renewed twice a day. At the end of four days, the excrescence becomes dry and mummified, and is easily detached. The wound then assumes a healing aspect, and the case is completed at the end of a week—*Union Med.*, No. 16.

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29. *Therapeutical Action of the Perchloride of Iron in the Treatment of Acute and Chronic Urethritis.*—Dr. BARUDEL, in his investigations upon the therapeutical employment of the perchloride of iron, found that this agent exercised a special influence on the genito-urinary mucous membranes, for, from the period when it was administered internally, the inflamed membranes were rapidly affected, and no longer secreted the abundant puriform fluids which accompany the inflammations of the genito-urinary tract. The perchloride appears to owe its efficacy to a double action, for it possesses a hæmostatic power which retains the blood in the capillary vessels, and opposes its escape, and also a sedative power which rapidly restores the functions of the capillary circulation to a regular mode of action. When the perchloride of iron was administered internally, Dr. Barudel found that it lowered the pulse from seventy to sixty, and even to fifty, in the minute, and he therefore was induced to employ it in many cases where it was essential to reduce vascular excitement. The duration of urethritis was certainly shortened by this medicine, and cases which ordinarily required a period of thirty or forty days for their cure were successfully terminated in eight or twelve under the use of the perchloride, which was administered internally in cases of acute and chronic blennorrhagia, and was also employed in the form of a styptic injection to the affected membrane. The employment of this treatment has never been attended, in the hands of Dr. Barudel, with any unfavourable results, so as to induce him to discontinue it, although, as is well known, a multitude of serious complications often spring up in the course of blennorrhagia.

In all the cases treated by Dr. Barudel, the treatment was identical, except in some rare instances. In acute urethritis, an injection of iodide of lead was employed, this salt being almost insoluble and exciting no pain in the inflamed mucous membrane; the solution of perchloride of iron in injections was reserved for chronic blennorrhagia, the constriction which it exercises upon the urethral canal rendering it more applicable to the chronic than the acute affections. The internal treatment of all the cases consisted exclusively in the administration of

[1] De Chirurgicis Operationibus, p. 146.

a mixture containing twenty drops of tincture of perchloride of iron, taken every two hours. In general, at the end of three days a certain improvement was effected; but no aggravation of the inflammatory condition of the parts was ever produced by this internal and external treatment, either in the acute or the chronic cases. The cure was almost always completed in fifteen days. In chronic urethritis, the only modification consisted in the use of an injection made of tincture of perchloride of iron and distilled water; this injection was repeated three times a day, taking care that the fluid should remain at least ten minutes in the urethra. The general treatment was of a strengthening nature, wine being allowed to the patients. Dr. Barudel quotes two cases, successfully treated in the military hospital at Lyons by the means just described, and he draws the following conclusions from his researches: 1. That perchloride of iron may be used successfully, both externally and internally, in the treatment of acute and chronic blennorrhagia; 2. That this agent is endowed with well-ascertained hæmostatic properties, and possesses also a sedative action on the general circulation; 3. That several venereal affections, which resist ordinary remedies, may be safely treated by the perchloride of iron; 4. That the perchloride, given both in acute and chronic cases, appears to act, in the great majority, as a specific; 5. That the perchloride of iron is sufficient in the treatment of simple urethritis; but that when the syphilitic element is associated with this disease, mercurials must likewise be employed; and 6. That the most important auxiliary to the internal use of iron is the employment of an injection, the iodide of lead being applicable to acute, and the perchloride of iron to chronic, urethritis.—*Brit. and For. Med. Chirurg. Rev.*, Jan. 1859, from *Bull. Gen. de Thérap.*, May, 1859.

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30. *Injections in Gonorrhœa.*—Prof. SIGMUND, of Vienna, as the result of his extensive observation in this class of diseases, is decidedly in favour of the employment of injections in the treatment of gonorrhœa. He believes those who have derived no benefit from their use, or who have observed mischievous consequences from this, have, in the great majority of cases, employed them improperly. He has tried injections with balsam of copaiba, and with chloroform, but has given them up as unpractical, and those made with the patient's own urine, while taking balsam copaiba, were found to be as inert as water. From among a large number of substances tried, he confines himself now almost to sulphate of zinc, acetate of zinc or lead, alum, and tannin; and of these he prefers the sulphate of zinc to all others, because the great majority of patients are cured by it; it acts mildly, neither soiling the linen, nor changing the colour of the urine, and it is very cheap.

For injections to succeed they must be used at the proper time, in a suitable dose and manner, and they must be continued sufficiently long. The period for their employment has arrived as soon as the inflammation of the mucous membrane of the urethra has become subdued; but they should not be used as long as there is present considerable swelling, great, or even slight, if continuous, pain, spasms, or frequent calls to pass urine. The dose of the material should be small, as five grains to the ounce of extract of lead, one quarter of a grain of nitrate of silver, one grain of sulphate or acetate of zinc, etc. It is seldom necessary to increase the original dose. The addition of anodynes, as opium, hyoscyamus, etc., has no advantageous effect. We should carefully teach the patient how to use the injection; and a small tin syringe, with a conical tube, is to be preferred. It should hold at least two drachms. The patient should be placed in the upright position, and should pass urine prior to the injection being thrown in. The tube must be so passed into the urethra, that no fluid can flow out between the canal and the tube. The fluid is now to be slowly thrown in, and then the mouth of the urethra is to be kept closed by two fingers, so that nothing can pass out during two or three minutes. Two injections are to be thrown in one after the other, and they are to be repeated three or four times daily. The injections should not be thrown in just before going to sleep, as they then sometimes give rise to seminal discharges. They must be persevered in for eight or ten days, after all traces of diseased secretion have ceased to be visible, even in the morning. The average time required will be from twenty-one to

twenty-eight days. Internal means may also, if desired, be employed, and balsamic medicines, in many cases, hasten the cure.

Dr. Sigmund rarely has recourse to caustic injections, as the nitrate of silver, sulphate of copper, chloride of zinc, etc., because generally the experiment is dangerous. He limits their use to simple, uncomplicated gleet, which has resisted the usual means, as also to recent gonorrhœa without inflammation occurring in persons who have already employed the treatment with advantage.—*Schmidt's Jahrb.*, bd. xcviii. p. 49.

31. *Suffocation from Foreign Body in Rima Glottidis.*—Mr. G. H. Porter presented to the Surgical Society of Ireland in 1856, a pathological specimen, which afforded an example of a piece of boiled mutton blocking up the rima glottidis. Recently (Jan. 20th), he presented a similar specimen, obtained from a man who dropped suddenly from his chair whilst eating his dinner, and struggled for about two minutes, when life became extinct. He was at the time intoxicated. When seen by Mr. P. the man's face was almost purple; lividity was well marked over the surface, particularly on the posterior aspect. On examination, he found that death had been caused by the plugging up of the rima glottidis by a large piece of mutton, which produced suffocation. His lungs and head were congested, and also the lining membrane of the trachea. He believed some surgeons were of opinion, that the way suffocation was produced, in such cases, was by the shutting down of the epiglottis. It had fallen to his lot to examine seven cases in which death was produced from suffocation in this way, and he only found the foreign body lying on the epiglottis in one. In all the others it was found plugging up the rima glottidis, as in this case—in two, the foreign bodies were large pieces of mutton; in three, they were large pieces of boiled beef; and in another, there was a large piece of bacon. The seventh was one in which the whole trachea was filled with a quantity of meat and vegetables—he could not tell the description of meat. It was rather a curious case. The man became suffocated from vomiting; the contents of the stomach having fallen back while he was in the act of vomiting, and filled up the windpipe. In six of the cases the parties were all in a state of intoxication when they met their death.—*Dublin Med. Press,* Feb. 9, 1859.

32. *Descent of a Testis in a Child, simulating Hernia.*—Mr. R. P. Bell relates (*British Med. Journ.*, Feb. 5, 1859) the following case, illustrative of the necessity of carefulness in diagnosis.

A fine healthy child, fifteen months old, was brought to Mr. B. by its mother, who supposed it to be suffering from a rupture, which had occurred a few minutes previously, whilst the child was standing on a sofa, when it suddenly screamed, and placed its hand on the body.

On examination, Mr. B. saw a protrusion, of the size of a walnut, at the right external abdominal ring, and found that the scrotum contained only one testicle. He explained the nature of the case, and merely ordered fomentation.

On calling to see his little patient two hours afterwards, he found him comfortably asleep, and the testis had completed its descent. The parents were not aware of the previous defect.

33. *Encephaloid Cancer affecting a Testicle which had been retained in the Abdomen.*—Dr. G. Johnson read an account of a case of this before the Royal Med. and Chirurg. Soc. (Jan. 11, 1859), which was peculiar from the severe pain which accompanied the disease in its early stages, and the great size of the tumour.

The subject of it was a well-developed, muscular man, 27 years of age, of active habits. The disease, which ultimately caused death, appears to have commenced about the month of September, 1857. At that time, while out shooting, an uneasiness low down on the right side of the belly, which had been felt for a short time before, grew into such intolerable agony that he had "to knock up" for two hours. The pain then went off, and he finished the day's sport. From that time the pain was more or less constant and severe, and on several occasions, greatly aggravated by active exercise. Dr. Johnson was first

consulted by letter in April, 1858. The description of the pain, and its situation in the course of the right ureter, suggested the notion that a calculus might be impacted in the ureter. Some questions relating to the effect of the pain upon the testicle elicited the fact that the right testicle had not descended from the abdomen. A careful examination, on the 17th of April, failed to discover any tumour in the right inguinal or iliac region. The patient's general health and nutrition were at this time but little affected. The urine was of high density, and deposited lithates and oxalates, but contained no other abnormal products. The pain continued, and the patient began to lose flesh and strength. It now occurred to Dr. Johnson, as a probable explanation of the symptoms, that the retained right testicle had become the seat of malignant disease. The patient's mother had died of cancer of the stomach. On the 12th of June, there was a consultation with Dr. Bright. At that time, there was decided evidence of a tumour or deposit in the abdomen above Poupart's ligament, on the right side. The tumour rapidly increased, until it extended much beyond the median line of the abdomen, above the umbilicus, and even to the epigastrium. The patient meanwhile became much emaciated, and died exhausted on the 7th of July. On post-mortem examination, the right testicle, situated in the abdomen, was found to be the seat of medullary disease, forming a tumour weighing sixteen pounds, even after the escape of about four pints of grumous fluid from some large cyst which had become developed in it. The lymphatic glands in the abdomen were free from disease.—*Med. Times and Gaz.*, Jan. 22, 1859.

OPHTHALMOLOGY.

34. *Iridesis: or the Formation of Artificial Pupil by tying the Iris.* By Mr. CRITCHETT.—The formation of what is commonly called an artificial pupil is required under various morbid or abnormal conditions of the eye, and demands a corresponding variety in the modes by which it is accomplished. In some cases, a restoration of the original pupil as regards size and situation is all that is wanted; in others, a change in the size, shape, and situation of the natural pupil is required; or, again, it may be necessary to form a new pupil in an abnormal situation, and in the very substance or tissue of the iris. In each of these different cases the object is the same, viz., to establish a clear pupil or aperture in the iris opposite to a transparent part of the cornea.

It is very desirable that, in the formation of an artificial pupil, the conditions upon which the perfection of the natural pupil depends, should be as nearly as possible preserved and imitated, both as regards its position and defined border, its size, mobility, and sensitiveness to light. In the methods usually employed, these conditions are frequently unattainable, and the circular fibres of the natural pupil are either cut or torn through, and an opening is formed which is very probably large and irregular in shape, fixed and insensible to light, ill defined and extending to the margin of the cornea—thus admitting rays of light that are too much refracted by the margin of the lens, and having the effect altogether of confusion of vision.

A consideration of the disadvantages attending the usual modes of operating for artificial pupil makes it obvious that it is desirable to produce a prolapse of the marginal or ciliary part of the iris, and thus to gain the power of altering the shape and size of the natural pupil in any direction, and to any degree that the specialties of the case may need. Up to the present time, this object has been attended with insurmountable difficulties. If an opening in the cornea is made, and a portion of the iris drawn out so as to form a small prolapse, this will almost invariably recede when the aqueous humor is resecreted; if its return is prevented by making the wound larger, and removing a small portion of the cornea, the prolapse may then exceed the intended limits, and the transparency and natural curve of the cornea is interfered with. These considerations have suggested to me the idea of placing a piece of fine silk round the prolapsed por-

tion of the iris—an operation to which I propose to apply the term Iridesis (Ιρις—δεσις). It is performed in the following manner: The patient, if at all restless, being placed under the influence of chloroform, the wire speculum is inserted, and, with a pair of forceps, a small fold of the conjunctiva close to the cornea is held so as to fix the eye. An opening is then made with a broad needle through the margin of the cornea, *close* to the sclerotic, and just of sufficient size to admit the canula forceps; with it a small portion of the iris, near, but not close to, its ciliary attachment, is seized and drawn out to the extent considered necessary to enlarge the pupil; a piece of fine floss silk, previously tied in a small loop round the canula forceps, is slipped down and carefully tightened around the portion of the iris made to prolapse, so as to include and strangulate it. This manœuvre requires a little practice and dexterity, and is best accomplished by holding each end of the silk with a pair of small forceps with broad extremities, bringing them exactly to the place where the knot is to be tied, and then drawing it moderately tight. A single tie is sufficient; the ends are then cut off, and the operation is complete. Little or no irritation usually follows. The small portion of iris included in the ligature speedily shrinks, leaving the little loop of silk, which may be removed from the eye about the second day. This operation has been performed many times by myself, and by my colleagues, Mr. Bowman and Mr. Poland, and the result has been in every respect most satisfactory; the size, form, and direction of the pupil can be regulated to a nicety; its mobility is preserved, and the eye speedily recovers from the effects of the operation. It is applicable to numerous groups of cases, including all those in which the natural pupil, or even a part of the natural pupil, is movable, and has a free edge. The simplest class of cases to which this operation is suitable, is a central defined opacity of the cornea, in which cases it is only required that the natural pupil should be moved slightly to one side, so as to bring it opposite to the transparent part of the cornea. By tying the iris in such a case, the defined edge and the natural contractility of the normal pupil are secured; but if the opacity is very large, and only a small margin of transparent cornea is left, the operation is not applicable. Another class of cases in which this method of proceeding may be employed, is a dense opacity of the cornea, the result of penetrating ulcer, with partial adhesion of the pupillary margin of the iris. In these cases the pupil is altered, both in size and shape, to a variable degree, and is drawn behind the corneal opacity, and in this way any useful vision is prevented. I have operated upon several of these cases by the new method, and have found that I could thus regulate the size of the displaced pupil, and preserve intact the circular fibres of the iris. It is comparatively easy, when the adhesion is slight, and only a small portion of the original pupil is implicated in the synechia, but when there has been a large penetrating ulcer involving the pupil, and thereby considerably diminishing its size, and sometimes reducing it to a mere slit, the difficulty of obtaining a useful pupil by iridesis is increased. In a case of this kind that came under my care a few weeks ago, I succeeded in obtaining a good central and movable pupil by making two openings in the cornea, about three lines distant from each other, and then drawing out and tying a small portion at each opening. The effect of this mode of proceeding was to stretch the circular fibres in two directions, so as to form a somewhat triangular pupil—movable, clearly defined and allowing of excellent sight.

I may mention, particularly, another case, as it seems to deserve special notice, partly on account of the very singular condition of parts before the operation, and also because the method adopted for its relief—viz., that of tying the iris—was the only one applicable to the case, and was very successful. A young lady, about twenty years of age, came to consult me respecting her right eye, the sight of which was very defective, particularly in a strong light. It appeared that she had suffered severely from inflammation of the eye during infancy. On examination, I found the cicatrices of two penetrating ulcers at opposite parts of the cornea, and at its extreme margin prolapse of the iris had occurred at each cicatrix, so that the pupil was reduced to a central narrow slit, which all but closed when the eye was exposed to a strong light. The effect of the operation was to restore excellent vision, and very much to improve the appearance

of the eye; the pupil being now nearly circular and *movable*. I should not have ventured to operate in this case unless I could have *exactly regulated the size of the pupil, and preserved the circular fibres of the iris*, and I feel satisfied that this twofold object could not have been attained in any other way than by *Iridesis*.

I have applied the same method in cases in which it has been desirable that the pupil should be brought close to the margin of the cornea. Instead of drawing out a portion of the iris, and either cutting it off or leaving it in the wound, according to the usual methods, I have tied it, in these cases, close to the margin of the wound in the cornea. This has the twofold advantage of keeping the edges of the artificial pupil secured and clearly defined, not only at the time of the operation, but permanently; and also of destroying the prolapsed portion of iris, and thereby preventing it from becoming a source of irritation.

I may just mention here that this method of operating has been adopted by my friend, Mr. Bowman, in some cases of conical cornea, for the purpose of *converting the pupil into a narrow slit*. It perfectly succeeded in effecting the desired object, and I think it may be safely affirmed that this could not have been efficiently performed in any other way.

The operation that I have endeavoured to describe in this paper has now been performed in twenty-seven cases. In no instance has it been productive of any serious amount of irritation, or failed to effect the desired object in a very perfect and efficient manner, and I venture to express my hope and belief that it will prove a useful addition to ophthalmic surgery.—*Ophthalmic Hospital Reports, and Journal of the Royal London Ophthalmic Hospital*, Oct. 1858.

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35. *Staphyloma of the Cornea.*—Mr. RICHARDSON exhibited to the Surgical Society of Ireland (Jan. 16, 1858), a specimen of staphyloma of the cornea, with the view of showing its minute structure.

"The designation staphyloma," he remarked, "has been indiscriminately given to a variety of prominences on the surface of the eyeball; but, as Dr. Wharton Jones remarks, the name staphyloma was originally given to that projection which 'occupies the place of the cornea, and is, according to the opinion of all writers' (these observations were made several years since, *Med. Gaz.*, 1837), 'the cornea itself degenerated and distended, together with the iris, which has become adherent to it throughout its whole extent; the anterior chamber being, of course, annihilated.' In order, says Mr. Lawrence, 'that the state of staphyloma should be produced, there must be severe inflammation of the eye, involving the entire cornea, and rendering its texture opaque; that inflammation must also have extended to the iris, and caused it to adhere to the cornea; and there must be increased secretion of the aqueous humour to push these unnaturally connected structures forwards.' Mr. Wardrop does not seem to have agreed to this explanation, for he says 'that staphyloma never occurs unless the cornea has been previously ulcerated, and unless the ulcer has penetrated into the cavity of the aqueous humour and destroyed the cornea as deep as the internal tunic.' It would have been nearer the truth, Mr. Jones observes, if he had said 'that staphyloma never occurs without more or less extensive destruction of the cornea.' I now come to Mr. Jones's mode of accounting for the disease; an explanation which, judging from a very careful examination of the preparation here this evening, seems to me most accurate. 'If,' he writes, 'in scrofulous, catarrhal, or catarrho-rheumatic ophthalmia, there be a penetrating ulcer of the cornea, the aqueous humour escapes, the iris falls forward into contact with the cornea, and a small part of it is, perhaps, prolapsed through the ulcerated opening. The progress of the ulceration being stopped by the yielding of the inflammation, the prolapsed portion of the iris, and the ulcerated part of the cornea, are involved in one cicatrice. The opening in the cornea being thus closed, the aqueous humour again collects, and the anterior chamber is restored, though somewhat diminished in consequence of the partial adhesion between the iris and cornea. There is no prominent distension of the front of the eye in this case, because, as the inflammation subsides, the small protruded portion of iris shrinks and flattens; but'—and here I beg the particular attention of the society—'if the destruction of the cornea has gone on further, either by ulceration or the giving

way of an onyx, and considerably more of the iris has protruded, the prolapsed portion of the iris does not shrink when the inflammation begins to abate, as in the former case, but remains, and forms a projection at one part of the cornea, generally the lower or lateral. This projection is at first merely a bag of the iris distended by the aqueous humour; but, by and by, its surface becomes covered by an opaque firm tissue, of the nature of the tissue of cicatrices, and this tissue is incorporated at the base of the tumour with the sound cornea.' This, as he further observes, 'is not a distension of the cornea itself, but a protruded portion of the iris covered by a *new tissue*, intending to supply the loss of the substance which the cornea has sustained. The mode of origin of a *total* staphyloma is essentially the same, but differs only in degree.' The whole or greater part of the cornea being destroyed, the iris falls forward, the pupil becomes closed, and, the aqueous humour being thus allowed to accumulate in the posterior chamber, the iris is kept distended in the form of a tumour on the front of the eye. Its surface generally gets covered with an opaque cicatrice-like tissue, or pseudo-cornea, which assumes a greater or less degree of thickness, and a total staphyloma is the result.' The tissue, then, according to this writer, is not degenerated and opaque cornea, but a *new tissue*, developed on the surface of the iris exposed by the destruction of the cornea itself. The more recent investigations of Mr. Bowman appear to have supported Mr. Jones's views on this question; and he, in addition, states 'that the new material becomes covered with an epithelium continuous with the conjunctival, just as an ulcer of the skin acquires an investment of the cuticle as it heals.'

"Having, as I have just mentioned, made a most careful examination of the staphyloma before us, I could not avoid the conclusion that it is altogether a *new structure*, and confirms the views of both Mr. Jones and Mr. Bowman. We have in it no trace of the true cornea; both elastic laminæ have likewise disappeared, and we have substituted a *fibro-areolated* tissue, covered by a thick layer of epithelium, composed of several strata of cells, resembling very much a section of epiphisary cartilage."—*Dublin Med. Press*, Jan. 27, 1858.

MIDWIFERY.

36. *Abolition of Craniotomy from Obstetric Practice.*—Dr. Tyler Smith read (Feb. 2, 1859) an interesting paper on this subject before the Obstetrical Society of London, a society very recently formed.

In this paper the author shows that craniotomy is resorted to in British practice about once in every 340 labours. The whole number of births in England and Wales exceeds 600,000 per annum; and if we apply the proportion of 1 in 340 to these figures, we get a total of about 1800 cases of craniotomy per annum. This is as though every year all the children born in London during rather more than one week were sacrificed; or as though all the children produced during the year in such a county as Westmoreland were born dead. The mortality to the mother from this operation is nearly 1 in 5, in British practice, which would give in England and Wales a maternal mortality of between 300 and 400 per annum. Craniotomy is performed about twice as often in British as in French practice, and four times as often in this country as it is in Germany. It is an obvious fact that every improvement which has ever been made in obstetrics has tended to restrict and diminish the cases and conditions in which the performance of craniotomy has been resorted to. It is the author's object to show that, with the proper and scientific use of all the means at our command, it may be laid down as a general rule, that craniotomy should not be performed in the case of a living fœtus after the period of viability has been reached. It is certain that, up to the present time, the measures which are the alternatives of craniotomy have never been carried out in practice to their full and legitimate extent. Turning was the first great obstetric operation which checked the voluntary destruction of the fœtus during labour. The objections

to turning which some obstetrists entertain depend on an almost superstitious fear of the uterus—a fear mainly owing to ignorance of the nature of the organ, and of the laws under which it acts. The dread of introducing the hand into the uterus has prevailed almost universally. But, apart from the danger of infection, the hand of the accoucheur, properly guided, can do no more harm in the uterus than any portion of the fœtus of equal bulk. Restrictions of the most absurd kind have been laid upon the operation, and it has come to be almost limited to arm presentations and cases of placenta prævia. On the continent, turning is the recognized practice in cases of difficulty, where the head is above the brim, beyond the reach of the forceps, when the os uteri is in such a state as to admit the hand, and when no serious distortion of the pelvis exists. The operation of turning in cases of moderate pelvic deformity was practised by Denman, but it was dealt with rather as an exception than a rule of practice until the matter was taken up by Professor Simpson. No unprejudiced person can read Dr. Simpson's papers on this subject without coming to the conclusion that turning may be performed in cases of moderate pelvic distortion at the full term, with comparative safety to the mother, and with a reasonable chance of safety to the child. It is also shown to be applicable to cases of greater deformity, in combination with the induction of premature delivery. Nothing has ever occurred in the history of turning which has so strongly tended to enlarge its usefulness as the introduction of anæsthesia into obstetric practice. Under chloroform we can turn with comparative ease in cases of excessive sensibility of the os uteri and vagina, in arm cases in which the waters have been long expelled, and the uterus has closed upon the fœtus with spasmodic force. It renders turning practicable in cases of convulsions or maniacal excitement, and in all instances it makes the uterus comparatively quiescent, and thus averts the dangers depending on contraction and resistance during the operation. Turning is performed nearly three times as often in France and Germany as it is in this country. After turning, the next great step in opposition to craniotomy was the discovery of the forceps. Before the time of Chamberlain, whenever turning was impracticable, there was no resource in cases of difficulty except in craniotomy. But it may fairly be questioned whether the whole powers of this instrument have ever been fairly brought out, especially in this, the country in which it was produced. If we examine our standard works, we find more pains taken to show when this instrument is not to be used than when it may be. The cases in which the forceps may be used are those of moderate disproportion or distortion, whether the arrest is at the brim, in the cavity, or at the outlet of the pelvis; cases of arrest from failure of the labour pains, without any morbid condition of the parturient canal; cases of convulsions in which the os uteri is dilated, and the head sufficiently low to be within reach of the instrument; cases of occipito-posterior presentation, not otherwise admitting of rectification, and face presentations; cases of accidental hemorrhage; and cases of rupture of the uterus, in which no great recession of the head has taken place. It should also be used at a comparatively early period in many of the cases which, if not assisted, run on to impaction from swelling of the fœtal head and tumefaction of the soft parts of the mother. The outlet and middle straits of the pelvis are the limits within which the short forceps should be used; at the brim the long forceps is the proper instrument. The forceps is used more than twice as often in France and Germany as it is in this country. The last, and it may truly be said the greatest, opponent of craniotomy is the induction of premature labour. The largest single source of craniotomy is deformity of the pelvis. Now, it may be asserted, without the possibility of contradiction, that in this great mass of cases, it would be right and practicable at once and forever to abolish craniotomy in the case of the living and viable fœtus. In all cases of known deformity, an examination should be made in the early or middle months of pregnancy, and the proper treatment of such cases should be the induction of abortion or of premature delivery. In cases of excessive distortion, where it would be altogether impossible for a viable fœtus to pass, abortion should be induced before the time of quickening. It would be quite impossible for intercourse and impregnation to take place in any case in which it would not also be possible to induce abortion with safety to the mother. In the very considerable number of cases

of moderate distortion in which the diminished capacity impedes delivery at the full term, but would allow of the passage of a child at the seventh or eighth month with a chance of living, the introduction of premature labour is the only justifiable practice. Besides the great operations of turning, the forceps, and the induction of premature labour, there are other means by which, in special cases, the necessity for craniotomy may be superseded. One of the most simple is the rectification of occipito-posterior presentations. When the occiput descends towards the sacrum in the third and fourth positions, instead of turning towards the right or left acetabula, great difficulty is produced, particularly in first labours, or when the head is large. Recorded cases of craniotomy show that the want of this rectification, which is generally possible with the hand, the lever, or the forceps, often leads to perforation. Cases of hydrocephalus in the fœtus are among the most difficult to deal with in an attempt to abolish craniotomy; but here we have the proposal of Dr. Simpson to tap the hydrocephalic head, and in this way reduce it so as to allow of delivery without the destruction of the fœtus. In actual occlusion or insuperable rigidity of the os uteri, incision is a safer and better practice than craniotomy. While it is the object of the present paper to advocate the abolition of craniotomy in the case of the living and viable fœtus, there is undoubtedly a class of cases in which perforation be practised beneficially, namely, in labours where the child has died during the course of parturition. No woman should be allowed to remain in difficult labour after the death of the child has been satisfactorily ascertained. Considering, then, the various means at our disposal in the way of preventing the necessity for craniotomy, the author unhesitatingly expresses his conviction that, as a rule of practice, craniotomy in the case of the living and viable fœtus should be abolished; and he believes that if all the resources of obstetrics in the way of prevention, management, and alternative treatment were properly wielded, the necessity for the operation would not arise.—*Medical Times and Gazette*, Feb. 12, 1859.

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37. *On the Indications for the Application of the Forceps.*—By Dr. Spiegelberg. Among the indications those derived from the insufficiency of pains are the most frequently assigned, and the most liable to give rise to errors in practice. The head remains at the floor of the pelvis, the pains, though more or less strong, being insufficient in the individual case to expel it beyond the external genitals. The patience of the accoucheur becoming exhausted (much sooner indeed than that of the woman), he has resort to instruments in order to terminate the labour. That such a procedure, common as it is, cannot be justified, scarcely requires to be said.

Much more frequent in occurrence than these examples of actual insufficiency of pain (which the author has always found at this stage of labour, especially in primiparæ, a very rare circumstance) are the cases in which the head, in spite of strong pains, remains at the floor of the pelvis, and although pressed down by every contraction towards the mouth of the vagina never distends the perineum. Well aware of the impropriety of too early or of useless operative interference, at the beginning of his practice, Dr. Spiegelberg contented himself in these cases with temporizing and pain-increasing measures, too often, however, with the result of still having to employ the forceps at last, as it seldom happened that the pains alone proved sufficient. Under these circumstances, a large proportion of the children were stillborn. Taught by this experience, he afterwards in such cases resorted to the forceps earlier, and was, as regards the children, much more fortunate. He was also exceedingly surprised at the ease with which the head, apparently so firmly fixed, was extracted. Traction was scarcely required at all, a suitable adjustment of the instrument and a few lateral pendulum-like movements commonly sufficing to effect the delivery. Sometimes all was completed with one hand, the other supporting the perineum. While in those cases in which the ergot had been used to effect the propulsion, retention of urine, erosions, and inflammation of the vagina or vulva were met with, no such occurrences followed the use of the forceps. The cause, which in these cases prevents good pains proving effective, and renders a forceps operation so easy of execution, is a purely mechanical one—arising from the too great flexure

of the head upon the chest. The face is turned towards the coccyx, the vertex rests on the perineum, and the occiput lies under the symphysis at the mouth of the vagina. The uterus, acting upon the trunk of the child, forces the occiput by means of the vertebral column, deeper every pain, pressing the chin more forcibly towards the chest. For the birth of the child, however, it is necessary that the neck should be stretched out so as to raise the head from the chest. The contractions of the perineal muscles, especially the levator ani, which, as well as those of the abdomen, are in a state of voluntary or of reflex activity, force the cranium still further upwards and backwards, *i. e.* towards the chest. The pains, however, increased in severity, prove useless, and the operation of the forceps is easily explained. When the blades are passed in at the sides of the head in the direction of the prolongation of the axis of the outlet of the pelvis, they remove the chin from the chest, stretch out the neck, and terminate the delivery at once. The head had already been forced through the pelvis, and the forceps has only to conduct it over the perineum. They do not act by traction, but as a lever or instrument for bettering the position. They must be passed up slowly, or the perineum may be injured. What is here stated has been but little observed, and as far as Dr. Spiegelberg is aware, Cazeaux is the only author who has clearly set it forth. If this excessive flexure of the head does not furnish the indication in nine out of ten forceps operations as stated by Cazeaux, at least it is the most frequent cause of their employment, and places this in the most favourable light. Just as weakness of pains is a rare, so is this condition a frequent indication. Dr. Speigelberg hopes that this communication may not be interpreted into a recommendation to have recourse hastily or uselessly to operations. But he adds that some of the worst consequences result from abstaining from operations when solid indications for interference present themselves; and that skill in employing his instruments is of even less importance to the obstetrician than the power of detecting the cases which justify his resorting to them.—*Med. Times and Gaz.*, March 5, 1859, from *Monatschrift für Geburtsk.*, Bd. xi.

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38. *Scarlatina after Parturition.*—Dr. McClintock read a paper before the Association of the Fellows and Licentiates of the King's and Queen's College of Physicians in Ireland (Feb. 2, 1859), on the occurrence of scarlatina within eight days after confinement. The mortality in such cases has been put down as two out of three, or over sixty-six per cent. Of twenty-eight patients treated in the Rotunda Hospital, seven died, or about twenty-five per cent. Dr. McClintock considered the advent of this exanthem supervening on delivery one of the most fatal complications of the puerperal state. The earlier the appearance of the rash, the more fatal; the same rule having applied to puerperal fever, whilst epidemic in this city, in 1854 and 1855. He referred to the peculiar acceleration of the pulse in these cases, to the eruption being occasionally tardy in evincing itself; and as regards the treatment, his experience leads him to attach great importance to the early exhibition of stimulants in these cases. —*Dublin Hospital Gazette*, Feb. 15, 1859.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

39. *On Arsenical Paper-Hangings. The mode in which they may produce noxious effects.* By Alfred S. Taylor, M. D.—A friend, whose library walls were covered with an arsenical-green paper, had suffered for some time from chronic inflammation of the eyes, especially affecting the conjunctivæ of the eyelids. On the discovery that arsenic was contained in the green pigment of this paper in rather large quantity, he caused it to be removed during the summer and replaced by another containing no arsenic. The inflammation from which he had suffered disappeared; but within the last few weeks it has returned. He informed me that he had been dusting some books in a bookcase belonging

to this room, and he imagined that the dust, which had accumulated for two or three years, had affected his eyes, and had caused a return of the inflammation. Some of the dust was carefully removed on Tuesday last from the tops of a few books by a feather, and submitted to a chemical analysis. The dust weighed one grain and a half: it had an olive-green colour, and under the microscope it presented the appearance of fibres, with numerous particles of various colours, chiefly of a grayish black. By Reinsch's process, a portion of the dust yielded a deposit of arsenic; and there was, therefore, clear evidence that some of the mineral arsenical pigment formerly on the walls, had found its way through the glass-doors of the bookcase, and had been deposited in the form of a fine dust on the tops of the books.

On Thursday last, after having made this chemical examination of the dust from a private dwelling, I procured from the shop of Messrs. Marratt and Short, Opticians, 63 King William Street, London-bridge, a quantity of dust for the purposes of analysis. The walls of this shop are covered with an unglazed arsenical paper; and, as I am informed, they have been so covered for a period of about three years. In collecting this dust from the tops of the cases containing the instruments, great care was taken not to touch the walls. The quantity thus collected for examination amounted to about four hundred and fifty grains. It was nearly black, and under the microscope appeared to consist of fibres and sooty particles. It was very light and flocculent. One hundred and fifty grains of this dust were examined by Reinsch's process; and enough metallic arsenic was obtained from it to coat about ten square inches of copper foil, in addition to a piece of copper gauze. From the latter deposit, by the application of heat, octahedral crystals of arsenic were readily obtained. The cases had not been dusted for a period of nine months.

The cases are secured by glass doors, and lined inside at the back with arsenical paper. Mr. Short removed, by a camel's hair pencil, a small quantity of dust which had accumulated on the thermometers and barometers, which are locked up within the closet. The quantity thus obtained from the projecting parts of a few instruments, amounted to eight-tenths of a grain, of which five-tenths were taken for examination. This half grain of dust sufficed to cover pretty thickly with metallic arsenic a square inch of copper gauze. A portion of this, when heated, yielded a large number of well-defined octahedral crystals of arsenious acid.

These facts lead to the inevitable inference that the air of a room, of which the walls are covered with an unglazed arsenical-green paper, is liable to be charged with the fine dust of the poisonous aceto-arsenite of copper. Those who inhabit these rooms are exposed to breathe the dust. The poison may thus find its way by the pulmonary membrane into the system, or it may affect the eyes, nose, and throat by local action. That so few cases of actual poisoning under these circumstances have occurred, is a fortunate circumstance; but only cases of serious mischief would be likely to attract attention. There may have been numerous instances of a disturbance of health, depending on this arsenical paper, which from absence of suspicion has been assigned to other causes. The degree of exposure, state of health, peculiar susceptibility, and the eliminating power of the system, may account for the comparative rareness of these cases. The mode in which the pigment is laid on the paper may be such as to prevent, in some instances, the fine particles of dust from escaping. The fact now demonstrated, that arsenical dust is, and must be substantially breathed by those who occupy rooms thus papered, explains the similarity of symptoms observed; justifies the statements made by Dr. Hinds, Dr. Halley, and others; and proves that those who have experimented on this subject with negative results, have not taken the right course to arrive at the truth. Their results have, to a certain extent, misled the public, by teaching them to rely on what is now proved to be a false security.

If, as a general rule, the quantity of arsenic which can penetrate the body from this source is small, it is still desirable that arsenic should not be breathed day by day in any proportion. The defenders of this noxious manufacture will hardly go the length of asserting that this arsenical-green, which is a potent poison in the stomach, can exert no injurious effect when taken into the lungs;

and yet, unless this assumption be made, the inevitable inference is, that these papers should not be used for covering the walls of our dwellings.—*Med. Times and Gaz.*, Jan. 1, 1859.

40. *Pharmacology and Toxicology of the Ranunculaceæ.* By Prof. CLARUS, of Leipzig.—Most of the Ranunculaceæ indigenous to Germany are distinguished by an acrid burning taste, and on this account, and from their injurious effects upon cattle when fresh, they are considered to be poisonous. The Ranunculus sceleratus, R. flammula, and R. acris, and the Pulsatilla pratensis and P. vulgaris, may be regarded as being endowed with the most acrid properties. The fact that the true Ranunculi lose their acrid taste by drying, while the Pulsatillas retain their active principles, leads to the belief that while the poisonous properties of the former are due to the presence of a volatile principle, dissipated by drying, those of Pulsatilla depend upon the presence of the same volatile principle, together with other acrid and narcotic substances. Dr. Clarus has ascertained that the narcotic principle of the Pulsatillas is *anemonin*, and the acrid principle is a resin with an acid reaction, while the acrid principle common to the Ranunculus sceleratus and Pulsatilla pratensis is a volatile oil.

From chemical experiments on the Ranunculaceæ and the administration of the products to the lower animals, Dr. Clarus has arrived at the following conclusions: 1. The Ranunculus sceleratus, and probably the rest of the Ranunculi, belong to the class of narcotico-acrids, inasmuch as they induce inflammation of the stomach and bowels, and irritation of the kidneys, and they diminish the number of the pulse and of the respirations. 2. The acrid principle of the Ranunculus sceleratus is a volatile oil, soluble in ether, of a very pungent taste and smell, which probably becomes decomposed, by keeping in close vessels, into inert anemonic acid and the narcotic principle, anemonin. 3. The narcotic principle of Ranunculus sceleratus is anemonin; it exists in this plant in much smaller proportion than in Pulsatilla pratensis, and hence the narcotic effects, such as diminution of the pulse and of the respirations, and palsy of the extremities, are less marked than in Pulsatilla, while the acrid effects, such as inflammation of the stomach and bowels and hyperæmia of the kidneys, preponderate. 4. The resin from the Ranunculus sceleratus is almost inert, and produces only a slight increase of diuresis. 5. Pulsatilla pratensis probably belongs to the class of narcotico-acrids, since it acts as an irritant to the skin, the gastro-intestinal tube, and the kidneys, and paralyzes the medulla oblongata, the spinal cord, and the sympathetic system. 6. The narcotic operation (stupor, palsy of the extremities, slow pulse and breathing) depends upon the anemonin, and the irritant effects on the skin, intestinal canal, and kidneys, depend upon two acrid principles, one of which is an acid, fixed resin, and the other is a volatile oil, analogous in every respect to oil of Ranunculus. 7. Like the oil of Ranunculus, the oil of Pulsatilla is decomposed in close vessels into anemonic acid and anemonin. The plant loses by drying that part of its efficacy which depends upon the presence of the volatile oil, but it retains its narcotic powers, which depend upon anemonin, and a part of its acridity due to the acid resin. 8. The freshly-pressed juice of the Pulsatilla and of the Ranunculus sceleratus combines all the narcotic and acrid powers of both plants. 9. The two plants now examined are very analogous to one another in respect to the quality of their active principles, since they both contain anemonin, volatile oil and a resin. In the Ranunculus sceleratus, the irritant principle prevails over the narcotic principle, but in Pulsatilla pratensis, the narcotic prevails over the irritant, and this difference is due to the varying proportion of anemonin in the two plants.—*Brit. and For. Med. Chirurg. Rev.*, Jan. 1859, from *Zeitsch. der Gesellschaft der Aerzte zu Wien*, Aug. 16, 1858.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Case of supposed Poisoning by Strychnia. By W. S. KING, M.D., Surgeon U. S. A.—On the 1st of January, 1859, at 5 P. M., I was sent for to see Thomas D——, aged 21 years, of good family and of active and energetic habits. I learned from his mother that he had left his home, perfectly well, about 2 P. M., and at 4 P. M. was found in a state of insensibility, in the erect position, holding on,[1] with the grip of a dead man, to a post in front of a Mexican house of a low character near the Military Plaza in San Antonio, Texas. He was carried into a house near by, a carriage was sent for, and he was brought home. I found him in bed, insensible; extremities rather cold; eyes half open, pupils dilated; breathing natural, without stertor, as if in a deep sleep; and jaws immovably fixed, as in tetanus. The patient could not be aroused, although pinched severely; and on the extremities being pricked with pins, manifested no sensibility whatever. Presuming, from the symptoms and from inquiries, that he had been drugged by strychnia in a drinking-store, an attempt was made to administer an emetic; but, owing to the obstinate closure of the jaws, which could not be overcome by any force used, and being requested by his mother to desist for fear of breaking his teeth, it was abandoned. An emetic was poured between the lips and teeth, but no deglutition was effected. Ordered the feet and legs to be put in a hot mustard-bath; afterwards sinapisms to ankles and epigastrium, and stimulating frictions, with ol. tiglii, along the spine and nervous centres. A stimulating enema was also directed. In addition to the symptoms already mentioned, there was also a constant twitching of the upper eyelids and jerking of the right arm. At 11 P. M. these last symptoms subsided; in other respects he was the same, except that he was more restless; had got up and walked firmly across the room, and sat down on a trunk. On being placed again on the bed, he tossed his limbs about, and persistently covered his head whenever a wet cloth was applied or it was touched by the hand. A few drops of diluted croton oil were dropped on his teeth, but none swallowed; jaws closed as firmly as ever; the pupils more dilated than when first seen. This was his condition when left at 12 o'clock at night. Called on the morning of the 2d, at 10 A. M. The eyes were now closed; jaws less rigid, but could not be opened; appeared to be in a deep sleep, and still unconscious; pulse and heat of skin natural. When the extremities were pinched, he withdrew them. Presuming that the danger was past, and all attempts to administer medicine by the mouth having again failed, he was left for the present. Returned at 4½ P. M., and found him better, and conscious. His father, who is a physician, informed me that half an hour previous—*exactly* twenty-four hours from the seizure—he had awoke, and had swallowed two tablespoonfuls of soup. He was out the following day, and stated that he had no recollection of any event of the 1st of January, subsequent to his riding out a little before 4 P. M., until he awoke, twenty hours after. He has never had a fit or sickness of any kind in his life before.

Dr. Kuppell, a very intelligent physician of San Antonio, who was called

[1] "*Como muerte*" was the language used by the Mexican woman who found him.

in by the family before sending for me, agrees with me in the opinion that the young man had drank liquor drugged by strychnia, though not in sufficient quantity to destroy life. I am unable to explain the symptoms satisfactorily on any other supposition.

FORT DUNCAN, Eagle Pass, Texas, Feb. 9, 1859.

Inhalation of Chloroform in Intermittents. By S. WHITEHORN, M. D., of Manhattan, K. T.—I am induced, by reading an article in the January number of the *American Journal* concerning the internal use of chloroform in intermittents, to record my brief experience in the same direction.

I have administered chloroform on several occasions to patients shaking violently with an ague chill. In every instance it has stopped the rigors after several inhalations. The feeling of oppression and all pains are simultaneously relieved. Should symptoms recur, I repeat the dose. I hope some of your subscribers, with a more extensive field than mine, will repeat the experiment, and report.

Extraordinary Fecundity. By WM. WOOD, M. D., of East Windsor Hill, Conn.—In the March number of the *Medical and Surgical Reporter* for 1856, I related the case of Mrs. R., who in five consecutive accouchments gave birth to five living children, weighing 62 lbs. 8 oz.—the largest 13 lbs. 10 oz., the smallest 11 lbs. 12 oz. Since then, a case has occurred in my practice which is perhaps still more remarkable. I was called to attend upon Mrs. S., Oct. 22, 1857, when she was delivered of three children, one male and two females. I was again called Oct. 13, 1858, when she was delivered of two male children, making five children in two confinements, in 11 months and 22 days. All would probably have been born alive had it not been for the officiousness of the nurse at the first labour, who said "she found something hanging and thought it ought to come away," and had actually pulled away 15 inches of the funis before my arrival.

All were born alive except this one. The twins weighed 13 lbs. 4 oz. (6 lbs. 13 oz. and 6 lbs. 7 oz.). The triplets, I regret to say, were not weighed. They were nearly as large as the twins; but calling them only 5 lbs. 8 oz. each, which I am confident is below their true weight, it gives about 30 lbs. of children in less than one year.

DOMESTIC SUMMARY.

Barton's Operation for Straightening the Knee-Joint by Excision of a wedge-shaped bit of Bone.—Dr. J. MASON WARREN recently read before the Boston Society for Medical Improvement a very interesting case of this.

The patient, a man 25 years of age, applied to Dr. W., in September, 1850, "on account of a great deformity of his limb, owing to an anchylosis of the knee-joint, the leg being bent at nearly a right angle with the thigh. He stated that his prospects had been destroyed and his life rendered wretched by his infirmity; and wished, if anything could be done for him short of extreme danger to his life, that it should be attempted. The history of the case, as given by him, was this. In November, 1841, he fell a distance of three feet, striking the knee. Three days after the fall, the knee began to swell and become painful. This went on for for four weeks, when it was punctured, and a pint of watery fluid escaped. It continued to discharge for fifteen months, during which time many small pieces of bone came away. The opening finally healed—leaving the joint and limb in its present distorted position. His hereditary tendencies were

scrofulous. In the erect position, resting upon the sound limb, the lame foot is seven and a half inches from the ground, but he can limp about with a high-heeled boot.

"I informed the patient that the only operation which suggested itself to me was Barton's operation, which had apparently been already described to him, and at once he requested to have it performed. I advised him to enter the hospital for the convenience of apparatus, which he did. Some of his friends attempted to deter him from running any risk, but he said he was determined either to undergo the operation, suggested by me, or to have the limb removed, as he could no longer bear the pain and mortification of his condition.

"On the second of October, the operation agreed upon was thus performed. A V-shaped incision was made through the skin just above the knee-joint, the base of the triangle, two inches wide, presenting outward, with the apex at the inner side of the limb. The flap was dissected up and the bone exposed, the other textures having become atrophied from disease. A wedge-shaped piece was sawn out of the femur, the incisions not being carried quite through, so as to avoid the artery. The remaining portion of bone was then broken; the flap was secured in its place, and the knee placed on a double inclined plane, and firmly fixed to it. There was no hemorrhage.

"On the following day, the patient said he had passed a restless night, but was free from pain. The limb was dressed on October 7th, and placed on a splint with a hinge and screw, so that it could be extended without any shock to the joint. By the 20th, the limb had been gradually brought to a straight position, and on the 29th the bones had united, and the wound was healed. Some time after this, he had a febrile attack, in the course of which the union became somewhat less firm, and threatened to dissolve; the system showing its scrofulous tendency. He gradually recovered, however, and left the hospital.

"About a year after his discharge, this gentleman presented himself to me, well. The limb was but very little shorter than the other, and with a pair of large trowsers the difference in the shape of the two limbs could scarcely be distinguished. He walked well with a cane, and the improvement between his present upright appearance in walking and his former painful method of locomotion would have almost prevented him from being recognized as the same individual.

"In a recent conversation with Dr. Barton—whose retirement from the profession in which he acquired so much honour is deeply to be regretted—he informed me that when he first began these operations, great danger was apprehended from the supposed interference with joints, or their vicinity. But he at once demonstrated what afterward seemed sufficiently evident, that the delicate structure of the joint had, in these cases, already been destroyed, and that the bones might as readily be interfered with at this point as in their continuity. In some cases greater symmetry may be gained by making the excision directly from the joint rather than above it, as there is then presented a much larger surface of bone. There are also other advantages. I have seen a patient thus operated upon, by Professor Mütter, with the most complete and gratifying success. Dr. Buck, of New York, has also done the operation successfully.—*Boston Med. and Surg. Journ.*, Dec. 23, 1858.

Diabetes.—Dr. Alonzo Clark called the attention of the New York Medical and Surgical Society to the history of two cases of diabetes, "in which he had resorted to a somewhat novel method of treatment with apparent benefit. The first case was that of a physician, aged 62, residing in the central part of the State of New York. He had naturally a robust constitution, and when in health had an average weight of 220 lbs. He stated that for the past forty years he had been actively engaged in the practice of his profession, and that he had enjoyed uninterrupted health until last spring, when his suspicions were excited by the occurrence of frequent micturition, accompanied with an increase in the amount of urine passed. He examined some of his urine, and detected the presence of sugar, fermentation taking place readily when the fluid was allowed to stand in a warm room. Its specific gravity was 103. Gradually he grew worse, muscular power being considerably diminished, and during the months of June

and July the urine still continued saccharine, and increased in quantity to a gallon per diem. At this time his strength was overtaxed in attending the practice of another physician. He continued at work, however, until about three weeks ago, when his failing health induced him to abandon his practice, and come to New York for advice. He arrived here three weeks since, and consulted Dr. Clark. The quantity of urine passed at this time was about the same, as also its specific gravity, and chemical examination revealed the presence of a considerable amount of sugar. The fluid responded to the fermentation test in twelve hours, tortulæ were formed in six hours, and the application of Trommer's test yielded a pretty abundant deposit of the red oxide of copper. Amongst the prominent symptoms were dryness of the mouth and skin, thirst, and constipation of the bowels, the feces being unnaturally hard. In the management of his case, Dr. Clark was led, from the experience of a previous one, to advise the use of bicarbonate of soda in doses of 11 grains, repeated as often as possible, provided the urine was not rendered alkaline, or the stomach nauseated. He also ordered counter-irritation to be established at the back of the neck, the idea of doing this having been suggested by the experiments of Bernard, who was able to cause a diabetic state of the urine by irritating the medulla oblongata. A mixed diet was allowed, vegetables in moderation, and hard biscuit being included, and the patient was cautioned respecting the use of water, which was to be taken only at meal-times, and at no time freely. With these directions, he went into the country on the 31st of August, and remained until Wednesday of that week, when he again visited Dr. Clark. During the interval of absence a vast improvement had taken place. He could now sleep eight hours, and on rising, pass less than a pint of urine, the quantity passed daily not exceeding three pints. He complained no longer of thirst, his lassitude was gone, his bowels were natural, and his weight had increased 7 lbs.; his urine, when examined, was found to have a specific gravity of 102.20, and when subjected to Trommer's test, gave the black instead of the red oxide of copper. The absence of sugar was further evidenced by the fact that the fluid was allowed to stand in a warm place over forty-eight hours without fermentation. Tortulæ were likewise absent, and in their place was a moderate number of crystals and oxalate of lime. The patient was sent home to continue the same plan of treatment.

"The *second case* was not as striking as the first, but yet was one of diabetes, treated in a similar way and with similar results. The patient was a gentleman who had once been under the care of Dr. Van Buren, and was seen by Dr. Clark two years ago. He then stated that he had suffered from the disease nine years previously, and that under the use of bicarbonate of soda, he had recovered and remained well for more than six years. At the time he applied to Dr. Clark, he was passing about a gallon of urine daily, which, on examination, was found to contain sugar; he also suffered the usual symptoms of the disease. He was put upon bicarb. soda, and a stimulating liniment, which, when rubbed upon the back of the neck, produced a sore that lasted for several weeks. During this period, he improved rapidly. Dr. Clark remarked that the results in these two cases were so satisfactory, that he thought himself warranted in recommending the same means of treatment for further trial.

"At a subsequent meeting of the society, Dr. Clark made a further report concerning the treatment of diabetes by blisters to the neck and administration of bicarbonate of soda. Since the time of the last notes, he had had three cases, in which to test it. The first passed a gallon of urine a day, and used soda for three weeks without benefit. The stomach was disordered by it, and the patient unable to continue the treatment.

"The second passed seven quarts a day of specific gravity 100.43—took soda for two and a half weeks, after which the amount went up to eight quarts, of specific gravity 100.4. This case left the hospital before treatment could be conducted to a close, or the remedy fairly tested.

"The third case, which passed ten pints per diem under blisters and soda, did not improve at first, but the dose of soda, being carried up to ℥iiss a day, the urine soon diminished in amount to six pints, of specific gravity of 100.28. That day only two quarts had been passed, and he seemed improving."—*New York Med. Journ.*, Jan., 1859.

Deformity after Fracture successfully treated.—Dr. Daniel Brainard states (*Chicago Med. Journ.*, Jan., 1859) that in 1853 he proposed a new method of treatment for irregularity of bones resulting from badly treated fractures. This method consisted in weakening the bone by subcutaneous perforation, and causing it to soften by the inflammation thus excited, and then straightening it by pressure. This mode of treatment was founded on experiments on animals, and it is only recently that he has been able to try it in the human subject.

This case was that of a stout boy, three years of age, who, when three months old, had fractured his left leg, to which injury little attention had been paid, and when seen by Dr. Brainard, the leg was three inches shorter than the other, and presented an angularity forwards a little below the middle.

"The child having been placed under the influence of chloroform, a perforator, one-fourth of an inch in breadth, was passed in two different directions through the tibia at the point of fracture, but a single puncture being made through the skin. After the perforator was withdrawn, a piece of adhesive plaster was placed upon the puncture through the skin, and a light bandage placed around the member. The leg lying at the time upon a firm bed, I attempted to rupture the callus with my hands, by throwing nearly the whole weight of my body upon it. It did not, however, yield in the slightest degree, and not thinking it safe to use more force I desisted, and ordered the bandages to be kept wet with cold water.

"The inflammation which followed this operation, and the efforts to straighten the member, was considerable, and an erysipelatous redness extended from the ankle to the knee, which lasted more than a week. There was no suppuration, and by rest and the use of evaporating lotions, this was dissipated; and at the end of ten days, viz: on the 25th May, 1858, another attempt to straighten the leg was made.

"Although hoping for a favourable change, I was somewhat surprised to find that a very moderate degree of force, applied by the hands, was sufficient to cause the callus to give. A carved wooden splint, well padded, with a foot piece, was now placed behind the leg, and secured to it by a roller drawn across the angular projection as tightly as could be borne. This giving rise to no pain, the bandage was reapplied, every three days at first, afterwards once a week, for four weeks, at the end of which time the leg was quite straight, except a slight overlapping of the fragments. During this part of the treatment, the boy walked about with the splint on his leg and suffered no pain. The parents were directed to press upon the projection daily with the hands."

Three months after the operation the splints were discontinued, the boy could walk, and Dr. Brainard considered the cure to be complete.

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Ovariotomy.—Dr. J. W. Hamilton, Professor of Surgery in Starling Medical College, relates (*Ohio Med. and Surg. Journ.*, Jan., 1859) two cases in which he performed this operation. In one the tumour was removed, and the patient recovered. In the second, after opening the abdomen, the adhesions between the abdominal walls and the tumour were found to be so firm that it was impossible to separate them. A trocar was introduced, but no fluid was discharged. The wound was closed, and the patient expired forty-two hours after the operation. Dr. Hamilton states the following facts in regard to the results of ovariotomy in Ohio:—

"In 1854, Dr. P. J. Buckner, now deceased, made a report to the State Society, that was supposed to contain all the cases operated on up to that time. It included eleven cases, of which six were successful and five fatal. Since that time, without having made this a special subject of attention, we have learned, through private channels mainly, of the following cases:—

"Professor Howard operated twice, found inseparable adhesions in one case, and abandoned the operation as impracticable, his patient dying within a few days. The other was cured and is still living. One esteemed friend operated twice, one patient dying, one recovering. Another undertook it twice, and was obliged to abandon it as often, one of the patients dying, one surviving. Within a few months, an eminent surgeon performed the operation, his patient dying within a few hours. Another made a moderate incision through the abdominal

parietes, his patient vomited, the tumour was thus forced into the external world, when ligating a small pedicle, the operation was completed. The patient recovered. We have been informed of three cases by another gentleman; two fatal, one successful.

"Of the operations performed since 1854, that we have learned of, accordingly, there are thirteen, five successful, and eight fatal. So that up to this time we have in Ohio twenty-four cases, eleven successful, and thirteen unsuccessful. This is probably about an average result."

Ovariotomy.—Dr. CHARLES A. POPE relates (*St. Louis Med. and Surg. Journ.*, Jan., 1859) four cases of diseased ovaria, in which he performed ovariotomy; two of which were successful, and two fatal. In one of the former, and one of the latter, the ecraseur was used to divide the pedicle.

Extirpation of the Eye.—Dr. C. R. AGNEW, Surgeon to the New York Eye Infirmary, in an interesting paper on this subject, extols the method of extirpation recommended by Mr. Critchett, which consists in incising the conjunctiva, and then dividing in succession the muscles near their insertions, as in the operation for strabismus; a method which, he thinks, is not generally understood. We can hardly suppose that any well read ophthalmic surgeon is unacquainted with this method, which certainly, in many cases, possesses great advantages over the old plan. The merit of suggesting it, however, belongs, he believes, not to Mr. Critchett, but to the late M. BONNET, of Lyons. (See LAWRENCE'S *Treatise on the Diseases of the Eye.* Philad. edition, 1854, p. 814.)

Apocynum Cannabinum as an Antiperiodic.—Dr. PETERFIELD TRENT, of Richmond, Va., having been disappointed in the treatment of intermittents with quinia, was induced, by the recommendation of Dr. R. S. Cauthorn, to try the apocynum cannabinum, and he reports (*Southern Med. and Surg. Journ.*, Jan. 1859) six cases successfully treated by this remedy. He gave it in pills, in the dose of five grains every two hours.

Chinoidine.—Dr. R. WYSONG, of Charlotte, N. C., uses chinoidine almost entirely instead of quinine, and has found the former, in a slightly increased dose, to be equally as efficient as the latter, and in some cases has proved to be even a more permanent antiperiodic. The quinoidine he has found, as have others, not to affect the head or disturb the stomach, as quinine sometimes does. He has administered the former in doses of a scruple every two hours, for ten hours, without its occasioning vomiting.—*Medical Journal of North Carolina*, Dec., 1858.

Iodine and Glycerine in Scrofulous Ozœna.—Ozæna is usually a very obstinate affection, and sometimes persists in spite of the usual treatment. Dr. H. F. CAMPBELL states (*Southern Med. and Surg. Journ.*, March, 1859) that he has recently cured, in less than a month, an obstinate case by the application, three or four times a day, of two grains of iodine dissolved in one ounce of glycerine, to the affected Schneiderian membrane; and by the internal administration, at the same time, of a tablespoonful, three times a day, in sweetened water, of a mixture consisting of iodide of potassium ʒij, Huxham's tincture of bark ℥viij.

Urea and Uric Acid in Urinary Secretions in Yellow Fever.—In our number for April, 1858 (p. 570), we noticed the statement by Dr. F. P. Porcher, that he had not been able, during the yellow fever of 1856, in Charleston, to detect uric acid or urea in the urine of those affected with the epidemic. In a paper in the *Charleston Med. Journ.*, for March, 1859, he states that during the prevalence of the disease last summer, having examined the specimens with more extended experience, and with special direction to this point, he found nitrate of urea, by evaporating the urine in a watch-glass over a spirit lamp, in some of the cases of fever. Crystals of uric acid he has not yet found, and he regards this as indicative of its diminished amount.

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GRADUATES OF JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA, MARCH, 1859.

At a Public Commencement, held on the 15th of March, 1859, the degree of DOCTOR OF MEDICINE was conferred on the following gentlemen by the Hon. EDWARD KING, LL. D., President of the Institution; after which, a Valedictory Address to the Graduates was delivered by Prof. BACHE.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Adams, Martin	Ohio.	Animal Heat.
Alexander, Eli M.	Mississippi.	Cholera Infantum.
Alexander, Samuel Lewis	Pennsylvania.	Drunkenness.
Almond, Andrew J. (M. D.)	Virginia.	Physiology of Digestion.
Anderson, Edwin P.	Tennessee.	Miasmata.
Anderson, Peter E.	Virginia.	Phthisis.
Bankhead, Robert A.	Mississippi.	Epidemic Colo-rectitis in Mississippi in 1857.
Barr, James M. (M. D.)	Virginia.	Asthma.
Baskin, Robert H.	Georgia.	Anatomy of the Eye.
Beadles, Percival	Virginia.	Pneumonia Typhoides.
Beman, Edward D.	Georgia.	Epilepsy.
Benson, Douglas B.	Virginia.	Concussion and Compression of the Brain.
Bibb, Alexander L.	Missouri.	Congestive Fever.
Bivins, Robert T.	Georgia.	Variola.
Blackwell, Thomas J.	North Carolina.	Puerperal Fever.
Blanton, William H.	Kentucky.	Gonorrhœa.
Bledsoe, Francis M.	Alabama.	The Tongue as a Symptom of Disease.
Blick, Joseph A.	Virginia.	Gonorrhœa.
Boggs, Charles D.	Virginia.	Epidemic Dysentery.
Boon, William C.	Missouri.	Heterologous Formations.
Bowers, Thomas C.	Canada West.	Inflammation.
Brewer, William T.	North Carolina.	Acute Inflammation.
Brinton, J. Bernard	Pennsylvania.	Aneurism.
Brown, Thomas H. B.	Virginia.	Diarrhœa.
Brown, W. C.	Arkansas.	Bilious Remittent Fever.
Brumby, G. McDuffie	Mississippi.	Inflammation.
Butler, Oliver H.	New York.	Phthisis Pulmonalis.
Campbell, William	Pennsylvania.	Treatment of Scarlet Fever.
Canfield, Augustus R.	Mississippi.	Fractures.
Carn, Lewis M.	Florida.	Pneumonia.
Caruthers, C. Kotzebue (M. D.)	Tennessee.	Pathology of Glycosuria.
Caswell, Edward T.	Rhode Island.	Urology.
Cauthorne, A. Hart	Virginia.	Circulation.
Cawood, J. C.	Tennessee.	Epidemic Typhoid Fever.
Chandler, Isaac L. (M. D.)	Georgia.	Dysentery.
Cheney, W. D.	Georgia.	Pneumonia.
Clements, William M.	Tennessee.	Dysentery.
Coard, William H.	Virginia.	Variola.
Comfort, Jonathan J. (M. D.)	Michigan.	Dislocation of the Hip-joint.
Cooper, Edward S.	Indiana.	Erysipelas.
Corbin, Philip S. P.	Virginia.	The Liver and Peyer's Glands.
Cotten, James F.	Georgia.	Pneumonia in Cobb County, Georgia.
Cox, George W.	Virginia.	Gonorrhœa.
Crawe, J. Mortimer	New York.	Dysentery.
Crawford, George G.	Georgia.	Dyspepsia.
Crawford, James J.	Virginia.	The Three Periods in Woman's Life.
Crews, O. L.	Alabama.	Alcohol.
Crigler, John L.	Mississippi.	Yellow Fever.
Cunningham, H. Clay (M. D.)	Kentucky.	Hysteria.
Davis, James F.	South Carolina.	The Cutis and its Functions.
Dean, James, Jr.	Georgia.	Bilious Remittent Fever.
Deloach, A. B.	Mississippi.	Aneurism.
Dickson, John H.	South Carolina	Inflammation.
Dorset, Thomas B., Jr.	Virginia.	Rheumatism.
Drennen, Horatio	South Carolina.	Puerperal Convulsions.
Dunglison, Thomas R.	Pennsylvania.	Rigor Mortis.
Dysart, Benjamin G.	Missouri.	Retroversion of the Uterus.
Dysart, William P.	Missouri.	Sunstroke.
Eads, Darwin D.	Kentucky.	Vital Phases.
Eckert, John N. (M. D.)	Pennsylvania.	Phthisis Pulmonalis.
Edmunds, Nicholas C.	Virginia.	Intermittent Fever.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Elder, William T.	Virginia.	Hæmaturia.
Fitzpatrick, William J.	Alabama.	Bilious Remittent Fever.
Flagg, Samuel D., Jr.	New York.	Abscesses of the Abdominal Viscera.
Flore, Frederick B.	Missouri.	Remittent Fever.
Flowers, John	Pennsylvania.	Organized Products of Disease.
Foley, Thomas W.	Louisiana.	Inflammation.
Foote, Frederick	Virginia.	Rheumatism.
Foote, George C.	Pennsylvania.	Healthy Human Urine.
Frame, Robert	Delaware.	Etiology of Puerperal Peritonitis.
Francis, G. M.	Texas.	Pneumonia.
Fulmore, Zachariah R.	South Carolina.	Icterus.
Fulton, James	Pennsylvania.	Puerperal Peritonitis.
Fussell, Benjamin L.	Pennsylvania.	Puerperal Peritonitis.
Gee, Edward C.	Virginia.	Dysentery.
Gilkey, John H.	North Carolina.	Dysentery.
Glass, W. L.	North Carolina.	Dyspepsia.
Glenn, James Mallory	Georgia.	Female Pelvis.
Glenn, James S.	Alabama.	Varicocele.
Glover, Charles P.	Tennessee.	Circulation of the Blood.
Goodwin, Thomas W.	Mississippi.	Remittent Fever.
Gordan, William H.	Pennsylvania.	Iron, chemically and therapeutically considered.
Grafton, Joseph D.	Arkansas.	Cirrhosis.
Graham, Joseph	North Carolina.	Acute Meningitis.
Greene, Marshall L.	Michigan.	Quiniæ Sulphas.
Gregory, Alfred B.	Georgia.	Intermittent Fever.
Gregory, Flavius J.	Virginia.	Mammary Abscess.
Gregory, Junius C.	Virginia.	Intermittent Fever.
Griffin, George G.	Georgia.	Hernia.
Grim, George W.	Pennsylvania.	Scutellaria Laterifolia.
Grooms, Stephen H.	Kentucky.	Scarlet Fever.
Gunn, Silas R.	Mississippi.	Duties of a Physician.
Gunn, William R.	Mississippi.	Femoral Hernia.
Hall, Joseph U.	California.	Climatology and Diseases of California.
Happersett, John C. G.	Pennsylvania.	Typhoid Fever.
Harper, Seborn A.	Mississippi.	Dysentery.
Harvey, Leon F.	New York.	Physical Exploration.
Hawthorn, Samuel W.	Virginia.	Abortion.
Henderson, Nat.	North Carolina.	Heterologous Formations.
Herrington, C. P.	Pennsylvania.	Cholera Infantum.
Hickman, Joseph T.	Virginia.	Simple Conjunctivitis.
Higgins, William F.	North Carolina.	Veratrum Viride.
Hillsman, John A.	Virginia.	Pneumonia.
Hines, Harvey L.	North Carolina.	Surgery.
Hines, William H.	Georgia.	Phthisis Pulmonalis.
Hite, Benjamin H.	Virginia.	Syphilis.
Hoard, Robert L.	Virginia.	Typhoid or Enteric Fever.
Hoffman, Robert H.	Virginia.	Digestion.
Holloway, Thomas P. (M. D.)	Kentucky.	Puerperal Fever.
Hopkins, James A.	North Carolina.	Veratrum Viride.
Hudgens, Thomas A.	South Carolina.	Menstruation.
Hudson, Gilbert L.	Georgia.	Mania a Potu.
Hull, John A.	Virginia.	Physiology of Digestion.
Hulse, John I. (M. D.)	Florida.	Yellow Fever.
Hunter, Horatio D.	Pennsylvania.	Intermittent Fever.
Jackson, John W.	North Carolina.	Bilious Remittent Fever.
Jackson, L. D.	Delaware.	Hæmoptysis.
Jackson, Thomas L.	Virginia.	Heterologous Formations.
Johnston, Thomas W.	North Carolina.	Pneumonia.
Jolly, M. A.	Alabama.	Honorable Medicine.
Jordan, Reuben Elm	Alabama.	Puerperal Uterine Hemorrhage.
Karsner, Charles	Pennsylvania.	Importance of a Medical Education
Keith, N. C.	Virginia.	Cholera Infantum.
Kelly, John B.	Alabama.	The Liver.
Kimbrough, Locket M.	Georgia.	Entero-mesenteric Fever.
King, George M.	Virginia.	The Pelvis.
Kirkwood, H.	Nova Scotia.	Dyspepsia.
Knorr, Matthias	Pennsylvania.	Inguinal Hernia.
Kuder, Joseph S.	Pennsylvania.	Prolapsus Uteri.
Lachenour, Henry D.	Pennsylvania.	Vaccination.
Leary, William B.	Virginia.	Mania a Potu.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Lide, William R.	Alabama.	Gastric Dyspepsia.
Lindsay, James E.	North Carolina.	Emphysema Vesiculare.
Linthicum, Rufus, Jr.	Kentucky.	Pneumonia.
Logan, John E.	North Carolina.	Cancer of the Stomach.
Longnecker, Benjamin F.	Illinois.	Heterologous Formations.
Magruder, George W.	Virginia.	Light, a Vital Stimulus.
Main, Elijah W.	New Jersey.	Chronic Hepatitis.
Maney, Samuel B.	Texas.	Jaundice.
Marbourg, M.	Pennsylvania.	Rubeola.
Marlow, Nicholas P.	Alabama.	A View of the Medical Profession.
Marshall, Samuel D.	Delaware.	Intermarriage.
Matlack, William H.	Pennsylvania.	Rubeola.
Maurer, Jacob S.	Pennsylvania.	Dysentery.
McCondichie, Wiley G.	Alabama.	Spermatorrhœa.
McCurdy, John M. (M. D.)	Ohio.	Pneumonia.
McEwen, Joseph W.	Pennsylvania.	Acute Rheumatism.
McFadyen, A. R.	North Carolina.	Dysentery.
McKinley, Charles A.	Georgia.	Gonorrhœa.
McLees, Joseph H.	South Carolina.	Hæmoptysis.
McMaster, James	Pennsylvania.	Scarlatina.
Mechling, John	Pennsylvania.	Diphtheritis.
Merritt, Daniel R., Jr.	Kentucky.	Cholera.
Miller, Thomas W.	Mississippi.	Remittent Fever.
Mitchell, William	Nova Scotia.	Erysipelas.
Mitchell, William G.	Kentucky.	Ovulation.
Monteith, William H.	Georgia.	Typhoid Fever.
Montfort, William J.	North Carolina.	Signs of Pregnancy.
Murfree, James B.	Tennessee.	Compound Fracture of the Thigh.
Murphy, Daniel	Pennsylvania.	Congestion.
Neal, Harrison	Pennsylvania.	Conservative Surgery.
Neel, James D.	South Carolina.	Cinchona.
Neff, Isaac P.	Pennsylvania.	Asphyxia.
Newcomer, David	Pennsylvania.	Podophyllum Peltatum.
Newell, Joseph B.	Georgia.	Fœtal Circulation.
Newell, William L.	New Jersey.	Necessity of Vigilance on the Part of the Physician.
Newton, Edwin D.	Georgia.	Phosphorus.
Nicholson, Malcolm J.	Georgia.	Chronic Dysentery.
Norris, J. W. Stump	Pennsylvania.	The Liver, its Structure and Functions.
Norwood, John	North Carolina.	Cystitis.
Nunnelee, Virgil T.	Georgia.	Alcohol, its Uses and Abuses.
Pancoast, George L. A.	Virginia.	Resections in Compound Fractures and Dislocations.
Parkes, A. H.	Tennessee.	Pertussis.
Parr, William P.	Indiana.	Influence of the Mind over the Body.
Peacock, John L. C.	Georgia.	Bilious Remittent Fever.
Perry, Turner Hunt	Alabama.	Acute Rheumatism.
Perry, Van Lear	Maryland.	Scarlatina.
Peyton, Edwin O.	Virginia.	Digestion.
Plimpton, Albert F.	Maine.	Delirium Tremens.
Potteiger, Jonathan B.	Pennsylvania.	Tonics.
Powell, William J.	Maryland.	Nicaragua.
Prentiss, John Hart	Maine.	Spermatorrhœa.
Prewitt, J. B.	Texas.	Pleuritis.
Purifoy, John H.	Alabama.	Aneurism.
Quinn, John P.	Dist. of Columbia.	Febris Intermittens.
Randolph, Lewis C.	Virginia.	Typhoid Fever.
Rea, Charles T. I.	Massachusetts.	Gonorrhœa.
Read, Clement H.	Virginia.	Inflammation.
Redd, John T.	Virginia.	Cholera Infantum.
Reddish, Thompson K.	Missouri.	Inflammation.
Reed, Thomas B.	Pennsylvania.	Fungi as a Cause of Fever, with a special notice of the Uredo Rubigo.
Richardson, David R.	Georgia.	Sterility.
Rigg, W. Cochrane	Kentucky.	Uterine Hemorrhage.
Roberts, Rufus A.	North Carolina.	Vis Medicatrix Naturæ.
Robertson, J. Royall	Virginia.	Hygiene.
Rodes, William R.	Missouri.	Physical Diagnosis.
Rogers, W. F.	Alabama.	Spermatorrhœa.
Rowe, Thomas	Virginia.	Anæsthesia.
Scott, Jesse F.	Kentucky.	Scarlatina.

NAME.	STATE OR COUNTRY.	SUBJECT OF THESIS.
Scott, Robert W.	Tennessee.	Fever.
Scruggs, Richard F.	Tennessee.	Inflammation.
Seip, William H.	Pennsylvania.	Entero-mesenteric Fever.
Shands, Thomas E.	Virginia.	Physiology of Digestion.
Sharp, William T.	Ohio.	Intermittent Fever.
Sheppard, James L.	North Carolina.	Pulvis Doveri.
Slaughter, Thomas G.	Alabama.	The Uterus.
Smith, A. Harvey	Canada West.	Water.
Smith, James	Pennsylvania.	Dysentery.
Snow, George W.	Massachusetts.	Primary Syphilis.
Somers, Job Braddock	New Jersey.	Atmospheric Air.
Spencer, R. M.	Virginia.	Dyspepsia.
Spicer, John Daniel	North Carolina.	Pregnancy, its Physiological and Pathological Phenomena.
Sprague, Albert G., Jr.	Rhode Island.	Constitutional Syphilis.
Stallings, C. H.	North Carolina.	Typhoid Fever.
Steele, A. J.	New York.	Fatty Degeneration of the Heart.
Stevenson, Joseph M.	Pennsylvania.	Encephaloid Cancer.
Stewart, David F.	Texas.	Obstetrical Duties in Natural Labor.
Stockdell, Hugh	Virginia.	Inflammation.
Sudler, Arthur E., Jr.	Maryland.	Malaria as a Cause of Disease.
Swayze, George B. H.	Pennsylvania.	Digitalis Purpurea.
Taylor, John H.	Maryland.	Malignant Remittent Fever.
Taylor, William H.	Kentucky.	Inflammation.
Thomas, Alfred N.	Mississippi.	Heterologous Formations.
Thrash, David E.	Alabama.	Enteric or Typhoid Fever.
Tracy, George P.	Pennsylvania.	Pneumonia.
Triplett, W. H.	Virginia.	Colchicum.
Vanhook, James D.	Kentucky.	Acute Pneumonia.
Waddill, John M.	Virginia.	Acute Pleurisy.
Walker, Charles W.	Virginia.	Oxygen.
Walker, George S.	Virginia.	Dysentery.
Walker, Walter	New York.	Cataract.
Walker, Z. J.	Virginia.	Pleuritis.
Walter, Barnet C.	Pennsylvania.	Variola.
Ward, Richard William	North Carolina.	Auscultation and Percussion.
Warren, A. L.	Alabama.	Catarrhal Fever.
Waugh, James R.	North Carolina.	Dysentery.
Welborn, William Pinckney	Indiana.	Milk Sickness.
Wells, Thomas	South Carolina.	Gonorrhœa.
West, Thomas S.	Mississippi.	Poisons.
Whitehead, Peter F.	Kentucky.	Cyanosis Neonatorum.
White, James A.	Tennessee.	Digestion.
White, Thomas C.	Mississippi.	Albuminuria.
Whitner, Hiram K.	Pennsylvania.	Noncontagiousness of Typhoid Fever.
Williams, John F.	Mississippi.	Chemical Affinity.
Williamson, John H.	North Carolina.	Veratrum Viride.
Williamson, Lea Z.	Mississippi.	True Aneurism.
Wilson, Adolphus J.	Alabama.	Bilious Remittent Fever.
Wilson, John B.	Maine.	Matter and Mind.
Wilson, L. James	Mississippi.	Pneumonia.
Winne, Charles K.	New York.	Control of Hemorrhage.
Wisely, Leonard A.	Missouri.	Intermittent Fever.
Wood, George J.	Ohio.	Cardiac Inflammation.
Young, Elisha	Alabama.	Traumatic Tetanus.
Young, Matthew H.	Kentucky.	The Blood.
Young, S. F.	Mississippi.	Gunshot Wounds.
Young, Thomas R.	Kentucky.	Intermittent Fever.

Of the above, there are from—

Virginia	47	Missouri	8	Florida	2
Pennsylvania	38	Maryland	4	Michigan	2
North Carolina	23	Ohio	4	Arkansas	2
Georgia	22	Texas	4	Canada West	2
Mississippi	18	Maine	3	Nova Scotia	2
Alabama	18	Indiana	3	Louisiana	1
Kentucky	15	New Jersey	3	Illinois	1
Tennessee	10	Delaware	3	District of Columbia	1
South Carolina	8	Rhode Island	2	California	1
New York	7	Massachusetts	2		256

GRADUATES OF THE UNIVERSITY OF PENNSYLVANIA, 1859.

At a Public Commencement, held March 17th, 1859, in the Musical Fund Hall, the Degree of Doctor of Medicine was conferred by HENRY VETHAKE, L. L. D., Provost, upon the following gentlemen; after which an Address was delivered by HENRY H. SMITH, M. D., Professor of Surgery.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Albright, John S.	Coopersburg,	Lehigh,	Pa.	Sleep, Psychologically and Pathologically considered.
Alston, Solomon W.	Arcola,	Warren,	N. C.	Diaphoretics.
Arjona, Joseph E.	Panama,		Panama.	Pneumonia.
Asay, J. Lambert	Philadelphia,		Pa.	Dentition.
Barnes, Benj. S.	Suggsville,	Clarke,	Ala.	The Ready Method in Asphyxia.
Bateman, Robert M.	Cedarville,	Cumberland,	N. J.	Morbi Cervicis Uteri.
Baylor, John C.	Norfolk,	Norfolk,	Va.	Endocarditis.
Beall, James W.	Hernando,	De Soto,	Miss.	Scarlatina.
Bergin, Thomas J.	Easton,	Northampton,	Pa.	Dysentery.
Black, Charles A.	Sackville,	Westmoreland,	N. B.	Nausea Marina.
Blake, Amaza	Lowell,	Washington,	Ohio.	Enteric Fever.
Bost, J. L.	Olive Branch,	Union,	N. C.	Enteric Fever.
Brincklé, Samuel C.	Philadelphia,		Pa.	The Treatment of Burns and Scalds.
Burg, Washington	Manor,	Lancaster,	Pa.	Typhus Fever.
Calhoun, J. Theodore	Rahway,	Middlesex,	N. J.	Railroad Surgery.
Campbell, Samuel A.	Daleville,	Lauderdale,	Miss.	Erysipelas.
Campbell, Samuel W.	Tamola,	Kemper,	Miss.	The Medical Man.
Cannady, Isaac G.	Tranquillity,	Granville,	N. C.	Amenorrhœa.
Carter, M. B.	Henrico,	Henrico,	Va.	Gonorrhœa.
Cavett, Thos. M.	Collinsburg,	Bossier Par.,	La.	Inguinal Hernia.
Childers, Jabes V.	Pulaski,	Giles,	Tenn.	Acute Dysentery.
Clardy, T. Fleming	Blandville,	Ballard,	Ky.	Acute Colo-Rectitis.
Clarkson, Henry M.	Columbia,	Richland Dist.,	S. C.	Life, Vegetable and Animal.
Clements, M. Joseph	Rockville,	Montgomery,	Md.	Scarlatina.
Coleman, Thos. P.	Concord,	Cabarrus,	N. C.	Acute Dysentery.
Coates, Isaac T.	Chester,	Delaware,	Pa.	Mind-Electricity; Matter as Cause, Medium, Effect.
Coates, Louis Montgomery	Philadelphia,		Pa.	
Cook, Thomas C.	Fairfield,	Pickens,	Ala.	Pernicious Fever.
Cotter, Wm. A.	Bullock's Creek,	York Dist.,	S. C.	Hygiene.
Cox, John	Philadelphia,		Pa.	Aphonia.
Craigen, Wm. J.	Washington City,		D. C.	The Menses—a Secretion or Hemorrhage.
Cropp, William W.	Conrad's Store,	Rockingham,	Va.	Enteric Fever.
Cruice, Robert B.	Philadelphia,		Pa.	Special and Surgical Anatomy of the Neck.
Davis, Edward B.	Horn Lake,	De Soto,	Miss.	Scarlatina.
Dick, Leonard W.	Sumter,	Sumter Dist.,	S. C.	Indications of the Tongue in Disease.
Dunlap, R. S.	Clinton,	Laurens Dist.,	S. C.	Opium.
Edwards, Thos. H.	Newport,	Waukulla,	Fla.	Luxations.
Egle, William H.	Harrisburg,	Dauphin,	Pa.	Recent Contributions to Therapeutics and Pharmacy.
Elliott, Samuel A.	Meadville,	Crawford,	Pa.	Diagnosis.
Faison, Thomas I.	La Grange,	Fayette,	Tenn.	Intermittent Fever.
Flowers, Samuel B.	Mt. Olive,	Wayne,	N. C.	Oleum Terebinthinæ.
Frazee, John M.	Germantown,	Mason,	Ky.	Fever.
Freeman, Robert J.	Norfolk,	Norfolk,	Va.	Acute Pericarditis.
Fryer, Blencowe E.	Germantown,	Philadelphia,	Pa.	The Constituents of the Blood.
Garretson, James E.	Philadelphia,		Pa.	Odontalgia.
Goodman, H. Earnest	Germantown,	Philadelphia,	Pa.	Necrosis.
Gordon, DeWitt C.	Pulaski,	Giles,	Tenn.	Dysentery.
Gregory, G. Charles S.	Enfield,	Halifax,	N. C.	Abortion.
Grigsby, A. S., jr.	Centreville,	Fairfax,	Va.	Hernia.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Harrell, Oscar F.	Selma,	Dallas,	Ala.	Cinires Vitis Vini-feræ as a Remedy.
Harris, A. S.	Louisburg,	Franklin,	N. C.	Enteric Fever.
Higdon, John M.	Holly Springs,	Marshall,	Miss.	Digestion.
Hobbs, Isaac M.	Clinton,	Sampson,	N. C.	Fever.
Holt, Jacob F.	Greenfield,	Hillsboro',	N. H.	Pulmonary Tubercle.
Hooff, James H.	Point Pleasant,	Mason,	Va.	Tetanus.
Hostetter, John	St. Catharine's,	Lincoln,	Can. West.	Fibrin of Blood physiologically considered.
Jacobs, John C.	Jackson,	Northampton,	N. C.	Quinine in Typhus Fever.
James, Henry C.	Melburn,	Ballard,	Ky.	Intermittent Fever in Western Kentucky.
Kaufman, John F.	Philadelphia,		Pa.	Glycerina.
Kepler, Samuel, jr.	Baltimore,		Md.	Influence of the Mind on Body affecting Health and Disease.
Kirkland, John R.	Warsaw,	Sumter,	Ala.	Pneumonia.
Klapp, Henry Milnor	Philadelphia,		Pa.	Delirium Tremens.
Koller, Noah	Bloomville,	Seneca,	Ohio.	The Circulation.
Laurence, A. A.	Statesville,	Iredell,	N. C.	Liberal Intellectual Culture indispensable to the successful Study of Medicine.
Lee, Chas. Carroll	Petersville,	Frederick,	Md.	Correlation of Physical and Organic Forces.
Leedom, J. Moore	Germantown,	Philadelphia,	Pa.	Lithiasis.
Leidy, Philip, jr.	Philadelphia,		Pa.	Dentition.
Leinbach, A. N.	Salem,	Forsythe,	N. C.	Scrofulous Diseases of Joints in Children.
Leiper, A. K.	Hookstown,	Beaver,	Pa.	Pneumonia.
Lewis, David Thacker	Philadelphia,		Pa.	
Martin, Dewees J.	Allentown,	Lehigh,	Pa.	Pharmacy.
Martindale, Jos. C.	Byberry,	Philadelphia,	Pa.	Water—its Uses and Effects.
Maxwell, J. Chappell	Chappell's Depot,	Newberry Dist.,	S. C.	Uric Acid.
Mayes, James M.	Boligee,	Green,	Ala.	Enteric Fever.
Mayo, Peyton H.	Tawboro',	Edgecomb,	N. C.	Pernicious Fever.
McCauley, James D.	Blue Ball,	Cecil,	Md.	The Duties of a Physician.
McClanahan, S. R.	Magnolia,	Anderson,	Texas.	Differential Diagnosis of Enteric Fever.
Means, T. Sumter	Glenn's Springs,	Spartanburg Dist.,	S. C.	The Duties of the Physician.
Means, Theophilus H.	Charlotte,	Mecklenburg,	N. C.	Erysipelas.
Mears, J. Howell	Branchtown,	Philadelphia,	Pa.	Disease.
Melvin, Walter	Chestertown,	Kent,	Md.	Thesis Writing.
Micks, Thomas R.	Clinton,	Sampson,	N. C.	Rubeola.
Moore, Joseph	Amherst,	Cumberland,	No. Scotia,	Dyspepsia.
Morton, Samuel	Clinton,	Green,	Ala.	Gastritis.
Moseley, George W.	Clinton,	Sampson,	N. C.	Sporadic Cholera.
Newell, William H.	Frederick,	Frederick,	Md.	Professionis Medicæ Dignitas.
Newsome, James E.	Winton,	Hertford,	N. C.	Puerperal Convulsions.
Oliver, George P.	Philadelphia,		Pa.	Puerperal Convulsions.
Palmer, Horace	Sumerville,	Fayette,	Tenn.	Hepatitis.
Pancoast, D. Parrish	Philadelphia,		Pa.	Ætiologia Æsthetica.
Perry, Alexander	Portsmouth,	Norfolk,	Va.	Remittent Fever.
Perry, S. Williams	Warrenton,	Warren,	N. C.	The Therapeutical Application of Oleum Terebinthinæ.
Pool, John H.	Elizabeth City,	Pasquotank,	N. C.	Influenza.
Porter, Felix F.	Manlyville,	Henry,	Tenn.	Pathology and Diagnosis of Fever.
Ramsay, Junius N.	Jackson,	Northampton,	N. C.	Intermittent Fever.
Reily, James R.	Washington City,		D. C.	Acute Colo-Rectitis.
Riddick, Joseph H.	Hertford,	Perquimans,	N. C.	Malarial Pneumonia.

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Richmond, Leonidas	Holly Springs,	Marshall,	Miss.	Sulphate of Quinia.
Riggs, Benj. H.	Selma,	Dallas,	Ala.	The Microcosm.
Robinson, William T.	Philadelphia,		Pa.	Audition.
Rodenstein, Louis A.	Dallas,	Dallas,	Texas.	Yellow Fever.
Rose, Joseph G.	Bentonsville,	Johnston,	N. C.	Cinchona.
Ross, G. A.	Yorkville,	York Dist.,	S. C.	Inflammation.
Rountree, F. M.	Pleasant Mount,	Pitt,	N. C.	Therapeutical Applications of Sulphate of Quinia.
Rowland, Melson L.	Philadelphia,		Pa.	Transfusion of Blood.
Sadler, Wm. King	Oxford,	Butler,	Ohio,	Science versus Empiricism—a Poem.
Schaffer, Charles	Philadelphia,		Pa.	Experiments on Heart's Action.
Schmid, H. Ernst	White Post,	Clarke,	Va.	Disease a Product of Cell Action.
Searle, Wm. S.	Utica,	Oneida,	N. Y.	A review of Draper's Physiology.
Shannon, J. Clement	Elizabeth City,	Pasquotank,	N. C.	Acute Dysentery.
Simpson, Doctor J.	Cross Hill,	Laurence Dis.	S. C.	De Anatomiâ et Physiologiâ Uteri.
Smith, Henning M.	Franklin,	Southampton,	Va.	Pernicious Fever.
Smith, James M.	Vicksburg,	Warren,	Miss.	Intermittent Fever.
Smith, Philip W.	Georgetown,	Sussex,	Del.	Typhus Fever.
Stuart, W. C.	Olive Branch,	De Soto,	Miss.	Calomel.
Sutphen, John C.	Lesser X Roads,	Somerset,	N. J.	Hydrargyri Chloridum mite.
Taylor, William A.	Pittsboro',	Chatham,	N. C.	Scarlatina.
Thomas, Benj. H.	Ringgold,	Montgomery,	Tenn.	Chronic Peritonitis.
Thomas, Columbus A.	Cedar Rock,	Franklin,	N. C.	Pernicious Fever.
Thompson, Jacob A.	Leasburg,	Caswell,	N. C.	Epidemic Erysipelas.
Thompson, Joseph W.	Lovelaceville,	Ballard,	Ky.	Neuralgia.
Thompson, V. Oscar	Henderson,	Warren,	N. C.	Papaver Somniferum—liquid preparations, Morphia and Salts.
Thomson, Thomas S.	Fort Motte,	St. Matthew's,	S. C.	Typhoid Pneumonia.
Thorp, Henry R.	Brownsville,	Haywood,	Tenn.	Enteric Fever.
Tilton, Henry R.	Barnegat,	Ocean,	N. J.	Emetics and their Mode of Action.
Tomlinson, Thomas H.	Roadstown,	Cumberland,	N. J.	Infantile Hygiene.
Tremaine, William S.	Halifax,		Nova Scotia,	Woman—Physically and Mentally.
Usher, Francis M.	Newstead,	Christian,	Ky.	Phthisis Pulmonalis.
Usher, James H., Jr.	Newstead,	Christian,	Ky.	Qualities and Acquirements of a Physician.
Wallace, Alfred	Columbia,	Richland Dis.	S. C.	Coup-de-Soleil.
Wedgeworth, Ste'n B.	Eutaw,	Green,	Ala.	Pneumonia.
Welch, William M.	Bethlehem,	Hunterdon,	N. J.	The Germ as an essential Condition of Life.
Whiting, John C.	Mobile,	Mobile,	Ala.	Yellow Fever.
Wilkerson, Benj. C.	Blue Wing,	Person,	N. C.	Syphilis.
Wilkerson, Thomas B.	Buchanan,	Granville,	N. C.	Enteric Fever.
Williams, Rush F.	Farmington,	Davie,	N. C.	Medical Topography and Enteric Fever.
Woodhull, Alfred A.	Princeton,	Mercer,	N. J.	The Pilgrims' Progress.
Woolverton, James	Trenton,	Mercer,	N. J.	Diagnosis.
Woolverton, Joseph W.	Trenton,	Mercer,	N. J.	Bloodletting and its Substitutes.
Wortabet, Gregory M.	Beyrout,		Syria.	Syria—its Diseases and Drugs.

At a public Commencement, held July, 1858, the Degree of Doctor of Medicine was conferred upon—

NAME.	TOWN OR P. O.	COUNTY.	STATE.	SUBJECT OF THESIS.
Carman, Benj. R.	Philadelphia,		Pa.	Miasmata.
Coggeshall, George R.	Darlington C. H.	Darlington,	S. C.	Typhus Fever.
Martin, Chesley	Pittsylvania C. H.	Pittsylvania,	Va.	Medicine.
Shields, J. Hayes	Coatesville,	Chester,	Pa.	Scarlatina.

Date Due